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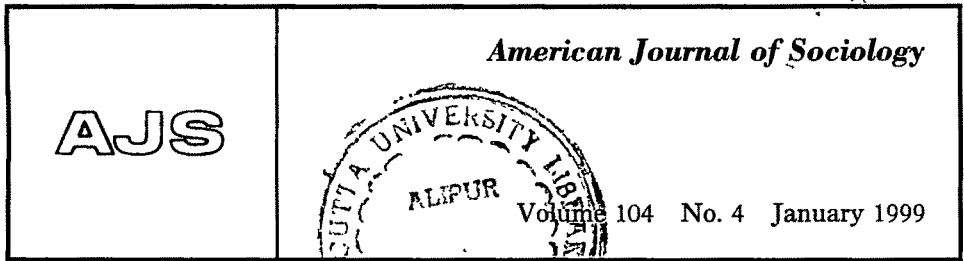
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IN THIS ISSUE

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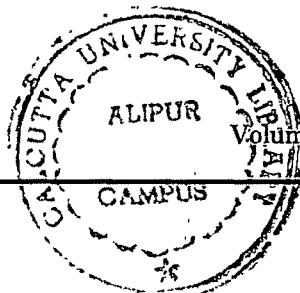
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W. L. GOLDFRANK

IN THIS ISSUE

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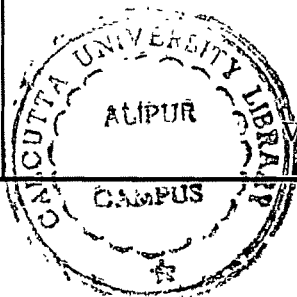
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(Rev. 3/98)

Prison Riots as Microrevolutions: An Extension of State-Centered Theories of Revolution¹

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Bert Useem
University of New Mexico

Prisons have long been used as a testing ground for social theory. This article explores the applicability of state-centered theories of revolution to the phenomena of prison riots. Prison riots are found to have numerous features in common with revolutions, including prior administrative crises, elite (guard) alienation and divisions, and a widespread popular (prisoner) sense of injustice and grievances regarding (prison) administration actions (not just toward imprisonment per se). The state-centered theory provides a better "fit" to prison riots than current functionalist, rising expectation, or management theories.

Social theory today is in a parlous state. This is in large part because of the devilish micro-macro problem; theories of large-scale social change and theories of individual behavior seem to fail altogether to connect (Collins 1992; Alexander et al. 1987). Continental theorists have tried to overcome the micro-macro disjunction with concepts such as "habitus" and "structuration" (Bourdieu 1990; Giddens 1987), and rational choice theorists have claimed that methodological individualism offers a golden road to constructing broader social relationships (Coleman 1990; Kiser and Hechter 1991). However, their efforts mainly show how individual behavior can give rise to complex social structures. They generally fail to provide

¹ The authors wish to thank Ryken Grattet, audiences at the University of Colorado, Boulder, the University of New Mexico, New York University, and the Center for Advanced Study in the Behavioral Sciences for helpful comments. This work was supported by grants from the Institute for Humane Studies and a University of California, Davis, faculty research grant. Direct correspondence to Jack Goldstone, Department of Sociology, University of California, Davis, California 95616.

empirically verifiable propositions regarding the dynamics of large-scale structures (Craib 1992).

This article takes a different tack at extending theory to different levels of social organization. Our argument (Goldstone 1991) is that social structures can be thought of as having "fractal" characteristics—that is, the principles of social organization repeat themselves on different *scales*. For example, the principles that underlie social cohesion or instability on a national scale should also obtain at the level of a region, or a municipality, or even formal or informal organizations. This article explores whether the social dynamics of U.S. prisons since World War II can be understood using the same theories of instability that state theorists have applied to nations (Skocpol 1979; Goldstone 1991).

Most efforts to generalize theories of revolution have assumed that the only unit of analysis that could be used for generalization is the society as a whole (Farhi 1990; Goodwin and Skocpol 1989; Wickham-Crowley 1992; MacHardy 1992; Dunning 1997). Yet by testing whether the state-centered theory of revolutions can be supported by studies of prison riots, we can examine whether theories that were developed to account for the social dynamics of complex structures with many millions of individuals can also help explain events in structures of a much, much smaller size, involving only several hundred individuals. Such a generalization would be a far more powerful extension of this theory than simply changing the list of states for which theories of revolution appear valid. It would also demonstrate the value of a fractal approach to overcoming the micro-macro divide.

There is a certain irony to this empirical application. For several decades, theorists of revolution and social movements have sought to distance themselves from characterizations of social protest as irrational outbursts by criminal or deviant individuals and groups (Tilly 1978; McCarthy and Zald 1979). They have pointed out that, even though collective action often involves violence, revolutionaries and social protestors are generally *not* criminals and that their actions are explicable through models based on analyses of social change, state power, and protestors' grievances, ideologies, and opportunities for action (Rudé 1964; Tarrow 1994). In short, if there is any collective action event that is generally held by current theory *not* to be a suitable case for broader theories of revolution and social protest, it would be outbursts of actions against clearly legitimate authority by convicted felons—prison riots.

Yet, as we shall demonstrate, the principles underlying collective behavior against authorities appear to be fundamentally the same whether one is examining revolution against monarchies and empires or riots against prison authorities. Thus, the irony arises that a theory of revolution developed out of a tradition that tried as hard as possible to disengage

from the kind of event called to mind by "prison riot" does in fact explain precisely those events.

PRISONS, SOCIETY, AND SOCIAL THEORY

Prisons have long been used as a laboratory to develop and test social theory (Beaumont and Tocqueville 1833; Sykes 1958; Goffman 1968; Foucault 1979; Garland 1990). Foucault (1979), for example, argued that the essence of prison—the use of power to discipline and punish nonconformist behavior—becomes the essence of all social life. Prison riots are but rebellions against this power. Gresham Sykes (1958), writing in the heyday of functionalism, suggested that prisons were functioning societies that, although they were "societies of captives," needed the same perquisites for functioning as any other societies. Prison riots were evidence of a dysfunctional or pathological relationship between the inmates and the prison authorities. In particular, Sykes argued that wardens who "crack down" on inmate privileges (rights to property, recreation time, social interaction) that sustain a viable inmate society will likely reap rebellion in response. Erving Goffman (1968) argued that prisons were a special kind of "total institution," similar to military bases or insane asylums in the degree of control they exercised over inmates' lives.

Yet another viewpoint, now overlooked but worth reviving, was expressed by Thomas Mathiesen (1965), who argued that prisons are similar to traditional patriarchal or monarchical regimes. He pointed out that wardens exercise a mixture of personal and bureaucratic rule and decree many of the practices of their institution, that they control armed force to maintain order, and that a well-articulated hierarchy—from prisoners to guards to higher officers on up to the warden—determines status and power.

We believe we can take Mathiesen's viewpoint even further: The hierarchical structure of prisons is quite similar to that which prevailed in early modern monarchical or imperial regimes. Prisoners occupy positions much like serfs or bound peasants. They have their own "space" but are subject to much oversight and control from the central authorities, whom they must obey or face punishment consisting of loss of property or space (e.g., relocation to punishment units). They have a routine of eating and working that, if they follow it and respect the authorities, they can generally maintain. However, they have few or no options for what Albert Hirschman (1970) has called "voice" or "exit" and thus little control over either the conduct of the authorities or the basic conditions of their life.

The correctional officers and prison's executive staff are much like the bureaucratic elites in an absolutist state. They come and go freely, they have considerable authority over the prisoners, but they also are strictly

subordinate to the central authorities and must perform their duties in the bureaucratic framework established by the prison administration.

Finally, the wardens face pressures similar to those faced by the heads of early modern states, particularly what Skocpol (1979) has called the Janus-like situation of the state, facing both internal and external demands at the same time. State rulers need to meet internal demands to maintain the loyalty of their bureaucratic elites and order among the population; yet state leaders' performance of these tasks is complicated by external demands, for other states can create military and economic pressures that strain state resources and make it difficult to meet internal demands. In similar fashion, wardens need to maintain the loyalty of their officers and order among the prisoners; yet they too face external pressures from the broader society. Although these are not military pressures, they can be no less imposing.

State and national legislatures, courts, and executives can mandate new rules for prison administration, increase prisoner populations, and demand changes in prison conditions, all while restricting or even reducing prison budgets. Political pressures and conflicts outside the prison can lead to the dismissal of wardens, support for prisoner demands, or insistence that the prison pursue certain outcomes, such as the integration of facilities or the rehabilitation of inmates. These external pressures can make it difficult or impossible for wardens to meet the internal demands of prison administration.

Given the great similarity between the sociopolitical structures of prisons and of early modern monarchical/imperial states, it seems quite reasonable to explore whether the state-centered theory of revolutions—developed to account for instability in this kind of state—can also be applied to prison riots. If so, it may both demonstrate an unusually “deep” generalization of these theories to phenomena on an entirely different social scale and shed light on the origins of the important social problem of prison disorders.

PRISON RIOTS AND REVOLUTIONS

Before moving to examine the applicability of theories of revolution to prison riots, let us first consider some of the broad similarities between prison riots and revolutions. First, there is the puzzle of the occurrence, indeed the *frequent* occurrence, of both prison riots and revolutions. It has long been a puzzle, given the overwhelming power of the state and the extremely limited resources and freedom exercised by peasants, why peasant rebellions and revolutions should occur at all. Yet revolutions, while certainly not part of the normal state of political affairs, are frequent

enough that they seem to be an intrinsic risk for state authorities. Over the past 500 years, there are hardly any major states that have not been repeatedly wracked by revolutions: Charles Tilly (1993) located 252 revolutions and revolutionary situations from 1492 to 1992 for six major regions of Europe (excluding Germany and Italy). One can question the broadness of Tilly's definition of a revolutionary situation and contend that truly major social revolutions that deeply and permanently transform societies are rare. Still, revolts against political authority are evidently quite common. Marc Bloch ([1931] 1952, p. 175) once wrote, "Peasant revolts appear to be an inseparable part of the seignorial regime, much as strikes are part of the modern capitalist enterprise" (our translation). It is interesting that one could add: or as prison riots are part of prisons.

Although one would think that the extremely close control and supervision of prisoners, their nearly zero resources, and the overwhelming power and authority of the prison guards and authorities, backed up by the heavily armed forces of state troopers, would virtually preclude any large-scale insurrection by prisoners, it is a remarkable fact that prison riots are fairly frequent occurrences. Since the spread of the modern penitentiary system in the early 20th century, episodes in which the prison authorities lose control of a significant amount of the "territory" and subject population of a prison (the analogue to Tilly's "revolutionary situation") have occurred in all nations with prisons, often in the tens or dozens per year (Adams 1994, pp. 12–13). Montgomery and Crews (1998) have located more than 1,300 riots in U.S. prisons between 1950 and 1995. Here too, one could challenge the broadness of their definition of riot, but clearly prison riots are part of prison history, just as revolutions are part of political history.

A second striking parallel is that prison riots, like revolutions, tend to occur in *waves* (Goldstone 1991; Katz 1997). Particularly notable waves in the United States occurred in 1928–29, in 1951–55, in 1968–1973, in 1980–82, and, most recently, in 1986. Despite their evident importance, almost no analytic attention has been given to the causes of waves of prison riots. Theories that focus on the actions of particular wardens, whether on their "cracking down" or "loosening up," clearly give no answer to why prison riots should strike a variety of different prisons, in different states, in clusters, at particular times.

A third empirical similarity is that prison riots, like revolutions, are quite varied in form and process. In both cases, the event can be largely spontaneous or the result of planning and leadership by a vanguard group. Among both prison riots and revolutions, some are extremely violent, while in others violence is moderate or almost absent. It is worth noting

that in both riots and revolutions, most violence is *not* perpetrated by revolutionaries or rioters against authorities: the highest tolls of death and violence occur either when authorities use force to suppress a revolution or to retake a prison (e.g., as at Attica in 1971) or when revolutionaries or prisoners have fully taken control and take violent action against other groups (e.g., reigns of terror in revolutions) or against other prisoners (e.g., as at New Mexico in 1980). Moreover, both revolutions and prison riots are—like all historical events—subject to the role of contingent events that can shape the outbreak, duration, and scope of the event.

And just as the duration and outcome of revolutions vary widely, so does that of prison riots. Of course, most revolutionary situations and prison disorders result in only a temporary breakdown of order. But just as a few revolutionary regimes succeed in capturing power for years before being overturned (e.g., the French Revolution of 1789–1815 and the English Revolution of 1640–60 both lasted decades before counterrevolutionary forces restored something much like the old regime), so a few prison riots succeed in maintaining control of large portions of prisons for days or even weeks. There is, of course, no analogue in the prison riot to something like Russia's October Revolution or the Chinese Communist Revolution, both cases in which the revolutionaries took permanent control of the regime.

However, if one thinks of most revolutionary situations—such as the Revolutions of 1848, or the Paris Commune, or the German Peasant Rising of 1525, or even the English and French revolutions—as ones in which the revolutionaries take only temporary control of significant territory or population before counterrevolutionary order is restored, then prison riots have a similarly varied range of time scales. The main difference is, as with the number of individuals, one of scale: whereas the normal time line of revolutions and rebellions runs from days and weeks to years and occasionally decades, the normal time line of prison riots runs from hours to days and occasionally to weeks. Again, we see the fractal phenomena of similar structure and dynamics of an event on widely different, in this case temporal, scales.

It is also important to realize that many prison riots *do* lead to far-reaching change, even a restructuring of a jurisdiction's entire correctional system. For example, an immediate consequence of the 1980 riot in New Mexico was that the state signed a "consent decree" with the state's inmates. This decree, to this day, governs nearly every aspect of corrections in New Mexico, from the cell space that must be made available to inmates, to rehabilitative programming, to the temperature of the water used to wash dishes. One need only think of the consequences of the Attica prison riot in New York (Wicker 1975; Aziz 1990, chap. 3), or the Woolf report in Great Britain following the riot and occupation of Strangeways

prison in Manchester (Player and Jenkins 1994), to see that prison riots, like many rebellions and revolutionary situations, can lead to major changes in the regime of authority merely in response to the temporary loss of control faced by the governing bodies.

Finally, to return to Tocqueville ([1856] 1955), it is a consistent finding for both revolutions and prison riots that such events frequently follow efforts by the administration to make reforms to *improve* the administration and conditions in the society or the institution. We offer an explanation of this fact below; at this point, we merely note it as yet another striking similarity between the dynamics of these phenomena.

Many of these similarities have been noted by other observers. Thus it is not surprising that efforts to explain prison riots in the criminal justice literature have run strikingly parallel to efforts to explain revolutions over the last few decades. Just as theories of revolution from the 1950s through the 1970s were divided between theories that focused on the motivations of revolutionaries (rising expectations theories such as those of Davies 1962; Huntington 1968; Gurr 1970) and systemic theories that focused on the integrity of social structures (Johnson 1966; Smelser 1963), so too, leading works on prison riots featured these viewpoints. As noted above, Sykes (1958) took a firmly functionalist view of the prison, arguing that riots were a response to intrusions on, or distortions of, the normally functional social system that prisoners created for themselves within the walls. In contrast, James Jacobs (1977) took a “rising expectations” approach, arguing that riots became more likely when a firm, traditional prison regime was replaced by a more tolerant and liberal one that gave prisoners both higher expectations about their privileges and more resources with which to make demands and to organize to rebel.

And, just as psychological and functionalist theories of revolution were found to be lacking when confronted with new empirical evidence since the 1970s, to be replaced by state-centered theories (Skocpol 1979; Goldstone 1991), so too, recent empirical studies have shown that these traditional explanations of prison riots will not hold. Specifically, the comparative study of prison riots (Useem and Kimball 1991) and of prison administration (DiIulio 1987, 1991a; Sparks, Bottoms, and Hay 1996)—a wave of scholarship now known as the “new old penology” (DiIulio 1991b)—revealed that unlike Sykes’s prediction, imposing stricter authority in prisons usually did *not* lead to riots. But in contradiction to Jacobs, these studies also found that “looser” prison regimes did not always lead to riots either. In fact, the whole notion that either “loose” or “tight” prison regimes were particularly disposed to riots was rejected. It was found that either “loose” or “tight” regimes could be effective; rather, it was generally in periods of transition or reform of existing practices—whether from tight to loose, or from loose to tight—that prison riots were

more likely to occur (DiIulio 1987, pp. 15–20; Sparks et al. 1996, pp. 300–306).

While psychological and functionalist theories of prison riots thus seem clearly flawed, no new theoretical perspectives, analogous to those of state-centered theories of revolution, have emerged in the study of prison riots. Although DiIulio sees value in bringing management theory into the study of administration of prisons, and it has been argued that a synthesis of social breakdown and relative deprivation theories of social protest may better inform studies of prison riots (Useem and Kimball 1991), it still seems highly desirable to take the step of examining whether more recent theories of revolution may do a better job than earlier ones in extending their explanatory scope to the phenomenon of prison riots.

STATE-CENTERED THEORIES OF REVOLUTION

The most widely cited state-centered theory of revolution is that of Theda Skocpol (1979, 1994). Her main innovation was to argue that the origins of revolution lie not in the desires of revolutionaries to overthrow the state but in the vulnerability of particular states to revolutionary conflicts. Noting the “Janus” problem of the state, which simultaneously faces external pressures from military and economic rivals and internal pressures from elites determined to preserve or improve their own status, Skocpol argued that social revolutions could arise only where states facing moderate to strong external pressures also harbored elites who had (and used) a position of significant leverage to oppose the central state. In addition, drawing on Charles Tilly’s (1993) observation that popular insurrection requires that people have the ability to mobilize for action, Skocpol argued that the condition of elite leverage must be combined with peasant-level organization. This organization, in turn, was to be provided either by traditional self-governing peasant villages or by a peasant-based political party (such as the Communists in China). Thus Skocpol’s formula for revolution consisted of (1) external pressures; (2) elites with institutional leverage that blocked or opposed state efforts to respond to those pressures, paralyzing the state; and (3) an autonomous, self-organized rural population that could mobilize to act on the opportunity provided by the state’s incapacity.

All of these characteristics of vulnerable states have possible analogues in prisons. We have already noted that wardens are subject to numerous external pressures affecting the numbers and conditions of prisoners and the resources available to prison administrators. Elite leverage—in the form of guard unions that oppose wardens, for example—is indeed also implicated in some prison riots. And the “autonomous and self-organized” population is certainly found in some prisons in the form of prison gangs.

Nonetheless, the simple formula of conditions 1 + 2 + 3 does not provide, in the case of prisons, an adequate explanation for the occurrence of prison riots. It is usually *not* the case, for example, that prison gangs initiate riots under the conditions of weak authority. Many severe prison riots have occurred where there was little or no gang activity. Moreover, many "autonomous, self-organized" prison inmate groups, such as born-again Christians and Black Muslims, are conspicuous for *not* taking part in prison riots (Useem and Kimball 1991).

In addition, elite leverage in the form of guard unions is also the exception, rather than the rule. While guards of course play a pivotal position in prison administration and, as we shall note below, are crucial in carrying out or impeding the directives of the warden, their formal institutional leverage, in the manner of the French *parlementaires* or the English Parliament, is usually nil.

From Skocpol's theory of revolution, then, the most useful element to take into the study of prison riots is a sensitivity to the joint external and internal pressures faced by wardens.² But this does not take us very far in identifying the full range of conditions that lead to riots in particular prisons. We must therefore consider some of the defects in Skocpol's theory of revolutions and recent efforts to correct them.

The chief defects of Skocpol's theory stem from its cardinal virtue—its relentless structuralism (Goodwin 1997). This structuralism leads Skocpol to neglect two factors that are increasingly realized to be crucial parts of any full explanation of rebellion and revolution: the role of agency, or decision making, by revolutionary actors (Kiser and Hechter 1991) and the role of feelings of injustice (Moore 1978; Gurr and Goldstone 1991; Gamson, Fireman, and Rytina 1982). Prodded by the trenchant critiques of structural approaches by Selbin (1993), Foran (1993, 1997), Goodwin (1994, 1997), and Emirbayer and Goodwin (1996), Jack Goldstone has sought to make greater room in a state-centered approach for beliefs about the injustice of the state and for the role of contingent decision making by both the state and revolutionaries.

Elsewhere (Goldstone 1991), an analysis of early modern revolutions extended Skocpol's analysis in several ways (see also Collins 1993). First, where Skocpol had argued that war and economic competition were the main fiscal burdens on the state, Goldstone substituted the broader concept of *fiscal and administrative stress*, including in addition the problems

² Skocpol has suggested something like this conclusion herself. When faced with the difficulties of applying her theory to divergent cases, such as that of the Iranian Revolution of 1979, she has noted that while particular details of her theory may not generally apply to all cases, what will remain valid and useful in her approach is a historically grounded analysis of the vulnerabilities of the state (Skocpol 1994, p. 243).

of coping with inflation and the rising real costs of administering larger populations and equipping larger armies. Second, where Skocpol had argued that elites needed positions of leverage against states to block and paralyze state administrations, Goldstone emphasized *divisions and alienation among elites* as the critical factor leading to revolution. For example, even in the Ottoman and Chinese empires, where elites lacked any institutional leverage analogous to European parliaments, internal conflicts among bureaucratic and landed elites, and their opposition to the court, created administrative chaos, hamstrung tax collection, and weakened the state sufficiently for massive regional and military rebellions to emerge.

Third, and more important, this work has emphasized the role of ideology in setting the stage for rebellion. While noting that administrative failures, elite dissension, and popular grievances are critical elements leading to revolt, the argument is made that state crises also comprise "a situation in which significant numbers of elites and popular groups believe that the central authorities are acting in ways that are fundamentally ineffective, immoral, or unjust" (Gurr and Goldstone 1991, p. 331). This widespread sense of injustice is articulated in "ideologies of rectification and transformation [that] become increasingly salient" (Goldstone 1991, p. 460). Such ideologies are a critical element in collective action (Gamson et al. 1982; Foran 1997). Not only does a widely shared ideology reinforce a sense of injustice and group solidarity, but it provides a way to overcome the free-rider problem. Drawing on social bonds established in prior interaction, and the sense of shared identity that comes from articulating an ideology of rectification or change, potentially revolutionary actors can calculate the consequences of their action with confidence that if the opportunity to act presents itself, others will join them (Goldstone 1994).

Finally, it also has been claimed that the state's response to initial protest actions can shape various actors' choices regarding further actions (Goldstone 1998). If the state treats the grievances of its opponents as reasonable and allows them means of protest, then the opposition is likely to organize itself within existing institutional boundaries, as a social or reform movement. Moreover, if the state acts competently to respond to grievances or to quell violent and disruptive acts, then the state is likely to gain support from the broader society. But if the state treats the grievances and actions of its opponents as wholly unacceptable and reacts with measures that are seen as arbitrary, unjust, ineffective, disproportionate, or inconsistent, then the state is likely to galvanize the opposition, push members of the broader society into sympathy or support for the opposition, and thus further the development of, or even trigger, a revolutionary situation.

In sum, while Goldstone has remained firmly in the mode of state-centered theorizing about revolutions, he has modified the strict structur-

alism of earlier versions of this theory and argues that there are now five conditions key to the development of a revolutionary situation. These are (1) state fiscal stress or other conditions that undermine the balance between a state's resources and capacities and its administrative burdens, eroding the effective administration and implementation of state policies; (2) elite dissension with state rulers and internal conflict among elites, which prevents elites from supporting and advancing the state's policies; (3) grievances among the populace about state actions, or about the state's inaction or inability to ameliorate material conditions, that depict the state as ineffective or unjust, providing a motivation for protest against the state; (4) the spread of heterodox or revolutionary ideologies, which indicates that grievances and a desire for change are widely shared among potentially revolutionary actors; (5) state actions, taken in response to expressions of grievances, that are seen as excessive, arbitrary, unjust, ineffective, or precluding peaceful reform, and therefore turn efforts by aggrieved parties to seek remedy from reform to attempts at revolution. It is this five-part theory that, we believe, can help explain the dynamics of prison riots.

EXTENDING THE THEORY OF REVOLUTIONS TO PRISON RIOTS: SOME CAVEATS

Before reviewing the case evidence, we need to address several common misperceptions regarding prison riots, collective action, and theories of revolutions. These are the role of crowding in prison riots, the role of formal organization in protest action, and the determinacy or predictability of revolutions.

Crowding as a Cause of Prison Riots

There is an *apparent* analogue between the analysis of early modern revolutions that stressed population growth as a cause of state breakdown (Goldstone 1991) and a commonly expressed view that prison riots are often caused by overcrowding (McShane and Williams 1996, p. 408; McCain, Cox, and Paulus 1980). However, as stated, these observations are both misleading.

Goldstone actually did *not* argue that population growth leads to revolutions, using the negative case of the Japanese Meiji Restoration to demonstrate that revolutionary situations can sometimes flow from periods of population stability. Rather, a conjunctural, state-centered theory of revolutions was used to claim that the first four conditions noted above (fiscal stress, elite divisions and conflicts, popular grievances, and the heightened cogency and appeal of radical ideologies) led to revolution and

rebellion. It was then demonstrated that the absence or presence of all of these conditions was strongly related to the balance of resources flowing to the state, to old and emerging elites, and to popular groups and that this balance in turn was strongly affected by waves of population stability or growth. Only where population increases could not be accommodated by social institutions, and the resulting imbalances gave rise to the four key conditions, would a revolutionary situation occur.

It is interesting that recent research on prison crowding has come to a quite similar conclusion. That is, increases in the number and density of prisoners in an institution are not strongly associated with increases in the propensity to riot. In fact, the most sophisticated quantitative study to date found no zero-order correlation between crowding or increased crowding, on the one hand, and measures of riots, inmate assaults, or staff assaults on the other (McCorkle, Miethe, and Drass 1995, p. 323). What appears to matter for maintaining order in prisons is not crowding per se but whether prison administrations have the resources and managerial skill to respond to the additional strains imposed by crowding. With a well-organized and unified administration and adequate resources for staff supervision and physical maintenance, prisons *can* be managed quite effectively under conditions of crowding (Camp and Camp 1989; Gaes 1994).

Thus for both revolutions and prison riots, the degree of population increase is *not* directly associated with the degree of risk of rebellion. What matters is how, and how well, administrators respond to population changes and the resources available to them.

The Contingent Role of Formal Organization in Collective Action

For much of the last several decades, a "prevailing wisdom" has held that because of the problem of "free riding," collective action—especially risky collective action—will fail unless such action is mobilized and reinforced through some kind of formal organization (Olson 1965; Tullock 1971). Even authors not explicitly working in a rational choice framework have argued that some kind of formal organization is either necessary for protest action or strongly helpful for movement success (Skocpol 1979; Tilly 1978; Gamson 1975; McCarthy and Zald 1979).

Yet this argument about the necessity, or at least highly positive impact, of formal organization on the emergence and success of collective protest, has not been without critics. Piven and Cloward (1992, p. 310) have argued that "riots require little more by way of organization than numbers, propinquity, and some communication. Most patterns of human settlement . . . supply these structural requirements." Indeed, they claimed that beyond a bare minimum, formal organizations can inhibit the radicalism

of protest groups. In his reexamination of Gamson's data on social protest groups, Goldstone (1980) found that the length of time from protest initiation to success was *inversely* related to the presence of formal organization—that is, groups that took the *longest* time to succeed had the most formal organization. Formal organization may have helped challengers to survive, but it did not accelerate their time to success.

Recently, analyses of events in Eastern Europe (Oberschall 1993; Opp, Voss, and Gern 1995), renewed study of the Civil Rights movement in the United States (Robnett 1997), and examination of other social movements (Tarrow 1994) have reinforced Piven and Cloward's view that formal organization is not a requirement for protest or its success. The popular mobilization in East Germany and Czechoslovakia in 1989–91 occurred without formal organization of protestors. Rather, numbers, proximity to churches that provided foci for assembly in Leipzig and Prague, and—most important—informal communication among neighborhood and friendship networks, which spread a belief in the possible efficacy of protest action, produced a repeated pattern of popular protest that brought down what had appeared to be formidable regimes (Pfaff 1997).

Moreover, reexamination of the Civil Rights movement by Robnett suggests that, while organizational resources based on neighborhoods and churches were important for early mobilization, the crucial factor in sustaining movement action was the informal “bridge” ties provided by women without formal organizational authority. The more formal organizations of the Southern Christian Leadership Conference and the Student Non-Violent Coordinating Committee played a rather more ambiguous role; indeed, she argues that the demise of the Civil Rights movement occurred in part because these organizations became more a means of promoting the careers of their leaders than of achieving the goals of the movement.

Collective action certainly requires some coordination and some way to overcome the “assurance problem,” that is, how to assure that if some part of a group acts, others will also act and not free ride (Olson 1965; Chong 1991; Lichbach 1995). Crucial to solving this assurance problem is some way for group members to ascertain that they share a belief in the wrongfulness of current conditions and a belief in the efficacy and necessity of action, should an opportunity arise. This may occur through formal organization, but it may also occur through informal neighborhood groups, occupational or workplace groups, or friendship networks (Gould 1995).

Indeed, such informal networks may be the necessary buttress for formal organization as well. As Tarrow (1994, p. 57) eloquently argues, “What counted [in many protest organizations] was the interpersonal

solidarities that underlay these organizations rather than the organizations themselves. . . . The role of informal movement networks helps to explain why even the most formidable movement organizations today can quickly collapse, and how movements can arise with no obvious organizational framework." In sum, the current state of the evidence argues for a more nuanced view of the relationship of organization to protest or revolution—such organization may be a facilitating factor, but it seems neither necessary nor sufficient for protest or revolution, or for their success.

This view is fully borne out in the study of prison riots. Although formal organization among inmates can sometimes facilitate prison riots (e.g., gang organizations orchestrated a 1975 riot in the Joliet prison; see table 1 below), such organizations are neither necessary, nor sufficient, for riots to occur. Indeed, prison riots are an exemplar of how informal networks can facilitate collective action against overwhelming authority, even from a position of close supervision and near-zero autonomy. Prison riots commonly occur in institutions where formal organizations among inmates are strictly banned. If the "conventional wisdom" of collective action and mobilization theory of the 1960s were true—that some sort of formal organization and selective incentives are critical to overcome free-rider problems and to empower people who are individually powerless against superior authority—then prison riots should be impossible.

Yet the risky collective action of prison riots, like that in many revolutionary and social protest movements, often occurs without any such organizational framework. What seems to matter is that prison inmates—who share cell blocks, dining and recreation facilities, oftentimes job sites, and have an obvious group identity as "inmates," can draw on these informal networks and sites of group interaction to communicate and build shared grievances and beliefs. Unless confined to an extreme degree—for example, kept in 24-hour solitary confinement—inmates have the "numbers, propinquity, and communication" to establish patterns of shared grievances and to coordinate their actions. Moreover, even where formal organization exists, its causal impact may work to *reduce* protest—in several cases described below, Muslim or Christian brotherhood organizations of inmates helped keep their members *out* of prison riots, organizing nonparticipation, if you will.

The role of formal organization in prison riots, then, as in revolutions and social movements, is that of a contingent factor, not a causal one. Formal organization may occur in a way that facilitates riots; or it may be wholly absent; or it may occur in a way that works against riot action. Unlike the causal conditions described below, one simply cannot draw any firm conclusions from the existence or scale of prisoner organizations regarding the likelihood of prison riots.

Determinacy and Prediction of Revolutions

Goldstone (1991, 1995) states that no theory of revolutions can provide a deterministic, lawlike necessary and sufficient set of conditions for a revolution that says "If conditions X, Y, Z hold, a revolution will occur within *N* days, or weeks, or years." The role of historical contingency is simply too great. A state with an otherwise low risk of rebellion could be plunged into turmoil by an extraordinarily incompetent administration, or a state with a high risk of rebellion might escape overt conflict through the fortunate lack of an event that would trigger underlying tensions. What theories of revolution can provide, Goldstone argues, is the kind of prediction offered by a barometer with regard to weather. That is, if most of the conditions for a revolutionary situation are strongly present, then the likelihood of a revolution occurring is high and rising; if most of the conditions for a revolutionary situation are weak or absent, then the likelihood of a revolution occurring is low and falling.

Something quite similar must be said about contingency and determinism in prison riots. For example, in some cases, a simple act of extreme neglect (e.g., leaving a cell door unlocked in a high-security prison with highly aggressive prisoners) can initiate a disturbance by inmates; ineffective or incompetent response can then lead to a full-scale riot. In other cases, even a prison with high tensions among guards and prisoners can go weeks or months without an incident or opportunity that leads to riotous confrontations. However, as we shall see, it is usually *not* the case that prison riots are random events. Rather, on surveying multiple cases, there does appear to be a syndrome or set of circumstances that typically prefigures a prison riot. And that syndrome is well described by the five factors elucidated above from the state-centered theory of revolutions.

Given that people can respond to varying situations, theories of revolutions or riots can only warn of conditions leading to breakdown, not predict them. Indeed, states or wardens who see the "barometer" rising can take measures to reduce those pressures and avert a crisis. It may take time, effort, and resources to respond to these pressures, but crises may then be averted. In contrast, not knowing how to read the "barometric signs," or taking actions that make such pressures worse, is likely to lead to the breakdown of order in the state (revolution) or in a penal institution (prison riot). It is only in this limited sense, allowing for contingency and agency, that the state-centered theory of revolutions or prison riots can be predictive.

THE CASES AND METHODS OF ANALYSIS

We consider data from 13 major riots that occurred at medium- and high-security prisons in the United States from 1952 to 1993 (see table 1). The

TABLE 1

CASES AND CONDITIONS FOR PRISON RIOTS

CASE	TIME	CONDITION*				
		1	2	3	4	5
State Prison of Southern Michigan (1952)	<i>t</i>	High <i>a,b,d,f,g</i>	High <i>a,f</i>	Med <i>f</i>	Med <i>c</i>	Low
Oahu, Hawaii (1960)	<i>t - 4</i>	Low	Low	Low	Low	Low
	<i>t</i>	High <i>d,g</i>	High <i>a,d</i>	Low	High <i>e</i>	Low
Attica, New York (1971)	<i>t - 5</i>	Low	Low	Low	Low	Low
	<i>t</i>	High <i>a,b,d,e,g</i>	High <i>a,b,d</i>	High <i>a,c,d,e</i>	High <i>a,b,d</i>	High <i>a,b,c,e</i>
Joliet, Illinois (1975)	<i>t - 2</i>	Low	Low	Low	Low	Low
	<i>t</i>	High <i>c,d,e</i>	Med <i>f</i>	High <i>d,e</i>	High <i>a</i>	High <i>a,c</i>
New Mexico (1980)	<i>t - 5</i>	Low	Low	Low	Low	Low
	<i>t</i>	High <i>a,c,d,e,f</i>	High <i>b,c</i>	High <i>a,b,c,d,e</i>	High <i>b,d,e</i>	High <i>d,e</i>
State Prison of Southern Michigan (1981)	<i>t - 5</i>	Low	Low	Med <i>a,b</i>	Low	Low
	<i>t</i>	High <i>g</i>	High <i>a,d</i>	Med <i>f</i>	Med <i>b,d</i>	High <i>f</i>
West Virginia (1986)	<i>t - 2</i>	Low	Low	Low	Low	Low
	<i>t</i>	High <i>a,b,d</i>	High <i>c</i>	High <i>a,b,c</i>	Med <i>b,d</i>	Med <i>e</i>
Kirkland, South Carolina (1986)†	<i>t - 3</i>	Low	Low	Low	Low	Low
	<i>t</i>	High/low <i>e</i>	Low	High/low <i>d</i>	Med/low <i>d</i>	Low
Atlanta, Georgia (1986)	<i>t - 1</i>	Low	Low	Low	Low	Low
	<i>t</i>	High <i>g</i>	Low	High <i>f</i>	High <i>b</i>	High <i>a</i>
<i>t - 4</i>	Low	Low	Low	High <i>a,d</i>	Med <i>d</i>	Low

Coxsackie, New York (1988)	<i>t</i>	Med	Med	Med	Low
		<i>a, e</i>	<i>b</i>	<i>d</i>	
Mack Alford Correctional Center, Oklahoma (1988)	<i>t - 2</i>	Low	Low	Low	Low
	<i>t</i>	Med	Med	Med	Med
		<i>d</i>	<i>a, b</i>	<i>a</i>	<i>a</i>
Camp Hill, Pennsylvania (1989)	<i>t - 3</i>	Low	Low	Low	Low
	<i>t</i>	High	High	High	Med
		<i>a, f, g</i>	<i>a, b</i>	<i>b, d, e</i>	<i>a</i>
	<i>t - 3</i>	Low	Low	Low	Low
Lucasville, Ohio (1993)	<i>t</i>	High	High	High	High
		<i>b, g</i>	<i>a, f</i>	<i>b, e</i>	<i>c</i>
	<i>t - 2</i>	Med	Low	Low	Low
		<i>e</i>			

NOTE.—Cells that are designated as “high” or “medium” are based on specific evidence. Cells that are designated “low” should be viewed much more cautiously, especially those in the “*t - N*” rows. A “low” value may genuinely reflect a low value, but it may also reflect missing data. False lows may result from the fact that, in the absence of a major disturbance, no efforts were made to record the presence of the condition in a form that would be accessible to us, *t* is the time period immediately leading up to the riot; *t - N* is some prior period that is noted in the case studies as a time of greater stability, depending on the cases and nature of the available research material, this may be one to five years prior to the riot

* CONDITIONS.—The five conditions established in the text are indicated by the arabic numeral in each column heading, the observed factors contributing to those conditions are indicated by letters

1. *Administrative erosion or ineffectiveness*: (a) fiscal stress, (b) outside demands for change without new or with declining resources, (c) rapid turnover of wardens or state corrections officials; (d) new warden or state prison officials who attempt to effect new rules/regimes but are unable to implement them due to lack of resources or staff resistance, (e) prisoners flaunting of rules or abuse of privileges (e.g., high levels of contraband, drugs) or high levels of prisoner/prisoner violence, indicating failure of administration to maintain order, (f) high levels of crowding without resources or administrative strategy to handle problems; (g) deep divisions over the agency's mission or its core tasks
2. *Grievances, conflicts, or resistance to procedures among staff*: (a) resistance to implementing new routines or rules, (b) irregular, slipshod, or highly variable implementation of prison routine; (c) sick-outs or unusually high staff turnover; (d) unionized or other organized complaints against prison administration, (e) staff corruption, abuse of position, or excessive staff/prisoner violence, (f) staff expressing concerns about being “intimidated” by prisoners or being “unable to do their jobs” because of prison rules
3. *Prisoner grievances about warden or staff actions or prison conditions or rules*: (a) state or judicial findings that conditions are substandard or prison rules/procedures are illegal/unconstitutional, (b) recent restrictions/reductions on prisoner activities or privileges, absent a plausible rationale for such restrictions/reductions, (c) confrontations or litigation/petitions by inmates regarding conditions or prison procedures, (d) “murmuring” of complaints in the prison that suggest widespread shared prisoner dissatisfaction with conditions/procedures, as reported by staff, (e) prisoner organizations that nurture feelings of injustice (e.g., gangs), (f) changes in rationales for imprisonment, or the length of imprisonment, that are disputed by inmates
4. *The spread of ideologies of protest or rebellion*: (a) revolutionism; (b) constitutionalism; (c) rehabilitationism; (d) rationalism, (e) Hobbism
5. *Actions by administration or staff that demonstrate ineffectiveness or injustice*: (a) confrontation of prisoners and staff in which prisoners appear to gain the upper hand or staff are penalized/blamed by administration; (b) transfers of prisoners, lockdowns, or other disciplinary action that punishes the wrong person or too many persons, (c) transfers of prisoners, lockdowns, or other disciplinary action that is poorly or ineffectively carried out (e.g., in a way that prisoners can readily observe or interfere with), (d) escapes, especially mass escapes, (e) failure to make good on public promises/requirements for improvement of conditions/procedures, (f) actions by staff that are encouraging or facilitating of a riot, usually due to staff/administration conflicts

† For the cells in this row with two values, the first refers to the special unit of Kirkland prison where the riot occurred, the second refers to the prison as a whole

case studies are not a random or stratified sample. However, they offer a reasonable variety of geographical-, state-, and federal-level and medium- and high-security facilities and do include all the most notable U.S. prison riots of the last 40 years. Moreover, they are sufficiently separate in space and time that they can be taken as independent events (unlike, e.g., riots in the same year in the same state corrections system, which are generally not independent events). In addition, these particular cases are chosen because factual case studies of these events are available from secondary texts. (For one case, Jackson, 1952, archival data were collected to supplement the secondary sources.)

The goal of this section is to show that analogues of the five elements of a "revolutionary situation" were largely absent in these facilities during periods of stability, but most or all of these elements can be demonstrated to have arisen shortly before these major riots occurred. This "before and after" comparative case-study design has well-known problems, including selecting cases based on the eventual positive outcome of the dependent variable and risking other forms of bias from the small number of cases (Lieberson 1992). Still, it is a defensible and widely used design, particularly in comparative-historical studies of revolution, for tracing causal dynamics in multiple cases and doing an initial examination of the applicability of a given theory to diverse cases.³ Alexander George (1997) has argued that this kind of test for the "congruence" of additional and novel cases to a set of clear propositions derived from an existing theory is a major step in examining whether complex and interactive causal relationships described in that theory can be applied to additional phenomena.

In addition, though one cannot apply standard statistical tests based on the normal or *t* distribution to test the effects of five variables over just 13 cases, it is possible to obtain strong statistical results using less restrictive nonparametric tests, such as the matched-pairs signs tests used in this study (Conover 1980; Siegel 1988). Although comparative case studies using three or five cases do not provide this option, given a dozen or so cases, nonparametric tests can provide strong quantitative tests of null hypotheses and allow us to explore the possible impact of coding errors. The number of our cases is thus sufficient to allow formal analysis, as well as case summaries, of the data.

Taking the five conditions for a revolutionary situation listed above and rephrasing them to create analogues for prison riots is straightforward: (1)

³ For an extensive debate on the use of small-*N* case studies to elucidate complex causal processes, see Ragin and Becker (1992), Collier and Mahoney (1996), Lieberson (1992), and the special issue (vol. 16) of *Comparative Social Research*, "Methodological Issues in Comparative Social Science," esp. Tilly (1997), Goldstone (1997), and Ragin (1997).

fiscal stress or other conditions that undermine the balance between a prison administration's resources and capacities and its administrative burdens, eroding the effective administration and implementation of prison policies; (2) dissension between the warden and corrections officers or internal conflicts among the corrections officers, which prevent the prison staff from supporting and advancing the warden's policies; (3) grievances among the prison inmates about the warden's or staff's actions, or about their inaction or inability to ameliorate material conditions, that depict the warden or staff as ineffective or unjust, providing a motivation for protest against the prison administration; (4) the spread of ideologies of protest or rebellion, indicating that grievances and a desire for change are widely shared among prison inmates; (5) warden or staff actions, taken in response to expressions of grievances, that are seen as excessive, arbitrary, unjust, ineffective, or precluding peaceful reform, therefore turning efforts by aggrieved parties to seek remedy into attempts to create a prison riot.

We have translated these five conditions into observable elements in specific prisons (e.g., legislation to require better conditions or more prisoners, complaints by staff or by prisoners, etc.) shown in the notes to table 1. We then coded each prison as to the level of each condition ("high," "medium," or "low") at two times: first, in the year that the prison riot occurred (t), second, in a prior period ($t - N$), ranging from one to five years earlier, which the case studies indicate was a noticeably different and more stable period in the administration of that same prison. We thus have a table of 130 cells, indicating values during "riot" and "stable" situations for each of five conditions over 13 different prisons. We shall first review the case-study evidence for the presence or absence of the key conditions leading to prison riots then proceed to a formal analysis of the data.

CONDITIONS LEADING TO PRISON RIOTS: EVIDENCE FROM CASE STUDIES

Fiscal Stress

Useem and Kimball (1991, p. 218), in summing up their case studies of prison riots, claim that "the key factor has not been organization of the inmates, but the disorganization of the state. . . . The riot-prone system is characterized by certain ailments which . . . sap the ability of the state to contain disturbances. . . . Prior to all the riots under study [five of the cases listed below], there was a breakdown in administrative control and operation of the prison."

Concrete descriptions of such breakdowns are numerous. However, most administrative crises generally had one of two sources; some have

both. The first is an administrative change—either a change in wardens or a reform of prison administrative practices (often, the two coincided)—that was carried out without the full support of the prison staff and without effective implementation. The result was a loss of confidence or respect for the warden among inmates or staff and slipshod, disorganized, and inconsistent operation of the prison routine. Indeed, one of the most striking findings of the analysis to follow is that specific *direction* of change—toward a more liberalized or more rigidly controlling prison regime—is *far less important* than the fact of change that is not accepted or poorly implemented.

The second source of administrative crises is financial and legal pressures imposed from outside the prison. Financial pressures may arise from changes in sentencing laws and parole regulations that result in an increase in the inmate population or from the imposition of budget cuts or freezes by state or federal legislatures. Budget cuts or freezes hurt staff morale and may require reducing services or staff. They also lead to deferred maintenance of the physical plant, and unlike, say, a university, the integrity of a prison's physical plant is critical to its mission. Sentencing or parole changes that increase inmate populations, if not accompanied by additional funding to increase staff, services, and maintenance or improvement of physical plant, can also greatly increase the strains on a prison's staff and administration. In addition, management options normally available—such as emptying a cell block for renovation or separating troublesome inmates from others—are lost in the same but overcrowded prison.

In addition to budget and sentencing strictures mandated by legislatures, law courts have periodically issued standards for the treatment and incarceration of prisoners that existing prisons fail to meet. In many cases, such standards are issued by courts, while legislatures defer providing the means to meet those standards. In such cases, media criticism of prison conditions often reaches well into the prisons and informs the views of both guards and prisoners. Major political and judicial authorities will state clearly, for all to hear, that prisons are violating prisoners' rights and that existing prison regimes are unjust, illegal, and unconstitutional. Such announcements put the prison administration on the defensive and build a strong presumption on the part of prisoners and staff that changes are due and imminent. Aside from unsettling the morale of the institutions, such outside demands reinforce prisoners' beliefs that any protest they make will have authoritative outside support.

Four of the riots under study—State Prison of Southern Michigan (SPSM or "Jackson") (1952), Oahu (1960), SPSM (1981), and Attica (1971)—occurred in periods of broad penal reform from above. In the early 1950s, there was a worldwide movement toward "scientific penol-

ogy," the thrust of which was to reorganize prisons along less authoritarian lines and to emphasize the rehabilitation of inmates over their mere custody (Newbold 1989; Rotman 1995, pp. 188–91). And, in the early 1950s, a wave of riots swept across U.S. prisons, including Jackson prison. In Michigan at this time, a reforming corrections commissioner—widely recognized for his humanitarianism and commitment to the "new penology"—sought to improve conditions for inmates. The changes included increasing mail and visiting privileges, substituting inmates for guards in the committee responsible for censoring mail, allowing longer recreation periods, and transferring authority over inmates' activities from custody staff to treatment staff.

However, the reform agenda was stymied. First, on the eve of the riot, the Jackson prison was overcrowded: 6,300 inmates lived in a space designed for 5,000 (Bannan 1952), while reductions in the budget for corrections made by the state legislature in a period of rising costs forced the prison to reduce the guard force below a point of safety and effectiveness (*New York Times*, April 23, 1952). Second, custody correctional staff were deeply resistant to the reforms. The feud between the custody staff and the treatment staff "split the prison wide open" (ACA 1970, p. 67), a point elaborated upon in the next section. For now, it may be noted that the riot's termination, 92 hours after its start, reflected the forces summoning it forth: The deputy warden for treatment (aligned with the commissioner) acceded to all of the inmates' demands, telling the inmates that the riot "may presage a new era of good, sound inter-relationships between inmates and administration in American prisons. They have done a service. Congratulations to you men of 15-block [who started and led the riot]" (*New York Times*, April 25, 1952). In a special session of the legislature, correctional officers demanded that the deputy warden for treatment be discharged, as he was, and somewhat later, the commissioner was let go as well (*New York Times*, April 27, 1952).

Significantly, Oahu prison in Hawaii did not have a riot in the early 1950s, even though it experienced Jackson-like tensions arising from a shift from a pure custody orientation to a more liberal and rehabilitative stance. By 1953, both inmates and staff groups were deeply divided. But the ensuing period of confusion and violence was short-lived. The reason for the successful transition at Oahu was effective institutional leadership. The warden initiated a series of "Warden's Nights": formal meetings with the entire inmate body, in which he effectively explained the transition to inmates. He also met with the old guard security staff, reassuring them that the new treatment orientation would not turn over the institution to prisoners, as they had worried. According to McCleery (1961, p. 182), "The warden's direct and eloquent answers to those issues are credited by inmates and staff alike with breaking the back of institutional tension." Also

important was that the warden's efforts had the strong backing of the state legislature, which in hearings on the prison made it clear that transition was not only wise but inevitable. "Legislative endorsement of the administrative position and the rehabilitative theory was followed by a significant decline in tension and disorder" (McCleery 1961, p. 182). In short, where a shift in policy was followed by unified support for the warden and effective implementation, there was neither administrative disorder nor riot.

However, this stable interlude at Oahu ended in 1960. A new liberal-minded correctional commissioner sought to push reform even further, but this time the reform was met by a recalcitrant and mobilized staff. A union emerged three months after she took office. Public controversy also broke out between the commissioner and career prison officials over the commissioner's alleged interference in management issues. When the riot began, "the custodial force did not really try to quell the riot. . . . The structure of effective authority over the guard force had collapsed as certainly as had the structure of authority over inmates" (McCleery 1968, p. 138).

The 1971 Attica riot followed a pattern almost identical to that of Jackson and Oahu. The New York Department of Corrections in 1970 and 1971 "attempted to enact a liberal prison policy but was hamstrung, on the one hand, by budgetary constraints, and, on the other, by the intransigence of the custodial staff, [which] successfully resisted these changes" (Useem and Kimball 1991, p. 25). Also, a federal court had ruled in 1970 that prison procedures violated prisoners' rights of due process. A new commissioner of corrections for the state, Russell Oswald (appointed January 1, 1971), repeatedly promised Attica inmates that reforms would be forthcoming. But Oswald was frustrated by staff resistance and lack of funds, so most of the promised reforms were never implemented. The budget crunch also created a staff shortage at Attica, such that routine inspections were reduced and faulty gates (which later failed) were not detected. In the riot later that year, many of the rioters' demands were simply for the reforms they had come to expect as due them (Useem and Kimball 1991, pp. 25-26).

A riot at the State Prison of Southern Michigan in 1981 also came about as a result of change mandated from above, but here the initial force for change was the federal courts. The courts ruled that the prison must provide "due process" hearings for inmates charged with violating prison rules. The upper administration at the prison went along with these newly mandated rules. This, along with staffing cutbacks and changes in procedure, left the correctional officers convinced that the administration was more interested in appeasing prisoners than in maintaining the authority and safety of the correctional officers. Their union challenged the new

rules and the warden, creating such widespread animosity between officers and the prison administration that the officers attempted to take over the prison and thus initiated the Michigan riots (Useem and Kimball 1991, pp. 129–33).

In four of the other cases under study, New Mexico (1980), Lucasville (1993), West Virginia (1986), and Joliet (1975), riots occurred in prisons in which the opposite trend in corrections was occurring. That is, the riots followed attempts—largely ineffective—to clamp down on inmate freedom and to eliminate inmate programs.

In the period between 1970 and 1975, the Penitentiary of New Mexico (PNM) was a well-managed prison with ample programs for inmates. However, following a political scandal involving allegations of corruption by prison officials, there was an attempt to tighten security in the prison (Colvin 1982). The reforms had the opposite effect. “Repeated organizational shocks, restructuring, rivalries, and massive turnover at high and low levels of personnel . . . ruined the system’s internal control, discipline, and general ability to function” (Useem and Kimball 1991, p. 90). Staff shortages due to budget cuts also were part of the problems at PNM, which experienced a 138% increase in prison population from 1972 to 1978. The elimination of programs for inmates, ostensibly a measure to tighten up the prison, instead closed off avenues of communication between officers and inmates and reduced the incentives for good behavior.

The impetus for reform at the Ohio Correctional Facility at Lucasville was the murder of one staff member and four inmates in the first nine months of 1990. A subsequent investigation by a state legislative committee described the prison as a “loosely run and operated organization lacking the necessary attention to detail one would expect from a maximum security facility” (quoted in OCSEA 1993, p. 17). A new warden was appointed in November 1990, specifically charged to tighten security under a formally designated program called “Operation Shakedown.” The warden, however, was stymied by both external and internal forces.

External forces included an increasing inmate population, which, in turn, required the prison to put two inmates in cells designed for one. At the same time, a federal court required that the cell assignments be racially integrated—greatly aggrieving some inmates. In addition, Ohio at this time had the second lowest officer/inmate ratio among the 50 states, with many other states having two to three times as many staff per inmate (Van Dine 1994). At Lucasville, because of this staff shortage, inmates were placed in security-sensitive work positions.

Internally, the warden lacked confidence in his administrative staff and middle managers, and they, in turn, distrusted him. Also, the warden attempted to reorganize the prison along a “unit-management” plan—that is, authority would be decentralized into living units under the direction

of a unit manager. Unit managers, however, were resented by the correctional staff, the latter feeling that "unit managers countermand or interfere with efforts by security staff to enforce institutional rules regarding inmate behavior" (OCSEA 1993, p. 42). The unit managers saw it differently, but, regardless, inmates were able to "take advantage of poor communication and turf battles" that resulted (ODRCDCC 1993, p. 3). All of this added up to a situation in which lines of authority were vague and communications up and down the line of command were poor (McShane and Williams 1996, p. 408). The riot began when the warden was unable to deal effectively with a controversy over the TB testing of Muslim inmates, who claimed that the test violated their religion. As tension mounted over the TB controversy, inmate clerical assistants learned of a planned lockdown and passed this information on to other inmates, even as many of the correctional officers were unaware of the plan. The riot occurred before the lockdown could be implemented (ODRCDCC 1993, p. 4).

The 1986 West Virginia Penitentiary riot also occurred when a new warden attempted to tighten security. A state court had ruled a few years prior to the riot that the prison conditions violated the legal rights of inmates, raising their expectations that conditions would improve. Yet the state was experiencing a deep economic depression in this period, which limited the improvements that could be made. When a new warden attempted to tighten security, inmates perceived this as a further injustice added to the already illegal behavior of the prison administration in failing to comply with the court's mandate.

Similarly, the 1975 riot at the Joliet prison occurred when a new warden attempted to crack down on gangs, which previously had been granted wide latitude in a situation of administrative chaos. At Joliet, the years before the riot had seen a revolving door of wardens and growing rivalries between the prison wardens and the state commissioners of corrections, in part because of electoral shifts. Three different wardens took office in 1972–74, not including a six-month period in which the state legislature held up the appointment of a warden and a temporary administration ruled (Useem and Kimball 1991, pp. 70–71). When the new warden attempted the crackdown, he lacked the institutional authority and staff loyalty to do so effectively.

In the eight cases described above, riots occurred when the prison regime was being changed—either to "loosen" or "tighten" the prison—and the change was implemented ineffectively, often with inadequate resources, bringing conflicts that undermined the authority, unity, and effectiveness of the prison administration. But not all prison riots are due to such changes; prisons may be debilitated by other problems of administration.

The clearest example of this is the riot at Camp Hill (1989). New sen-

tencing guidelines passed in the Pennsylvania legislature in 1982 increased the prison population, but there was no increase in staff, such that the prison superintendent estimated that on the eve of the riot the prison was short 88 officers and 60 treatment staff. The cells and locks were already in disrepair, and revenue shortfalls prevented implementation of the warden's plan to improve interior fencing to prevent prisoners from moving freely through the whole interior of the facility. Many corrections officers perceived the prison administration to be more concerned with the comfort of inmates than with the officers' safety and security, a situation similar to Michigan in 1981.

In two other cases, although there was not the extreme tension between staff and administration noted above, there was a gradual erosion of the authority, routine, and physical plant necessary to run a high-security institution. At the Coxsackie Correctional Facility in 1987–88, funds were pending, but not yet approved, to replace glass/wire windows, which were known to be vulnerable to assault. In addition, changes in personnel and assignments became slipshod, resulting in all four officers and a sergeant who were assigned to the especially high-risk Special Housing Unit having little or no experience. Following abbreviated routines instead of full standard operating procedure, they created an opening for prisoners to escape control and initiate a riot (Useem, Camp, and Camp 1996, p. 110). At Mack Alford Correctional Center in 1988, the inmate population increased from 530 to 670 prisoners in the months before the riot. At the same time, old weak walls of cinder block and plaster, which were easily punched through and created hiding places for contraband, were not yet reinforced. Finally, correctional officers had shown great loyalty to former warden Mack Alford, who had run the prison for 24 years. But when Alford died suddenly of a heart attack, his replacement did not command the same loyalty among correctional officers and installed a confusing new system of dual chains of command that unsettled the officers (Useem et al. 1996, pp. 84–88). As a result, when tensions in the prison rose over a racial incident, and prison officials attempted to remove an inmate from the prison, the procedure was handled so clumsily that inmates got the upper hand; instead of separating the prisoner for removal, the inmates were able to separate the guards and managed to take several hostages.

In sum, Camp Hill, Coxsackie, and Mack Alford illustrate the point that administrative decay weakens chains of command and control, strains the loyalty of corrections staff, and ruins the careful and consistent administration of prison routine necessary to maintain order in a high-security institution. Even small administrative failures that arise under such conditions—failed communications, poor responses, or breakdowns of physical security—can have large consequences. In each of these cases, the failure to implement obviously needed and planned physical maintenance

and improvements played a role in the ensuing riots as gates or cells or windows or walls failed and allowed prisoners to run rampant in the facility (Useem et al. 1996, pp. 59, 84, 110).

Arguably, two of the case studies did not display the hypothesized first condition of riot—severe erosion of administrative effectiveness. One possible negative case was Kirkland Correctional Institution (1986). This prison was generally well managed with good morale among prisoners and officers and respect for the warden. However, there was a local exception—the Special Security Risk Unit, with 32 highly hostile prisoners—that suffered from an extremely high turnover rate for staff and low attention to security routines. As a result, faulty locks on cell doors went undetected, allowing prisoners to escape from their cells and to initiate a breakout. “Still, for the most part, the general inmate population did not take part actively in the riot. *This may have been because most were not especially angry with the administration*” (Useem et al. 1996, p. 126; emphasis added). This is a striking case that shows that prison inmates are not always ready to launch into riot or rebellion at any opportunity.

A second arguably anomalous case is the riot at the U.S. Federal Penitentiary in Atlanta (1986) by Cuban Mariel detainees. The prison had greatly improved its management and control in the period leading up to the riot. A 1987 Bureau of Prison report found that “conditions prior to the disturbance were very good at Atlanta” and vastly better than what they had been a few years earlier (quoted in Useem et al. 1996, p. 16). This assessment was supported by several objective indicators: a reduction in the number of assaults and other serious incidents, reduced turnover among correctional officers, and few security deficiencies found in routine audits (Useem et al. 1996, p. 17).

Yet if one shifts the focus from the prison per se to the broader imprisoning authority, then the hypothesized conditions are in fact observed. Arthur Stinchcombe (1997, p. 2) makes the point that the distinctive mission of the courts is to produce legitimate decisions and that they cannot endure without achieving that intended product. By the same token, it would seem fair to say that the distinctive mission of prisons is to produce legitimate incarceration. Normally, that legitimacy is measured in the coin of material conditions, security, and services: is the prison decent, secure, safe, orderly, healthful, and (sometimes) rehabilitative by prevailing standards? This is the coin because another condition of legitimacy—that most of the inmates deserve to be incarcerated—is provided by the courts. What prisons are for is to incarcerate people who “deserve” punishment, or at least restraint, according to the order of the courts.

But this condition of legitimacy was *not* met at Atlanta in the period leading up to the riot. While the prison per se was in relatively good shape, the overall incarcerating authority was divided and disorganized, with

great debate over the legitimacy of the continued incarceration of the Mariel detainees as they awaited their expected hearings on asylum and their deportation hearings (Hamm 1995). In 1984, a jury had acquitted two prisoners, who had led a riot that year in the facility, because the jury found that their indefinite detention (while negotiations on repatriation dragged on with Cuba) was unjust. Following this decision, several federal judges and members of the U.S. Congress publicly voiced their opinion that the indefinite detention was unconstitutional and that the detainees deserved greater due process, if not freedom. When prisoners learned, without warning or due process, that deportations to Cuba would resume, they felt badly betrayed, and a riot seemed inevitable. Nonetheless, just prior to the riot there was an "unexplained" communications failure—guards were warned of plans for a coming riot but somehow failed to communicate this information to the warden, who left the prison unprepared for the riot that occurred the next day (Useem et al. 1996, p. 20).

To sum up, prison riots occur where prison administrations are already in difficulty. They may be facing unusually high turnover of administrators or staff, demands from state or federal courts that they change conditions or procedures that they are unable to meet, new administrative policies or initiatives that they are unable to implement, or fiscal problems due to increases in prison populations or budget restrictions, such that levels of staffing and physical maintenance are seriously compromised.

The two apparently anomalous cases actually lend support to the theory. In one case, where administration seemed sound and a riot did occur (Kirkland), the riot was localized, most prisoners did not participate, and the particular unit in which the riot occurred showed rather more administrative problems than the rest of the prison. In the other (Atlanta), there was a breakdown in the legitimacy of incarceration for the entire inmate population, brought about by political and judicial turmoil over the fate of the detained Cubans.

In order to save space, in the following sections we more briefly review the evidence for the remaining four conditions that increase the likelihood of riots.

Dissension between the Warden and Corrections Officers

Although administrative constraints are generally implicated in riots, they are far from sufficient by themselves to create disturbances. In several cases, prisons with long-serving and well-respected wardens managed to maintain order despite crowding, curtailed budgets, and a flawed physical plant. From 1936 to 1969, Warden Joseph Ragen and his successor Frank Pate ruled Joliet and Stateville prisons "with an iron grip," keeping order with a strict and famously harsh but consistent management style, despite

extremely limited resources (Useem and Kimball 1991, pp. 66–67; Jacobs 1977; Kantrowitz 1996). It was only with the revolving door change of administrators in the early 1970s, which also brought a new liberal corrections regime that empowered gangs in the Illinois prisons, that conditions mounted for the Joliet riot.

In West Virginia, the firm and highly respected wardens Donald Bordenkircher and his successor Manford Holland kept West Virginia Penitentiary in good order from 1979 to 1985. Their achievement was particularly striking given that Bordenkircher was hired after a spectacular escape and murder of a state trooper by the escaped inmates and that he was chronically shorthanded in regard to staff and resources. But he saw his role as tightening the prison until “it squeaks” and fulfilled that mandate. It was only when a later warden, Jerry Hedrick, sought to tighten the prison even further, but did so without the respect of either staff or inmates, that a riot occurred.

The ineffective and contested implementation of changes in policy is characteristically due to resistance by prison staff to the new procedures. Corrections officers are notoriously conservative and hostile to changes in prison routines. It takes a considerable amount of management skill, staff education, and patience on the part of administrators to gain the support of staff for change (Cressey 1977). Without such efforts, the best-intentioned plans for changes of prison routine can often provoke resistance and contribute to a crisis.

We have already mentioned the divisions between different departments at Jackson prison that “split the prison wide open” (ACA 1970, p. 67) prior to the 1952 riot and the determined and largely successful resistance of the Attica prison staff to the liberal reforms proposed by Commissioner Oswald in 1971. We have noted also that in Michigan in 1981 new due process laws took disciplining of prisoners out of the guards’ hands. The correction officers’ union challenged the warden and the new rules, creating such severe conflicts between guards and the prison administration that efforts by the guards to take over the prison triggered the Michigan riots (Useem and Kimball 1991, pp. 129–33). To expand on this point with regard to Michigan in 1952, the commissioner’s new policies gave rise to “question in the minds of many of the custodial force . . . as to the authority vested in the warden” (Jackson, Beattie, and Campbell 1952, p. 6). One correctional officer commented caustically, “They are punishing officers more than they punish inmates” (Millard 1952).

In New Mexico, guards refused to comply with the warden’s orders, and each guard captain had his own security routine. Staff turnover, a normal 28% in 1970–74, shot up to nearly 80% in 1978–79, just before the riot (Useem and Kimball 1991, p. 91). Similarly, in West Virginia, the new warden Hedrick lost the confidence of his staff, who judged him

“unworthy of respect, unlike his predecessors.” One guard clearly stated that “Hedrick has a very poor relationship with the officers. A majority of them would like to see him go” (Useem and Kimball 1991, p. 178). The riot occurred on New Year’s Day, when a “sick-out” by guards left the facility critically understaffed.

Perhaps the clearest example of guard alienation and divisions occurred at Camp Hill, where a liberalizing regime had just issued new rules for softer discipline and changed the rules for prisoner access to medical care: “Camp Hill before the riot was a house divided. Antagonism between administration and staff is not rare in corrections, but at Camp Hill the division was unusually deep. . . . Camp Hill had not one but two organizational cultures. Each side believed that its own approach represented good corrections and that the other side’s approach was unwise and unjust” (Useem et al. 1996, pp. 61–62).

Prison officers and staff know that they work under potentially dangerous conditions and that their authority—and their safety—depend on sound routines for handling prisoners. They therefore not only resist changes in routine out of an innate conservatism but out of fear that new routines will place them at risk. Unless prison staff are convinced that a reform of prison administration and routine will work to their benefit, they are inclined to believe that any such changes involve sacrifices of their authority and safety for the sake of the warden’s pet policies or for the (generally undeserved) sake of the inmates. The result is both a withdrawal of loyalty to the top administration and efforts to foot-drag or impede the implementation of change.

The combination of administrative strains—whether imposed by outside pressures or tight budgets, or by the rapid turnover of high officials and efforts at changing prison regimes—with active resistance to the administration by the staff, makes smooth administration of prisons nearly impossible. It is certain that many external and internal administrative pressures are unavoidable. But unless measures are taken to ensure that the warden has the full loyalty and support of his staff, weathering the vulnerability imposed by those pressures can be an overwhelming task.

Grievances among the Prison Inmates about the Warden’s or the Staff’s Actions

Almost all prison riots are preceded by some change in conditions or privileges that prisoners construe as a grievance. This does not mean that *any* deterioration of conditions is so viewed. Prison inmates make their own judgments of what deprivations represent unfair impositions and thus are the subjects of grievances.

In Attica in 1970–71, inmates felt aggrieved at efforts by guards to

counter Commissioner Oswald's clear intent to increase prisoners' privileges. Prison officers harassed inmates who filed civil rights lawsuits, censored books and magazines received by inmates, and denied Black Muslims religious services. Similarly, prison conditions in Michigan in 1952 were not particularly bad; in fact, according to one account, they were "considered to be among the best in the nation" (Fox 1956, p. 74). Still, inmate grievances emerged as a reforming commissioner of corrections sought to improve those conditions, but these efforts were met by the resistance of the warden and his custodial staff (Bannan 1952).

In West Virginia and New Mexico prior to the riots, work and education programs for inmates were drastically cut, and visiting and personal property privileges were curtailed. Worst of all, in New Mexico the administration initiated a "snitch" system to encourage prisoners to spy on each other and report to the guards.

In Michigan 1981, prisoners were outraged at a new state law that simply took away their "good time" toward early release and flatly denied any future early parole. At Camp Hill, prisoners faced new restrictions on their access to medical care, which even many in the correctional staff believed to be unfair deprivation. At Mack Alford, Atlanta Federal Penitentiary, and the Joliet prison, inmates were aggrieved over efforts to transfer inmates to other facilities for reasons considered to be ill-considered or wholly unjust (Useem and Kimball 1991, pp. 70–72, 90, 117–18, 177; Useem et al. 1996, pp. 22, 62, 84–86).

Of course, while such grievances give prisoners reasons to challenge the actions and authority of guards, they usually will not have either the opportunity or inclination to make such challenges if they respect the warden and if the prison is efficiently and fairly run. It is only if the first two conditions are met, such that prisoners believe—or know—the administration is under fire, the administration and the guards are at odds, and there are likely to be lapses in security and some support for their goals, that prisoners are likely to believe a riot will be an effective mode of protest.

Moreover, it is crucial that the prisoners not only find some actions by the prison staff or warden undesirable but consider those actions to be part of a broader scheme of unjustified or illegitimate changes brought by the administration. It is thus particularly when prison administrations change their top personnel, and seek to implement new policies, that small grievances become part of a broader picture of inconsistency and ill-understood changes and prisons become more vulnerable to riots (DiIulio 1991a). As Useem and Kimball (1991, p. 205) argue: "Uniformity and stability also contribute to the presumption of legitimacy; change and disorder weaken it; and arbitrariness and chaos shatter it. . . . When things are made worse, there had better be a cause—a decline in resources, a mass escape . . . ; otherwise the new level of deprivation will be taken as

illegitimate. Even when things are made better, there had better be a cause—otherwise, yesterday's rule was illegitimate; and if the rule was illegitimate yesterday, mightn't it be so again today?"

Oahu appears to be an exception. McCleery (1968, p. 138) reports all efforts to find "plausible grievances" behind the riot failed. Nevertheless, there was a shared ideology among inmates that the prison was not fulfilling its mission, as will be discussed in the next section.

The Spread of Ideologies of Protest or Rebellion

Ideologies of protest or rebellion may be heterodox or conventional, religious or secular, radical or conservative, but they become a potent force when they undercut the legitimacy of the prison and give inmates a belief that a riot is justified. Useem and Kimball (1991, pp. 205–7) name four ideologies that the prisoners they observed used to justify rebellions. These are rationalism (the belief that the prison is being badly run and a riot will call attention to these faults and bring improvements), constitutionalism (the belief that the prison is being run unconstitutionally and a riot will call attention to that failure and bring improvements), rehabilitationism (the belief that prisoners have a right to opportunities to improve themselves and that failure to provide such opportunities, or their reduction or withdrawal, requires protest), and revolutionism (the belief that prisoners are unfairly imprisoned because of their race or poverty or other illegitimate reason and that a riot will demonstrate their just cause to those outside the walls). We add a fifth ideology here: Hobbism, that is, the belief that there is insufficient authority in the prison, such that there is an unchecked competition of self-interested actors. Under Hobbism, rebellion is not so much an effort to overthrow authority or alter its practices but a demand for more of it or, at least, for less of the consequences of its absence.

Revolutionism, the most extreme of these ideologies, is actually rare. Most prisoners, however they may feel about their own incarceration, generally believe that their fellow prisoners are criminals and require supervision and a well-administered prison for the sake of all inmates' safety. Rationalism, constitutionalism, rehabilitationism, and Hobbism are thus far more common. Among the cases of prison riots examined here, only in Attica and in Atlanta, and to a lesser degree in Joliet, was revolutionism a factor. In all of these cases, there was a strong racial or ethnic divide between the inmate population and the prison staff and administration, which fed inmates' belief that their imprisonment was the result of being treated with different standards than prevailed for white society. In West Virginia and New Mexico, where courts had declared the prisons in violation of the law, constitutionalism was especially strong. In Michigan, ratio-

nalism was strongest, as prisoners had been told that conditions did not live up to rational standards, but no changes were implemented. Rehabilitationism was also expressed at West Virginia, Michigan, and Lucasville, where budget and program cutbacks led to longer periods of inmate idleness.

Hobbiism was prevalent at both New Mexico and Oahu. At the former, inmates preyed upon each other in a genuine war of all against all. At the latter, the ideology tying inmates together was that no one was effectively in charge of the prison. "When that demand to know where authority lay was answered by the National Guard, the answer clearly implied that it no longer lay with the career officials of the institution" (McCleery 1968, pp. 138–39).

All of these ideologies have a core belief that, by some standard held by the broader society, the prison administration is no longer acting legitimately and has forfeited its right to respect and obedience. According to the summary of findings by Useem et al. (1996, p. 55), "When inmates' hopes are raised and then dashed, when inmates' sense not merely of deprivation but also injustice is high, when policy makers outside the prison are seen as potentially sympathetic to the inmates' cause, then riot is in the air."

In sum, what matters is not prisoners expressing personal grudges against guards or other prisoners or vague complaints about prison conditions being poor; such feelings seem to exist in most prisons. Rather, riots tend to occur only when prisoners share a coherent view or ideology about what is "wrong" or "bad" about the prison. Sharing a coherent ideology, large numbers of inmates come to be united against the administration, carrying with them a sense that they were fighting for something they deserved—either recognizing the illegitimacy of imprisonment (revolutionism in Attica and Atlanta), the need for authorities to comply with court orders or executive promises (constitutionalism), the need for opportunities for self-improvement (rehabilitationism), the need to rein in unfair or corrupt use of power by the staff or administration (rationalism), or the need to restore staff/administration authority in a chaotic prison (Hobbiism).

Warden or Staff Actions Taken in Response to Expressions of Grievances

According to Useem and Kimball (1991), in what they call a period of "preparation," prisoners seeking change try various means to influence prison authorities or see if they can circumvent prison rules. Only when the prison administration's response is seen either as unjust or ineffective does inmate opposition increase and lead to prison riots. They note that

"common to nearly all of the pre-riot prisons we studied was a period during which the tempo of conflict escalated and the rebellious side won some sort of battle demonstrating the vulnerability of its opponents" (Useem and Kimball 1991, p. 209). This helps solve the assurance problem, as described above.

At Attica prison, the population already included inmates who had engaged in peaceful protests at other New York prisons, and were promised good treatment, but who had been transferred to Attica as a reprisal for their acts. Then, immediately prior to the riot, there was a major incident in which a prisoner struck a guard, and the authorities responded in a confused fashion, taking no immediate action and then punishing the wrong man (Useem and Kimball 1991, pp. 25–26). Together, these actions showed that the administration was both fiercely unjust and confused.

At Camp Hill, prior to the riot there was a fight between prisoners and staff. The warden meted out unusually wide-ranging and severe punishments to the prison staff, including several high-ranking officers. These actions destroyed staff morale; the guards started to see the warden as being on the prisoners' side (Useem et al. 1996, pp. 61–62). At the Penitentiary of New Mexico, a nonviolent work strike protesting cuts in the work program was met by tear gas and abuse by guards. Lawsuits condemning conditions in the prison were won, but the prison administration was slow to implement any changes (Useem and Kimball 1991, pp. 96–99). It was thus clear to the inmates that "legitimate" efforts to remedy conditions were going to evoke no useful response. A mass escape by 11 New Mexico inmates demonstrated that collective action could be successful.

At Michigan in 1981, mutinous guards ordered a lockdown of the prison that inmates heard was against the warden's orders. At West Virginia, guard dissension led to high absenteeism (a "sick-out") on January 1; the prison administration did not respond by locking down the facility but continued to operate normally despite a critical shortage of guards on duty. In both of these cases, the administration's response to guard actions signaled that the prison was not being well-run and was "open" for challenge.

And in Atlanta, Joliet, and Mack Alford, the prison administrators bungled efforts to transfer protesting prisoners to other facilities. They either gave advance notice of the impending transfer (Joliet), or were caught in a lie about it when media publicized their plans (Atlanta), or attempted to separate the prisoners openly from a larger group (Mack Alford). In each of these cases, prisoner opposition to the transfers became the flash point for a riot. Finally, at Lucasville, the day before the riot started, an inmate Muslim leader was involved in two incidents that would, by normal standards, have led to disciplinary action but, in this situation, did not. The inmate dropped a broom from a second floor tier and struck an officer and, in a separate incident, threatened officers and a chaplain. He

was not disciplined and the next day became one of the key leaders of the riot (OCSEA 1993, pp. 9–10, 59).

Two patterns seem common to these events. One is the mishandling of disciplinary efforts and the other is to treat challenging inmates as if they were simply pawns to be moved and disciplined at will, without explanation or rationale. The first emboldens inmates; the latter enrages them. For example, three days before the Lucasville riot, the warden sent a memorandum to an inmate leader telling him of his unwavering intent to proceed with the TB testing: "I expect full compliance to my orders for all inmates to be tested. You are in *no* position to dictate to me how you perceive this should occur" (OCSEA 1993, p. 60). No effort is made in the memo to explain that the warden had received a letter from two Islamic organizations, both stating that Islam does not prohibit TB skin tests. This, it would seem, would have helped diffuse inmate anger.

Public figures in many states issued ringing condemnations of prison conditions without regard for the effect these pronouncements would have on prisoners' own feelings about their situation. And prison administrators in some cases acted to transfer prisoners or enact disciplinary measures without regard to possible inmate responses. One can only be struck by the similarity to Tocqueville's description of the relationship between French elites and the French population immediately before the revolution:

When at long last the authorities began to take an interest in the masses, they talked about them in their presence, as if they were not there. Indeed, there seems to have been an impression that only the upper classes could use their ears. . . . The very men who had most to fear from the anger of the masses had no qualms about publicly condemning the gross injustice with which [the masses had] always been treated. They drew attention to the monstrous vices of the institutions which pressed most heavily on the . . . people [and thus] made them acutely conscious of their wrongs. The people of whom I now am speaking [are] members of the government, high officials. (Tocqueville 1955, p. 180)

In sum, prison riots generally do not come out of the blue, catching everyone unaware. They occur after a period in which grievances—usually among staff and prisoners—are known and have been tentatively expressed. It is the failure to take such grievances seriously; or, conversely, an overreaction; or, in some cases, public endorsement of the grievances and promises of remedy that is not forthcoming that inflame a situation and create the resolve to riot.

FORMAL DATA ANALYSIS

A summary of the riot cases and of conditions immediately prior to the riot (time *t*) and during a more stable period some years before the riot

($t - N$) is given in table 1. Although we have generally tried to look four or five years back from the time of the riots to get at such conditions, we are limited by the coverage in the secondary sources, which varies in its degree of detail in describing the prisons in question some years before the events under investigation. N thus varies from one to five years. We will examine some of the implications of this below.

For every case where the condition in question was detected at a "medium" or "high" level, we list the operational indicator we used as evidence of that condition. A full list of these operational indicators for each condition is given in the notes to table 1. Generally, the "level" of a condition is determined by the number of indicators active for that condition; however, where the secondary sources stressed extreme conditions on one indicator, or rather mild levels on two, we adjusted the overall imputed level of the condition accordingly. However, the main differences in the table are between prisons and times when *none* of the operational indicators of a condition can be identified—hence a "low" level on that condition—and cases where some indicators are clearly present.

As shown in table 1, of the five conditions hypothesized to produce prison riots, eight out of 13 cases had all conditions present. Of the five remaining cases, three cases had four conditions present, and two cases had three conditions present. Thus in no case did a riot occur without at least three of the five conditions observed, and in the majority of cases all five conditions were present. Among the five conditions, the key element of the state-centered view, administrative erosion or ineffectiveness, was detected in *every* case. So too was the spread of ideologies of protest or rebellion. Staff grievances or resistance was present in all but two cases, prisoner grievances were present in all but one case, and initiating actions by the administration or staff were present in all but four cases. A clear initiating event is obviously one of the more contingent elements of a riot; as noted, simple carelessness that leaves a gate unlocked or a guard exposed to prisoners can lead to a riot if other conditions favorable to a riot are present.

Conversely, looking at the same prisons in prior periods of stability, all five conditions were absent in 10 of the cases, four conditions were absent in two, and three conditions were absent in one. Thus, as above, every stable case showed an absence of at least three of the five conditions, and most commonly all five were absent. Of the five conditions, administrative erosion and ideologies of rebellion appear in only one case each; staff grievances and initiating events were absent in all cases, and prisoner grievances were absent in all but two cases.

In sum, the "riot" and "stable" periods show a sharp asymmetry on the five key variables. Not all conditions were present for every riot, and not all conditions were absent for every stable case; thus these conditions or

their absence are not "necessary and sufficient" in a strict, deterministic sense. However, it seems clear that these conditions are strongly linked to the probability of riot or stability. If all five conditions are present, riot appears a certainty. Yet if even three of the five are absent, riots do not occur. In between these extremes, where three or four of the conditions are present, riots also seem likely and clearly have occurred.

We can examine the statistical significance of this asymmetry by taking a dynamic approach and using the matched-pairs signs test to look at the *differences* between prisons on the edge of a riot and the same prisons in prior years when riots did not occur. This nonparametric test (Conover 1980; Siegel 1988) considers each pair of t and $t - N$ readings as a single case and examines whether the change in the level of a condition between $t - N$ and t has increased (+), decreased (-), or remained unchanged (0). Assuming as a null hypothesis that differences in either direction are equally likely, the actual number of pluses observed can be compared with the probability of obtaining at least that many pluses by chance from the binomial distribution. In other words, for our 13 cases, if the direction of change in our conditions was unrelated to the incidence of riots, the chances of observing a plus 13 times would be the same as the odds of tossing a coin and having it come up "heads" 13 times in a row. If we observed 11 pluses, the probability of such an outcome occurring by chance would be the same as that of tossing 11 or more heads out of 13 flips of a coin. Thus, with even 13 cases, a statistically significant result can be obtained if there is a strong asymmetry in the direction of change on the key conditions.

The changes in the key conditions are shown and tabulated in table 2. The number of pluses out of 13 cases is shown for each condition at the bottom of the table, along with the single-tailed probability of obtaining these outcomes by chance. For every condition except the last—the presence of initiating actions by the administration or staff—the probability of a positive change between stable and riot conditions is statistically significant at the .05 level or greater. (As noted, an initiating action is least likely to be a "necessary" condition, as mere carelessness will also supply opportunities for a riot to commence). Moreover, in *every case*, we find both that administrative erosion increased and that a total of at least three of the conditions has changed in a positive direction between the riot and stable periods. In eight out of the 13 cases, all five of the conditions show a positive change. It is interesting that an increase in the ideology of rebellion is also present in all cases. This may be a confirmation of theories of revolution that stress the role of ideology (Selbin 1993; Foran 1997); however, given that the secondary data we used was based mainly on postriot interviews with prisoners, it may also simply reflect the need for riot par-

TABLE 2
SIGNS TESTS OF CONDITIONS FOR PRISON RIOTS

CASE	CONDITIONS AND SIGN OF DIFFERENCE					SUM
	1	2	3	4	5	
SPSM (1952)	+	+	+	+	0	4
Oahu, Hawaii (1960) ..	+	+	0	+	0	3
Attica, New York (1971)	+	+	+	+	+	5
Joliet, Illinois (1975)	+	+	+	+	+	5
New Mexico (1980)	+	+	+	+	+	5
SPSM (1981)	+	+	+	+	+	5
West Virginia (1986)	+	+	+	+	+	5
Kirkland, South Carolina (1986)	+	0	+	+	0	3
Atlanta, Georgia (1986)	+	0	0	+	+	3
Coxsackie, New York (1988)	+	+	+	+	0	4
Mack Alford, Oklahoma (1988)	+	+	+	+	+	5
Camp Hill, Pennsylvania (1989)	+	+	+	+	+	5
Lucasville, Ohio (1993)	+	+	+	+	+	5
Pos-0	13-0	11-2	11-2	13-0	9-4	13-0
<i>P</i>000	.01	.01	.000	NS	.000
Pos-0*	10-3	8-5	8-5	10-3	6-7	13-0
<i>P</i> *05	NS	NS	.05	NS	.000
Pos-0†	8-5	7-6	7-6	8-5	6-7	13-0
<i>P</i> †	NS	NS	NS	NS	NS	.000

* Assuming a 20% error rate in "low" coding at $t - N$

† Assuming a 40% error rate in "low" coding at $t - N$.

ticipants to develop some rationalization for their actions—we cannot be sure which is the case.

A major problem with the retrospective coding of the data based on postriot investigations is that our sources pay far more attention to conditions just prior to the riot than to events in prior years. Some of our conditions are easily observed across time—for example, if a rapid turnover in wardens should occur following a long period of administrative stability, then we can clearly code a change in administrative effectiveness on that score. However, other conditions, ranging from staff and prisoner grievances, to provocative actions by staff or administration, to subtle administrative breakdowns, to the spread of rebellious ideologies among prisoners, may simply go unremarked if they occurred in isolation some years before a riot occurred. There is thus a strong bias in our data toward concealing the presence of the key conditions in the "stable" cases, or in other words of recording false "low" values for the $t - N$ cases.

We have attempted to compensate for this bias by repeating the analysis on the assumption of error rates of 20% and of 40% in coding the values of each condition for the stable cases. That is, we have tried to see what difference it would make if in three out of 13, or even five out of 13 cases, we have mistakenly overlooked the presence of key conditions during a stable period because it was not reported in the secondary data. These results are also shown at the bottom of table 2.

With a 20% putative error rate, the data for administrative erosion and ideologies of rebellion would still be significant at the .05 level, although the other conditions would lose statistical significance. If the error rate was as high as 40%, all the statistical significance for the individual conditions, taken separately, would disappear. However, the *combined* effect of the five conditions would remain significant. This is because in every case, at least three out of five conditions show a positive change from the stable to riot periods. Thus, assuming our errors are randomly distributed across our cases, even if for every case we had miscoded two out of five factors (40%), in no case would such an error be sufficient to create a net zero or negative sum of changes. Rather, in 13 out of 13 cases, we would still find that summing the pluses, zeros, and minuses for the five conditions showed a net positive difference between the stable and riot years. Thus, while we recognize the problems inherent in a retrospective design, we believe our results quite robust even in the face of considerable imputed errors in our coding.

In considering our statistical analysis, the reader might further object that we only examined prisons that had riots and hence have selected on the dependent variable. This could potentially lead to two kinds of errors. First, it might be that the key conditions were present in the riot cases at times other than at the time of riots (t). We believe our retrospective analysis, looking for *differences* between conditions at t and $t - N$, specifically addresses this possibility.

The second possible error is that our riot conditions were equally prevalent in average nonriotous prisons that we did not observe—possible but highly improbable. For this to occur, the (unobserved) nonriotous prisons would have to more closely resemble our riot cases at time t (key conditions present and riots occur) than they resembled our cases at time $t - N$ (key conditions absent). This would mean that our cases of prison riots occurred not because normal prisons deteriorated in multiple ways but because our $t - N$ cases were in many ways abnormally virtuous and became riotous because they descended to the condition of “normal” prisons. There is a great deal of evidence, both in the cases we have and in other accounts of prison life, that our prisons at time $t - N$ were not exceptionally virtuous, becoming more “normal” at time t .

True, there were some cases of abnormal virtue in our $t - N$ cases. In

Oklahoma, in West Virginia, and in Oahu at $t - N$, the prisons were administered by truly outstanding wardens, who did an exceptional job of coping with adverse financial or other conditions. But for the most part, prisoners interviewed after the riots—many of whom had been transferred and had experienced conditions in other prisons—did not at all claim that their experience in the years before the riot had been outstanding or exceptionally good and that conditions closer to the riots were more like those they had experienced at other prisons. Quite the reverse—the typical testimony from staff and inmates alike is that conditions at $t - N$ were not great, usually no better than at other prisons and sometimes even worse. What changed at time t were new conditions that clearly made things worse than staff or inmates expected for normal prison life—understaffing, abnormally high administrative or staff turnover, reduction of inmate privileges, statements from prison or outside authorities that conditions were unconstitutionally or illegally bad, broken promises for mitigation, new harsher rules for inmates, new restrictions on staff, or a breakdown in prison order or routines.

Thus we believe the evidence shows—and that other scholars reading the postriot investigations in conjunction with other literature on prison life would agree—that the conditions leading to riots in our cases were not only “bad,” relative to the recent history of those prisons, but were “bad” relative to broad staff/inmate expectations about prison life in general. We therefore believe that any argument that we have erred by missing the fact that “normal” prisons were really more like our cases at time t , and that our cases at time $t - N$ were exceptionally virtuous, must be rejected.

In sum, the formal data analysis reveals a statistically significant relationship between four of the key conditions, taken individually, and prison riots. The relationship is even stronger for *combinations* of these variables, which complements the conjunctural analysis featured in the state-centered theories of revolution. Elsewhere (Skocpol 1979; Goldstone 1991), it is stressed that some combination of critical factors is necessary to produce a revolution. In our prison riot data, we find that a combination of three or more factors was present in all cases of riots and that the absence of three or more factors was observed in all cases of stability.

CONCLUSION

The state-centered theory of revolutions appears to generalize usefully to the prison. This theory does a better job of explaining the incidence and causes of a range of major prison riots than either the functionalist or rising expectations theories. Moreover, it explains phenomena that cannot be explained at all by prior theories—such as why prison riots can be

spurred even by attempts to *improve* prison conditions and why prison riots often occur in waves.

In regard to waves of prison riots, the underlying dynamic appears similar to waves of revolution (Goldstone 1991; Katz 1997). Just as states are enmeshed in an international states system, so prisons are enmeshed in broader political and social frameworks that can impinge jointly on many prisons, helping to spread prison riots. These include national court decisions that impose new administrative standards on prisons, national economic conditions that impose fiscal austerity on prison budgets, and national political and judicial campaigns for tighter sentencing that require (but rarely provide) greater prison resources or that extend new rights to prisoners and encourage liberalization of prison regimes. All such actions can impose changing population/resource balances and new administrative/fiscal burdens on prison administrations or can create conflicts between wardens and correctional staff, throughout a state, a region, or the country as a whole.

The reason that prison riots often come just at the time that prison officials are seeking to improve prisoners' conditions is similar to the reason that reforms often precede revolutions. Authorities are aware that conditions need rectification. Yet their desire to effect change falls short because of elite resistance, resource constraints, or poorly managed implementation. At the same time, their loudly expressed belief that change is needed has the effect of undermining the illegitimacy of existing arrangements, while increased conflicts with elites and poorly implemented reforms weaken the administrative structure. Whether at the level of the national state or at the level of a penal institution, such a combination sharply increases the risks of revolt.

The mirroring of basic structures and their dynamics of stability and revolt at these widely disparate levels of social organization suggests the value of a fractal approach to understanding social organization. Indeed, the remarkable number of ways in which the conditions of social stability in large monarchical or imperial societies in revolution parallels the conditions of social stability in prisons suggests that the main principles of state-centered theories of stability and breakdown may apply generally to any hierarchical, absolutist-type social organization, whether it operates on the scale of millions, or merely hundreds, of individuals. States and prisons are coercive and not collegial or democratic institutions; we expect that the latter might show rather different behaviors. But we suggest that it is worth further investigation to examine whether in other kinds of hierarchical, authority-based settings—such as military organizations, schools, or business firms—the stability and efficacy of the institution is severely disrupted if it encounters a combination of these five key factors: administrative erosion due to external demands or fiscal weakness, elite dissension

and conflicts, grievances among the "base" population of the institution, the spread of heterodox ideologies, and incidents that demonstrate injustice or ineffectiveness on the part of institutional officials.

A hallmark of good theory is that it can usefully be extended to phenomena not anticipated in the original development of the theory. The applicability of the state-centered theory of revolutions—developed to account for the behavior of early modern monarchies and empires—to 20th century prison regimes suggests that this is not just a moderately generalizable historical model but a true, and robust, instance of sociological theory.

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How Unregulated Is the U.S. Labor Market? The Penal System as a Labor Market Institution¹

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Comparative research contrasts the corporatist welfare states of Europe with the unregulated U.S. labor market to explain low rates of U.S. unemployment in the 1980s and 1990s. In contrast, this article argues that the U.S. state made a large and coercive intervention into the labor market through the expansion of the penal system. The impact of incarceration on unemployment has two conflicting dynamics. In the short run, U.S. incarceration lowers conventional unemployment measures by removing able-bodied, working-age men from labor force counts. In the long run, social survey data show that incarceration raises unemployment by reducing the job prospects of ex-convicts. Strong U.S. employment performance in the 1980s and 1990s has thus depended in part on a high and increasing incarceration rate.

Institutional analysis of labor markets typically focuses on the effects of social policy and industrial relations (Crouch 1985; Colbjørnsen and Kalleberg 1988; Korpi 1990; Kolberg and Esping-Andersen 1990; Hicks 1994; Janoski, McGill, and Tinsley 1997). For this research, the United States provides a model of market deregulation. U.S. unions are weak, and the welfare state affects only those at the fringes of the job market.

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This contrasts with Europe, where employment relations are highly regulated. Unions set wages for entire economies, and welfare states significantly influence the supply and demand for labor. These institutional differences acquired special importance over the last two decades as European unemployment rose in comparison to U.S. unemployment. Recent trends are striking. While unemployment in the European Union averaged 9.5% between 1990 and 1993, the U.S. average was only 6.5% (OECD 1996). Currently, unemployment hovers around 10% in Germany, Italy, and France, while U.S. unemployment averaged less than 6% between 1994 and 1996.

These trends suggest unregulated labor markets yield strong employment performance. Of all the labor markets of the advanced economies, the United States best approximates the competitive model of neoclassical theory. In this model, job seeking is intensified by meager state support for the unemployed, and low unionization allows wages to adjust to market conditions. In Europe, institutions introduce inefficiency: large welfare states and strong unions stifle labor demand and reduce work incentives (Olson 1982; Lindbeck 1985; Giersch 1993; OECD 1994a).

We challenge this analysis by arguing that labor markets are embedded in a wide array of social arrangements that extend beyond the welfare state or industrial relations. In the United States, criminal justice policy provides a significant state intervention with profound effects on employment trends. The magnitude of state intervention is reflected in budget and incarceration figures. In the early 1990s, \$91 billion were spent on courts, police, and prisons, dwarfing the \$41 billion spent on all unemployment benefits and employment related services (*Statistical Abstract of the United States* 1995, table 585). By 1996, 1.63 million people were being detained in American prisons and jails—a threefold increase from 1980 (Gilliard and Beck 1997, p. 1). These figures suggest that incarceration generated a sizeable, nonmarket reallocation of labor, overshadowing state intervention through social policy.

This article studies the penal system as a labor market institution and provides evidence for its dynamic effects. Our central argument is that U.S. incarceration lowers conventional measures of unemployment in the short run by concealing joblessness among able-bodied, working-age men, but it raises unemployment in the long run by damaging the job prospects of ex-convicts after release. Incarceration, unlike social welfare policy, deepens inequality because its effects are increasingly detrimental for young black and unskilled men, whose incarceration rates are highest and whose market power is weak. This argument suggests that incarceration has lowered the U.S. unemployment rate, but it also implies that sustained low unemployment in the future will depend on continuing expansion of the penal system.

This dynamic argument differs from conventional analysis. Typically, covariation between institutional conditions and labor market outcomes provides evidence of institutional effects (e.g., Crouch 1985; Hicks 1994; Janoski et al. 1997). Incarceration resists this approach because of the conflicting direction of its short- and long-run effects. Focusing on just the short-run reduction in the labor supply through imprisonment or the long-run rise in unemployment risk among ex-convicts provides an incomplete picture of how incarceration effects unfold over time. The short-run analysis—highlighting low unemployment in the United States—neglects the significant threat of unemployment in the future. The long-run analysis—emphasizing the high unemployment risk of ex-convicts—appears anomalous in light of the low rate of U.S. unemployment in the mid-1990s. Understanding how incarceration shapes labor market outcomes thus requires consideration of both kinds of effects.

While speaking to the comparative analysis of labor markets, this argument also shares concerns with earlier research in stratification and criminology. Like stratification research on segmented labor markets, we also identify institutional bases of persistent inequality (cf. Doeringer and Piore 1971). While earlier work focused on the structure of firms and the job hierarchy, however, we underline the role of noneconomic institutions. We also view incarceration as a kind of hidden joblessness, similar to an old tradition in Marxist criminology (cf. Jancovic 1977). Unlike that work, hidden unemployment here is viewed as a consequence of incarceration, not a functional necessity of capitalism. Instead of recruiting replacement workers to the reserve army of labor, we also claim that incarceration tightens labor markets in the short run and makes workers more unemployable in the long run.

Our dynamic approach motivates a two-part analysis with two distinct research designs. Following a brief comparison of labor market institutions, we examine the short-run effect of incarceration by including the inmate population in estimates of U.S. and European labor inactivity. After showing that incarceration effects are negligible in Europe but large in the United States, we focus on U.S. social survey data to study the long-run effect of incarceration on the employment of ex-convicts. A discussion summarizes the results and compares the penal system to the welfare state as a labor market intervention.

THE INSTITUTIONAL CONTEXT OF THE U.S. LABOR MARKET

Industrial Relations and the Welfare State

It is often observed that the United States lags behind Western Europe in industrial relations and welfare state development (Goldfield 1987; Gourevitch 1986; Freeman 1995). The weakness of U.S. industrial rela-

TABLE 1

SELECTED INDUSTRIAL RELATIONS AND SOCIAL POLICY CHARACTERISTICS
OF 17 OECD COUNTRIES

Countries	Private-Sector Union Density	Collective Bargaining Coverage	Total Social Spending	Unemployment Benefit Coverage	Active Labor Market Spending
Australia	32	80	13	82	34
Austria	41	98	25	132	.30
Belgium	90	25	148	1.04
Canada	28	38	19	129	.68
Denmark	72	.	28	113	1.56
Finland	65	95	27	112	1.76
France	8	92	27	98	.88
Germany	30	90	23	89	1.64
Italy	32	...	25
Japan	23	23	12	36	.13
The Netherlands	20	71	29	105	1.12
New Zealand	42	67	19	92	.74
Norway	41	75	29	61	1.14
Switzerland	22	53	...	53	.27
Sweden	81	83	33	93	3.21
United Kingdom	38	47	24	71	.59
United States	13	18	15	34	.25
Average excluding the United States	38	72	24	94	1.03

NOTE — Union density is written as a percentage of all private-sector employees. Data are for 1988, except for Canada, the Netherlands, and Switzerland (measured in 1985), Finland and the United Kingdom (1989) and New Zealand (1990) (Visser 1991, p. 113). Collective bargaining coverage is written as a percentage of all employees. Data are 1990 except for France (1985), Germany (1992), and Japan (1989) (Traxler 1994, p. 173). Total social spending is measured as a percentage of GDP. All data are for 1990 (OECD 1994b, pp. 59–60). Unemployment benefit coverage measures unemployment beneficiaries as a percentage of unemployed recorded in labor force surveys. Data are for 1990–91 except for Denmark (1992) and Sweden (1992) (OECD 1994a, p. 188). Active labor market spending includes public spending on training, employment services, youth employment measures, and subsidized employment expressed as a percentage of GDP. Data are for 1990–92 (OECD 1993, pp. 73–78).

tions is illustrated by unionization and collective bargaining coverage statistics (table 1). Unionization is lower in the United States than in most other OECD countries. Although union density has recently fallen in Europe (Western 1997), the terms of collective agreements are often extended to nonunion workplaces (Traxler 1994). Consequently, most workers enjoy union wages and conditions. In the United States, collective bargaining covers only certified workplaces, and employment is mostly governed by individual contracts between workers and their employers.

Market relations are also strongly imprinted on social welfare institutions in the United States. U.S. welfare spending is much lower than wel-

fare spending in Europe. Approximately one-quarter of gross domestic product (GDP) is devoted to social welfare in the large European countries, while U.S. social spending accounts for only 15% of GDP (table 1, col. 3). U.S. unemployment insurance is also patterned by market conditions, and the coverage of unemployment insurance is low by comparative standards (table 1, col. 4). Beyond passive measures for income support, active labor market policies that mobilize workers into jobs are poorly funded in the United States (table 1, col. 5).

U.S. institutional exceptionalism supports the claim that extensive labor market regulation in Europe fueled unemployment in the 1980s and 1990s. The economic burden imposed by European unions and welfare states was a strong theme in theories of "Euroclerosis." In this account, large unions prevent wage cuts in response to declining demand. Unions also limit employers' control over hiring and firing, which obstructs adjustment to shifting market conditions. Generous welfare benefits can raise unemployment by reducing incentives for work (Giersch 1993; p. 151; Lindbeck 1985; Layard, Nickell, and Jackman 1991, pp. 55–56). In the United States, Olson (1982) developed a similar argument that concluded that "there is no substitute for a more open and competitive environment" to achieve strong employment performance (p. 233). More recently, the OECD (1994a) has endorsed the importance of labor market flexibility and deregulation as a solution to Europe's persistent unemployment problem.

Sociologists who favor institutional rather than market explanations of unemployment often agree that the United States offers a model of labor market deregulation (Crouch 1985; Korpi 1990; Hicks 1994; although, cf. Janoski 1990). In these studies, the United States consistently scores low on measures of leftist party power, union centralization, and union organization. The U.S. place as an institutional laggard was formalized in the corporatist theory of unemployment. For this theory, highly centralized labor markets may produce low unemployment through union-bargained wage restraint, but the same outcome is delivered in the United States by the unregulated forces of supply and demand (Calmfors and Drifill 1988).

Although welfare and industrial relations statistics illustrate the weakness of social protection mechanisms in the United States, this does not justify the claim that market principles alone drive the superior U.S. employment record. We next consider state intervention in labor allocation through the criminal justice system.

Criminal Justice and Incarceration

According to the Bureau of Justice Statistics (BJS), between 1980 and 1996, the number of people imprisoned in the United States grew by 300%,

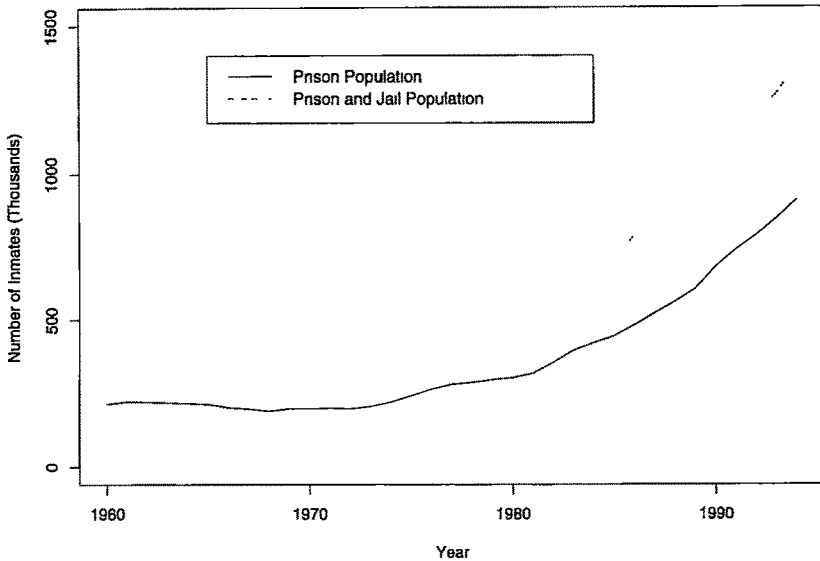


FIG. 1.—Growth in the U.S. prison and jail population, 1960–95

from 500,000 to over 1.6 million. Rates of incarceration began to increase in the early 1970s, but the most rapid growth took place in the 1980s and early 1990s. Federal and state prisons now house over 1 million prisoners, up from approximately 250,000 in 1970 and 266,000 in 1980; the jail population expanded from about 183,000 persons in 1980 to nearly 500,000 in 1994 (BJS 1997, p. 502). These trends are shown in figure 1, which reports a time series of the federal and state prison population and reports the total inmate population, which also includes those in local jails. The take-off of incarceration in the late 1970s is clearly indicated.

Incarceration is spread unevenly across the adult population. While increasing numbers of women were imprisoned in the 1980s and 1990s, men continue to make up more than 90% of all inmates. Incarceration is also concentrated among the young and the less educated. For example, 68% of all state prisoners in 1991 were under 35, and 65% did not complete high school (BJS 1992, p. 3). The dramatic expansion of the criminal justice system has also had a pronounced impact on young African-Americans. Blacks accounted for 22% of all of those admitted to prison in 1930. In 1992, 51% of the prison population was black. By 1995, one out of three black male youths was under some form of state supervision, and nearly 7% of all black male adults were incarcerated (BJS 1995a).

The allocative impact of incarceration on the U.S. labor market is clear

TABLE 2

NUMBERS OF INMATES AND INCARCERATION RATES PER 100,000 ADULT
POPULATION FOR SELECTED OECD COUNTRIES, 1992-93

Countries	Incarceration Rate per 100,000	N of Inmates
Australia	91	15,895
Austria	88	6,913
Belgium	71	7,116
Canada	116	30,659
Denmark	66	3,406
France	84	51,457
Germany	80	64,029
Italy	80	46,152
Japan	36	45,183
The Netherlands	49	7,935
Sweden	69	5,668
Switzerland	85	5,751
United Kingdom	93	60,676
United States	519	1,339,695
U.S. blacks	1,947	626,207
U.S. whites	306	658,233
Average excluding the United States	78	26,988

SOURCE —Mauer (1994) and Beck and Gilliard (1997)

in comparison to other industrialized democracies. The relative size of the prison population is typically measured by the incarceration rate—the number incarcerated on a single day per 100,000 of the adult population. In 1993, the U.S. incarceration rate was 5 to 10 times greater than the other OECD countries for which data were available (table 2). In Germany, for example, there are only 80 prisoners for every 100,000 adults, while in the United States there are over 500 inmates per 100,000. Cross-national differences in incarceration rates are even larger if we focus just on African-Americans. The incarceration rate among American blacks exceeds European figures by more than 20 times. These high rates correspond to large absolute numbers. (For total male U.S. incarceration counts by race, see appendix table A1.) Prison and jail inmates number in the millions in the United States, while the European prison population is measured in the hundreds of thousands.

It might be objected that the growth of the U.S. penal system is an inevitable response to high or rising crime rates rather than an active policy intervention. According to the National Crime Surveys, however, crime rates have fallen since 1980 (BJS 1996). Although the Uniform

Crime Reports, based on police reports to the FBI, indicate rising crime rates during some of this period, systematic analysis of these and other data suggests that the incidence of crime remained constant through the 1980s (Bogges and Bound 1993; Jencks 1991; O'Brien 1996). From a comparative viewpoint, the International Crime Surveys from 1988, 1991, and 1995 (cited in van Dijk and Mayhew [1992] and Tonry [1995, p. 198]) show that U.S. crime rates are only slightly above average among industrialized countries. As an exception, U.S. murder rates are very high, but homicide convictions account for less than 5% of all prison admissions. This evidence suggests that U.S. incarceration trends are only loosely connected to the level of criminal activity.

Instead, rising incarceration rates appear to result from more aggressive prosecutorial practices, tougher sentencing standards, and intensified criminalization of drug-related activity. As a result, the United States incarcerates proportionately more property and drug offenders, and does so for longer periods, than other industrialized countries (Lynch 1995). These differences are growing as a result of the increasing likelihood of incarceration after arrest (Bogges and Bound 1993; Langan 1991). The war on drugs also boosted the prison population (Beckett 1997; Donziger 1995; Tonry 1995). For example, the percentage of state prison inmates convicted of nonviolent drug offenses jumped from 6% in 1979 to nearly 30% in 1994. While explaining these policies is beyond the scope of this paper, research shows that the war on crime and drugs has involved legislators and court officials in a larger trend of redefining state responses to the symptoms of rising inequality (Beckett 1977; Gans 1995).

In sum, welfare and industrial relations institutions are weak in the United States compared to Europe, but the U.S. state plays a comparatively larger role in labor allocation through prisons and jails. The following section examines unemployment trends in light of recent incarceration figures.

THE SHORT-RUN EFFECT OF INCARCERATION

The performance of national labor markets is commonly summarized by the unemployment rate. This statistic is usually given by:

$$u = \frac{U}{U + E} \times 100,$$

where U is the number of unemployed and E is the number of civilian employees. The OECD provides standardized unemployment rates designed for cross-national comparison. These data are collected with national labor force surveys, where the unemployed are commonly defined as those without paid employment actively seeking work in the month

before the survey (OECD [1987] discusses the comparability of unemployment data).

We present two alternative measures that capture the short-run effect of incarceration. In the short run, incarceration influences the unemployment rate by keeping those with high unemployment risk out of the labor market. This can be understood as the causal effect of incarceration. The penal system also carries hidden unemployment because jobless inmates are not counted in standard labor force accounts. Hidden unemployment can be understood as the accounting effect of incarceration.

The Causal Effect of Incarceration

Incarceration lowers unemployment by institutionalizing many who would otherwise be unemployed. Incarceration thus reduces the labor supply by removing able-bodied, working-age men from the workforce. If the number incarcerated is written, P , we can adjust the usual unemployment rate to estimate the causal effect:

$$u_1 = \frac{U + pP}{U + P + E} \times 100,$$

where p is the proportion of all inmates who would be unemployed if not incarcerated. The statistic u_1 can be interpreted as *the unemployment rate that would be obtained if the incarceration rate was zero*. Although p is not observed, it can be estimated. National surveys of prisons and jails report the proportion of inmates unemployed at time of incarceration. Hypothetical unemployment among inmates at zero incarceration, p , can be estimated by this proportion.

To fix ideas, table 3 reports figures for u_1 for U.S. men in 1995, the latest year for which data are available. To estimate p , the average proportion of unemployed inmates is calculated from the *Survey of Inmates of State Correctional Facilities* 1979, 1986, and 1991 (BJS February 1997c, May 1994, and October 1993), the *Survey of Inmates of Local Jails* 1983 and 1989 (BJS February 1997b and June 1997), and the *Survey of Jail Inmates* (BJS February 1997a). On average, more than a third of male inmates were unemployed at the time of incarceration in these data. Given $p = .36$, the 1995 estimate of u_1 in the United States is 6.2% compared to the conventional rate of 5.6%. Through the incapacitation of unemployed workers from the labor force, incarceration is estimated to have lowered the American male unemployment rate by more than half a percentage point in 1995.

How accurate is this estimate of the causal effect of incarceration on unemployment? Accuracy is affected by the quality of estimates of p —the hypothetical proportion of unemployed inmates. One improvement

TABLE 3

ESTIMATING THE MALE UNEMPLOYMENT RATE AT AN INCARCERATION RATE OF ZERO
FOR THE UNITED STATES, 1995

Symbol	Description	Estimate
U	Number unemployed	3,983
E	Number employed	67,377
P	Number incarcerated	1,467
p	Hypothetical proportion of inmates unemployed at zero incarceration	.36
u	Actual unemployment rate (%)	5.6
u_1	Hypothetical unemployment rate at zero incarceration (%)	6.2
u_2	Unemployment rate including inmates among unemployed (%)	7.5

SOURCE.—Labor force statistics are from OECD (1997), p is estimated using data from BJS (October 1993, May 1994, October 1994, February 1997a, February 1997b, February 1997c, and June 1997)

NOTE.—All numbers are in thousands unless otherwise indicated

might allow p to vary over time, perhaps as a function of the actual unemployment rate. Still, the estimate for p seems neither grossly too large nor too small. As a result, u_1 probably provides an unbiased but noisy estimate of the hypothetical unemployment rate at zero incarceration. Although unbiased, measurement error in u_1 motivates an alternative approach.

The Accounting Effect of Incarceration

Measurement error can be minimized and the short-run effect of incarceration can still be assessed by introducing a broader unemployment concept. In this approach, labor market inactivity among able-bodied, working-age men is understated by the conventional unemployment rate. A more accurate measure of labor market inactivity includes inmates in the jobless count. This yields an alternative measure,

$$u_2 = \frac{U + P}{U + P + E} \times 100.$$

In this case, u_2 describes the *unemployment rate that would be obtained if the definition of unemployed were extended to include those incarcerated*. Similar to other modifications of conventional unemployment statistics, u_2 describes the level of labor underutilization (cf. Clogg 1974, pp. 4–10). From this perspective, incarceration creates hidden unemployment by reducing labor utilization in a way that is not captured by the usual labor market accounts. With a large prison and jail population, the labor market operates with substantially less than the full productive potential

of the whole able-bodied adult population. This loss of productive potential is more accurately captured by the adjusted unemployment statistic, u_2 , than the unemployment rate, u .

Although statistically simpler than u_1 , u_2 is also measured with error. By assuming that all inmates are without jobs, u_2 is upwardly biased because some prisoners are engaged in paid employment. The number of inmates in paid employment is not known with certainty, so bias cannot be estimated accurately. In 1990, administrative records show that around 8% of prisoners worked in prison industries, mostly for public-sector employers. An additional 1%–2% worked in agriculture for consumption outside the penal system. A significantly smaller proportion worked in local jails (BJS 1995*b*, p. 14; Miller 1997). Upward bias in P due to employment in prison industries is thus likely to be no larger than 10%.

Other researchers have also noted this sort of short-run effect of incarceration on unemployment (Jancovic 1977; Rusche and Kirchheimer 1939). Jancovic (1977) studied the idea that prison removes part of the surplus labor pool from the labor market. His research for the period 1926–74 found that the size of the incarcerated population in the United States did not markedly affect the size of the unemployed population. Recent effects may be different, however, because of growth in prison and jail populations over the past 20 years.

Results

The importance of incarceration as a source of hidden unemployment varies by sex and across countries. More than 90% of prison and jail inmates are male in the United States and abroad, so we focus on trends in the labor market conditions of men. From a comparative perspective, the short-run effect of incarceration is tiny in Europe because incarceration rates are low (table 4). In Europe, unemployed males outnumber male prison inmates by between 20:1 and 50:1. In the United States, the ratio of unemployed to incarcerated was less than 3:1 in 1995. The United States and Europe also differ in the relative sizes of the conventional unemployment rate, u , and the adjusted figures that count the incarcerated, u_1 and u_2 . Including prison inmates in the jobless count only changes the unemployment rate by a few tenths of a percentage point in Europe. These effects might be just large enough to register in the labor force surveys that measure unemployment. The small differences between conventional and adjusted figures in Europe contrast strikingly with U.S. data, where prison and jail inmates added 1.9 points to the usual unemployment rate in 1995.

Figure 2, part *a*, compares U.S. unemployment to average European unemployment between 1976 and 1994. In contrast to the stylized facts of

TABLE 4

MALE INCARCERATION AND UNEMPLOYMENT IN THE UNITED STATES AND WESTERN EUROPE, 1995

COUNTRIES	NUMBER UNEMPLOYED (U)	NUMBER INCARCERATED (P)	U/P	UNEMPLOY- MENT RATE (u)	ADJUSTED UNEMPLOY- MENT RATE	
					(u ₁)	(u ₂)
Austria	72	6.5	11.1	3.3	3.3	3.5
Belgium	217	6.8	31.7	9.1	9.2	9.4
Denmark	93	3.3	28.4	6.2	6.3	6.4
Finland	231	2.9	79.0	17.7	17.8	17.9
France	1,370	50.8	26.9	10.1	10.2	10.4
Germany	1,594	63.3	25.2	7.1	7.2	7.4
Ireland	110	2.0	54.5	12.3	12.3	12.5
Italy	1,358	47.1	28.9	9.5	9.6	9.8
The Netherlands	254	8.2	30.9	5.9	6.0	6.1
Norway	61	2.3	26.8	5.3	5.3	5.5
Sweden	190	5.5	34.5	8.4	8.5	8.7
Switzerland	63	5.4	11.8	2.7	2.8	2.9
United Kingdom	1,607	49.5	32.5	10.1	10.2	10.4
United States	3,983	1,466.7	2.7	5.6	6.2	7.5

SOURCE.—Eurostat (1997), OECD (1997)

NOTE.—Data from all countries are for 1995, except Austria (1994), Belgium (1993), and the Netherlands (1994). The unemployed and incarcerated are measured in thousands.

Euroclerosis, U.S. unemployment was higher than the European average through the late 1970s and early 1980s. At the peak of the recession in 1983, average male unemployment was about three points higher than unemployment in Europe. Consistent with claims of superior U.S. labor market performance, however, U.S. unemployment fell substantially from the mid-1980s, while European unemployment drifted upward from 1989. In every year from 1984 onward, U.S. male unemployment was lower than the European average.

U.S. employment performance looks weaker once the size of the prison population is taken into account. The u_1 series suggests that prison and jail has lowered male unemployment by between a half and one percentage point since the late 1980s (fig. 2*b*). The u_2 series that adds all inmates to the unemployment count shows that U.S. labor inactivity never falls below about 7% in the 1980s (fig. 2*c*). By the recession of the early 1990s, the adjusted unemployment rate approached its 1983 high. The modified estimate suggests that unemployment in the economically buoyant period of the mid-1990s is about 8%—higher than any conventional U.S. unemployment rate since the recession of the early 1980s.

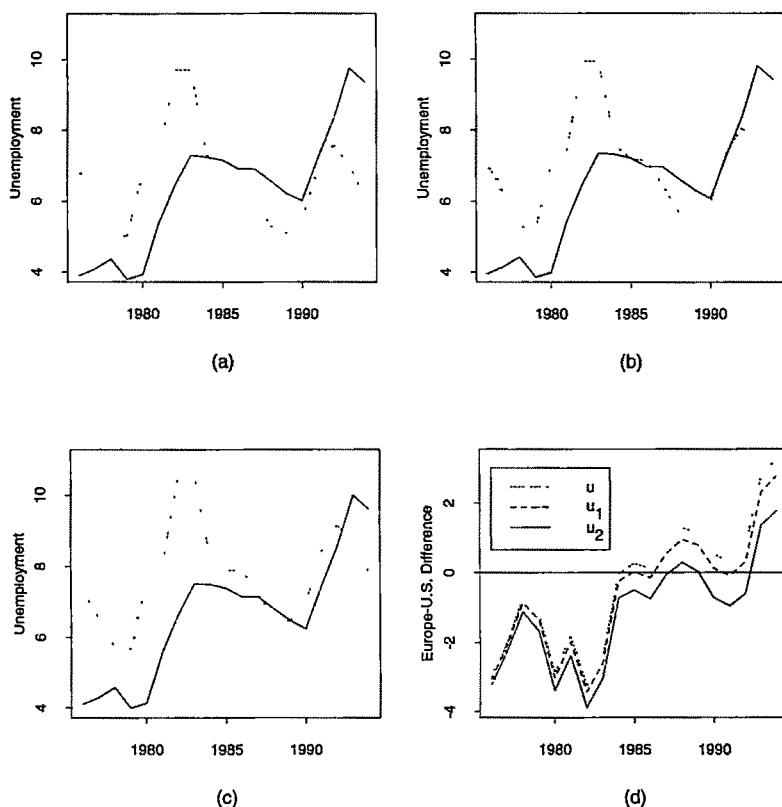


FIG. 2.—Male unemployment rates for the United States (broken line) and Europe (solid line), 1976–94: (a) u , (b) u_1 , (c) u_2 ; (d) the difference between European and U.S. series. The European average is based on the countries listed in table 4. European incarceration data are from Council of Europe (1992) and Eurostat (1997). Missing incarceration data for Switzerland and the Netherlands (1976–83) were imputed.

The relative performance of the U.S. and European economies is highlighted in figure 2*d*, which reports the difference between the European and U.S. series. Positive values indicate the superior performance of the U.S. labor market. According to the usual measure, the United States enjoyed consistently lower unemployment than Europe since the mid-1980s. However, with u_1 , which counts a third of all inmates among the unemployed, less than a percentage point separates the United States and Europe for most of the 1976–94 period. If all inmates are included among the unemployed, u_2 , labor utilization in Europe is higher for 15 of the 19 years from 1976. Adjusted unemployment, u_2 , in the United States generally exceeds European figures, despite strong U.S. economic growth and

TABLE 5
OBSERVED AND ADJUSTED UNEMPLOYMENT AND JOBLESS RATES
FOR U.S. MEN, 1983-95

YEAR	UNEMPLOYMENT			JOBLESSNESS		
	u	u_1	u_2	u	u_1	u_2
All men:						
1983	9.7	9.9	10.6	29.4	29.4	29.9
1985-89	5.5	5.9	6.7	26.2	26.3	27.0
1990-94	5.9	6.5	7.6	27.0	27.2	28.1
Black men						
1983	19.1	20.0	22.9	39.5	39.5	41.7
1985-89	11.6	13.3	16.9	34.0	34.3	37.0
1990-94	11.3	13.6	18.8	34.3	34.6	38.5
White men						
1983	8.6	8.8	9.2	28.3	28.3	28.6
1985-89	4.7	5.0	5.5	25.3	25.3	25.7
1990-94	5.2	5.5	6.2	26.2	26.2	26.8

SOURCE —Labor force data are taken from series supplied by the Bureau of Labor Statistics; incarceration data are from unpublished series of the Bureau of Justice Statistics (see, table A1 below). Data described in table 3 above gave estimates of $p = .36$ and $p = .32$ for black and white men, used to calculate u_1 .

job creation between 1985 and 1994. When taking account of incarceration, European unemployment significantly overtakes U.S. unemployment only from 1993.

A more detailed examination of U.S. data allows us to distinguish the short-run effect of incarceration for black and white men. The three unemployment measures are shown in table 5. These figures are given for 1983—the peak year for U.S. unemployment. Averages for the late 1980s and early 1990s are also reported to assess the effects of economic recovery. In 1983, when the prison population is added to the unemployment count, the adjusted unemployment rate for all men rises by just one point. However, estimates of unemployment among black men in 1983 increase by four points to 23%. The effect of prison on whites is much smaller, raising the unemployment rate by only about half a point. As the prison and jail population grows through the 1980s, the labor market effects of incarceration become much larger. While data for u_1 show that the causal effect of incarceration in the 1990s equals about half a percentage point on the unemployment rate, the effect is five times greater for black men. Data for u_2 show that nearly one African-American man in five is without a job throughout the 1990s when all those incarcerated are counted among the unemployed. For white men, the impact of incarceration on the overall unemployment rate is relatively small.

The current focus on unemployment neglects those of working age who have been discouraged from seeking work. Labor force participation rates may offer a more accurate picture of joblessness or the underutilization of labor. Table 5 also reports overall jobless rates. As before, the adjusted figure adds the incarcerated population to the jobless count. The results here are similar to those for unemployment. The incarcerated population raises the jobless rate, and this effect is largest for black men. Unadjusted participation rates for black men showed labor market conditions in the early 1990s had improved substantially from 1983, when nearly 40% of black men over 20 years old were without jobs. Once the size of the prison population is considered, however, the magnitude of the economic recovery appears considerably smaller. The adjusted measure, u_2 , suggests that joblessness in the 1990s is just under 39% for black men, little improved from its 1983 level. The effect for white men is much smaller, with the incarcerated population adding less than one point to the level of total joblessness.

In sum, the growth of the U.S. penal system through the 1980s and 1990s conceals a high rate of persistent unemployment and joblessness. Adjusted figures that count the incarcerated population as unemployed suggest that the U.S. labor market has performed worse, not better, than Europe for most of the period between 1976 and 1994. These effects are especially large for African-Americans. Once prison inmates are added to the jobless statistics, total joblessness among black men has remained around 40% through recessions and economic recoveries.

THE LONG-RUN EFFECT OF INCARCERATION

While conventional labor force statistics understate joblessness when incarceration rates are high, other research suggests that rising incarceration increases unemployment in the long run. The long-run effect of incarceration highlights the employment experiences of convicts after they are released. Such experiences are difficult to observe in the aggregate statistics of labor market accounts. We thus shift research designs to an analysis of survey data to follow workers as they move from prison to the labor market.

Incarceration is a dramatic, life-changing event that creates a variety of challenges for those who experience it. Ex-convicts face what has been called the "reentry problem": the task of surmounting the psychological, social, and financial consequences of incarceration and reintegrating oneself into mainstream society (Irwin 1970; Eckland-Olson et al. 1983). The ability to find stable, legal work is a crucial component of the reintegration process. Experimental studies suggest that the prospects of job

applicants with no criminal record are far better than those of persons who were convicted and incarcerated (Boshier and Johnson 1974; Buikhuisen and Dijksterhuis 1971; Schwartz and Skolnick 1962). Even those convicted but sentenced to probation instead of prison appear to fare better on the job market (Petersilia and Turner 1986; Freeman 1991; Grogger 1995).

Job prospects for ex-convicts may be even worse in the current context. While convicts who acquire educational and vocational skills in prison are able to improve their chances for employment (Irwin and Austin 1994), resources for education and vocational training within prisons have declined in both absolute and relative terms. The recent decision to deny inmates Pell grants to pursue higher education suggests that this trend is likely to continue in the future. Furthermore, incarceration may erode the value of vocational skills. The increasingly violent and overcrowded state of prisons and jails is likely to produce certain attitudes, mannerisms, and behavioral practices that on "the inside" function to enhance survival but are not compatible with success in the conventional job market (Donziger 1996; Irwin and Austin 1994). Consequently, incarceration may raise unemployment in the long run by increasing joblessness among inmates after release.

Data and Methods

We study the long-run negative effect of incarceration on employment using data from the National Longitudinal Survey of Youth (NLSY; Center for Human Resource Research 1995). The NLSY provides panel data from a national sample of American youth ages 14 to 21 in 1979. Respondents are reinterviewed annually providing information about their labor market experiences in the previous year. Following Freeman's (1991) analysis of incarceration effects in the NLSY, the dependent variable, written E_{it} for respondent i at time t , is measured by the proportion of weeks worked in the past year. This dependent variable is constructed for each respondent, for every year from 1984 to 1993. The analysis is confined to males whose schooling was completed before 1984.

Two key independent variables provide information about the long-run effect of incarceration on employment. First, the 1980 NLSY includes a crime module with questions about involvement in crime and the criminal justice system. Incarceration status is measured by an item that records whether the respondent has ever spent time in a juvenile or adult correctional facility. We use this item to create a dummy variable for youth incarceration, Y_{it} , which scores "1" for respondents incarcerated in or before 1980 and "0" otherwise. Youth incarceration status was only

obtained in 1980, so Y_i varies across respondents but not over time. Freeman (1991) also studied this variable, finding large and enduring effects of youth incarceration on adult earnings and employment status. Despite these strong results, that study was essentially cross-sectional. The possibility of jail spells following youth incarceration was neglected as a rival explanation for current employment status.

We remedy this problem by introducing a second independent variable that measures adult incarceration. The NLSY offers annual data on adult incarceration by recording a correctional facility as a possible residence. Adult correctional residence, written C_{it} , is a dummy variable, scoring "1" if a respondent is currently resident in a correctional facility and "0" otherwise. Adult correctional residence is an imperfect measure of incarceration because time served between interviews is unobserved. Estimated effects are biased to zero with this measurement error. This downward bias is offset by the short-run effect of incarceration: Part of the estimated negative effect of adult incarceration is due to incapacitation from the labor market while incarcerated. As a result, the long-run effect of reduced job opportunities after release is confounded with the short-run effect of joblessness during confinement in prison. To distinguish the short-run and long-run effects, we should ideally control for weeks lost from the labor market while incarcerated. Unfortunately, this information is unavailable in the NLSY.

To help isolate the long-run—post-release—effect of incarceration on employment, we add the lagged variables, C_{it-1} , C_{it-2} , and C_{it-3} . The contemporaneous effect of C_{it} is dominated by incapacitation during imprisonment, but the lagged effects increasingly reflect the postrelease employment experiences of ex-inmates. There are several alternatives to this specification. We could construct a postrelease variable that scored "1" if incarcerated in year $t - 1$, but not in year t . With some loss of information, the dependent variable could also be written as a dummy variable for unemployment at the time of the interview. We experimented with these models and obtained essentially identical results to those reported below. (Details of these analyses are available upon request.)

A final problem with earlier analyses is that unobserved characteristics that place men at risk of unemployment or low wages also raise their chances of criminal conviction. For many studies, causal inferences about conviction neglect the unobserved heterogeneity of offenders (Freeman 1991; Bound and Freeman 1992; cf. Waldfogel 1994). We address unobserved heterogeneity in two ways. Lagged values of the dependent variable adjust for work experience. This way, employment status before and after incarceration are used to assess the impact of jail time. Random effects are also added for each respondent to adjust for unobserved hetero-

geneity. The random effects model fits an additional parameter for every respondent. The model thus accounts for respondent-specific characteristics that are unobserved and not captured by the independent variables. (Random effects models for panel data are described by Hsiao [1986, pp. 32–41].)

Using a pooled cross-sectional time-series design, employment is written as a function of youth incarceration, correctional residence, and work experience. Dummy variables for time periods are introduced to allow for changes in the mean level of employment between 1984 and 1993. The period dummies, $P88_{it}$ and $P91_{it}$, indicate the years 1988–90 and 1991–93. These periods were chosen to break the 10-year time series into three segments of roughly equal length. By interacting the period dummies with youth incarceration, we test whether the impact of juvenile imprisonment decays with time.

The core of the model is written

$$\begin{aligned} E_{it} = & \beta_0 + \rho_1 E_{it-1} + \rho_2 E_{it-2} \\ & + \beta_1 P88_{it} + \beta_2 P91_{it} + \beta_3 Y_i + \beta_4 Y_i P88_{it} + \beta_5 Y_i P91_{it} \\ & + \beta_6 C_{it} + \beta_7 C_{it-1} + \beta_8 C_{it-2} + \beta_9 C_{it-3} + \gamma_i + \epsilon_{it}, \end{aligned}$$

where γ_i is a normally distributed random effect to account for unobserved heterogeneity across respondents, and ϵ_{it} is a normal error term. The core model is supplemented by a set of control variables used in other studies of the labor market effects of arrest and incarceration (Witte and Reid 1980; Freeman 1991; Waldfogel 1994; Sampson and Laub 1993). Control variables include age, education, marital status, urban residence, the local unemployment rate, region, and a number of variables measuring juvenile contact with the criminal justice system (see appendix tables A2 and A3). These variables help operationalize theories of human capital, social control, employment continuity, labor market conditions, and the impact of noncustodial contact with the criminal justice system. The random effects model can be estimated with maximum-likelihood methods. Maximum-likelihood estimates were obtained using the statistical software, S-Plus, version 3.4.

Table 6 reports sample sizes and descriptive statistics for the dependent variable and key independent variables. Like the analysis below, descriptive statistics are reported separately for white and black males. Time in paid employment rises over time as the NLSY cohort becomes older. Increased unemployment in the early 1990s is shown in the final period. The incidence of jail or prison residence also rises over time, especially for black respondents. Consistent with incarceration statistics, youth incarceration is also higher for black respondents than white.

TABLE 6

MEANS OF WORKING TIME, JAIL RESIDENCE, AND YOUTH INCARCERATION VARIABLES,
NLSY MALES, 1984-93

	All Respondents	Blacks	Whites
1984-87			
Working time (<i>E</i>)815 (.293)	.716 (.362)	.862 (.252)
Correctional residence (<i>C</i>)015 (.122)	.038 (.191)	.005 (.071)
1988-90.			
Working time (<i>E</i>)869 (.293)	.782 (.345)	.910 (.219)
Correctional residence (<i>C</i>)021 (.144)	.052 (.222)	.008 (.086)
1991-93			
Working time (<i>E</i>)848 (.293)	.744 (.377)	.896 (.244)
Correctional residence (<i>C</i>)021 (.144)	.058 (.234)	.006 (.074)
1984-93.			
Youth incarceration (<i>Y</i>)035 (.182)	.045 (.206)	.028 (.166)
<i>N</i> of respondents	2,917	854	1,914
Total observations	27,142	7,904	17,855

NOTE.—The total number of observations is not an exact multiple of the total number of respondents because of missing data. SDs are in parentheses.

Results

Table 7 reports estimates from the employment model. The negative youth incarceration effect indicates that respondents spending time in correctional facilities before 1980 spent less time in work four years later. On average, youth incarceration reduces employment by about five percentage points, or about three weeks per year. The effect is particularly large for blacks, whose employment is reduced by about nine percentage points (around five weeks in the year) by juvenile jail time. The size of this effect can be interpreted in light of other effects in the model (see appendix tables A2 and A3). Adult employment lost through youth incarceration exceeds the large negative effects of dropping out of high school or living in a high unemployment area.

Interaction effects trace the impact of youth incarceration over time. The effects are substantively small and insignificant, suggesting that the negative impact of imprisonment is extremely long lasting. Despite con-

TABLE 7
EFFECTS OF YOUTH INCARCERATION, Y , CORRECTIONAL RESIDENCE,
 C , AND WORK EXPERIENCE, E , ON TIME EMPLOYED,
NLSY MALES, 1984-93

	All Respondents	Blacks	Whites
Youth incarceration:			
Y_t	-.050 (3.88)	-.087 (3.06)	-.040 (2.64)
$Y_t \times P88_t$	-.010 (1.26)	-.014 (.88)	-.009 (.91)
$Y_t \times P91_t$..	-.004 (.56)	-.014 (.90)	-.001 (.07)
Correctional residence:			
C_t ..	-.246 (22.12)	-.240 (15.10)	-.255 (12.77)
C_{t-1} ..	-.189 (15.81)	-.170 (9.90)	-.215 (10.16)
C_{t-2} ..	.066 (5.48)	.093 (5.29)	.001 (.05)
C_{t-3} ..	.019 (1.63)	.012 (.68)	.021 (1.01)
Period dummies:			
1988-90 ($P88_t$) ..	-.004 (1.94)	-.005 (1.08)	-.003 (1.44)
1991-93 ($P91_t$) ..	-.013 (4.93)	-.014 (2.35)	-.012 (4.21)
Work experience:			
E_{t-1}408 (70.76)	.430 (39.76)	.375 (53.58)
E_{t-2} ..	.062 (11.37)	.067 (6.37)	.060 (9.10)
R^256	.59	.55

NOTE.—The R^2 statistic is defined as the squared correlation of the predicted with the observed values. Absolute t -ratios are in parentheses. Results for control variables are reported in the appendix (tables A2 and A3).

trols for adult incarceration, there is barely any tendency for the negative impact of youth incarceration to decay over time. Even after 15 years, respondents incarcerated as juveniles worked between 5 and 10 percentage points less than their counterparts who experienced no youth incarceration. Although these effects are large and enduring, causal inferences about youth incarceration may be biased because of unobserved heterogeneity. A stronger test must control for preincarceration employment status.

This control is provided by estimates of the effects of correctional residence. The contemporaneous effect of correctional residence is large and negative. This coefficient is driven upward by time out of the labor market during detention, rather than the postrelease effect of incarceration. The lagged effects provide stronger evidence of how ex-inmates fare on the job market. The first-order lags are large and negative. While partly reflecting short-run incapacitation, these estimates indicate that respondents lose more than two months of employment in the year after correctional residence. The coefficient for C_{t-2} is small and positive, varying by race. The estimate in the full sample depends largely on the result for African-Americans. With several lagged effects, the positive coefficient for the second-order lag can be interpreted as the speed of readjustment to regular employment patterns two years after incarceration. These estimates indicate that the postrelease penalty of incarceration vanishes faster for blacks than for whites. Because of the high incarceration rate among young African-American men, black ex-inmates may be less stigmatized than their white counterparts. Employers may treat black noninmates and ex-inmates more similarly as a consequence. Three years after correctional residence, negative incarceration effects have largely decayed for all respondents.

Interpretation of the correctional residence effects is also affected by the lagged employment variables. With this dynamic specification, incarceration in 1984, say, may cause unemployment in 1985, which further raises unemployment in 1986. Figure 3 shows the dynamic impact of incarceration by plotting the pattern of unemployment produced by a single year of correctional residence in year 1. Other independent variables are set at fixed levels. This example illustrates the impact of a year of incarceration over a seven-year period for a white, urban high school dropout, age 21, with average employment in the time leading up to year 0. (All other independent variables were set to "0".) Predicted employment was calculated recursively using the estimates reported in the first column of table 7 (see Box, Jenkins, and Reinsel 1994, pp. 390-94). Each prediction is bounded by a 95% confidence interval, conditional on the predictions of earlier years (Draper and Smith 1981, p. 94). Correctional residence affects employment after year 1 through its lagged effects and the lagged dependent variable. In year 1 when correctional residence is reported, employment falls on average by one-third, from 42 weeks to 28. The year after reported incarceration, time in employment falls another three weeks. Like the effect for year 1, the negative impact of incarceration in year 2 partly expresses detention from the labor market during imprisonment. In year 3, however, there is no possibility of incapacitation, and we find a postrelease effect of five weeks' unemployment compared to the

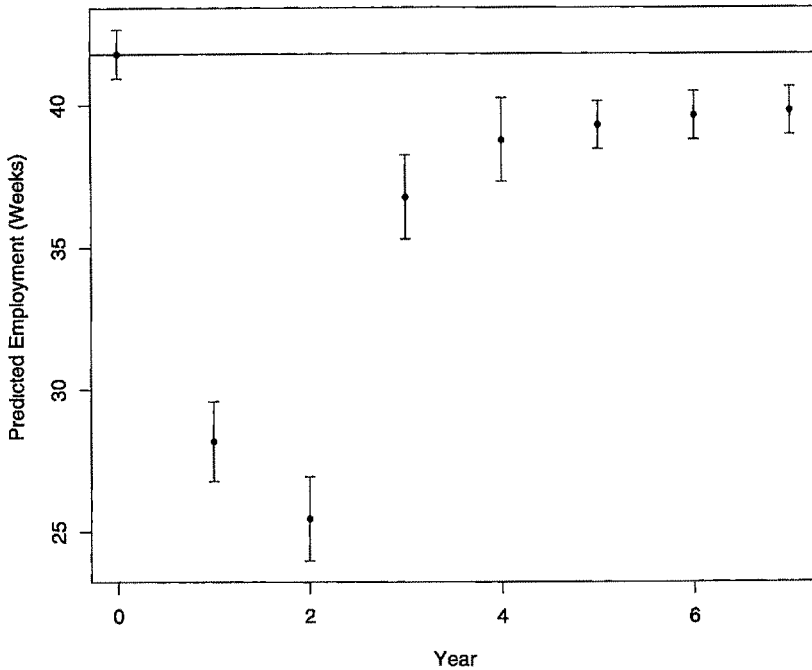


FIG. 3.—Predicted employment in weeks given correctional residence in year 1 (error bars show a conditional 95% confidence interval around predictions).

baseline year 0. Because the confidence interval for this prediction does not overlap the baseline level of employment in year 0 or the long-run level of employment in year 7, the data offer support for a negative long-run effect of incarceration on employment. This negative effect persists for several more years but becomes relatively small. Other prediction exercises yield substantively identical results.

In sum, the NLSY provides strong evidence of a small but persistent effect of juvenile incarceration. Respondents incarcerated in or before 1980 experienced high levels of unemployment some 15 years later, even controlling for later incarceration and work experience. Adult incarceration, on the other hand, has large negative effects on ex-inmates after release, but these effects decay within three or four years of release. In general, a variety of models show that incarceration has large and enduring effects on the job prospects of ex-convicts. Results were also robust to different subsets of control variables. The effects of control variables all conform to theoretical expectations.

DISCUSSION

U.S. prisons and jails grew threefold from 1979 to detain around 1.6 million people by 1995. This paper analyzes the short-run and long-run effects of this rapid growth in incarceration on the labor market. In the short run, large prison and jail populations conceal a high level of joblessness. If included in labor market statistics, the population of incarcerated men would contribute about two percentage points to the U.S. male unemployment rate by the mid-1990s. These effects are especially large for African-Americans: labor inactivity is understated by about one-third, or seven percentage points, by the conventional measure of black male unemployment. Challenging claims of "Eurosclerosis" and the successful deregulation of the U.S. labor market, our estimates of labor inactivity among U.S. men consistently exceed average European unemployment rates between 1975 and 1994. State intervention in the labor market through the penal system thus contributes to a falsely optimistic picture of U.S. labor market performance in comparison to Europe.

While incarceration conceals unemployment from conventional jobless statistics in the short run, it increases the chances of unemployment among ex-convicts in the long run. NLSY data indicate that the negative effect of youth incarceration on adult employment can last for over a decade and that adult incarceration lowers later participation in paid employment by 5 to 10 weeks a year. With over a million men now in prison or jail, the results suggest that the penal system annually generates the equivalent of a full year of unemployment for more than 200,000 American men. In the long run, incarceration thus significantly undermines the productivity and employment chances of the male workforce.

Viewing the U.S. penal system as a labor market institution likens it to the European welfare states. Just as European researchers argue that welfare states artificially reduce the labor supply (Neubourg 1983, pp. 25–27), we find significant hidden unemployment through incarceration. Despite this similarity, our analysis suggests two important differences between welfare states and penal systems as labor market institutions.

First, while European social policy is redistributive, the employment effects of U.S. incarceration exacerbate inequality. Comparative research shows that tax and transfer policies lifted about half the nonelderly poor out of poverty in European countries in the 1980s (McFate, Smeeding, and Rainwater 1995, p. 39). Incarceration has the reverse effect. Because incarceration rates are highest among young, unskilled, minority men, the negative employment effects of jail time are focused on those with the least power in the labor market. The penal system thus deepens existing market inequalities.

Second, welfare institutions and their economic effects appear stable

over time, but incarceration's short-run effect of lowering conventional unemployment is sustained by an ever-increasing incarceration rate. Recent research suggests that European welfare measures create few economic inefficiencies (Blank 1995). Indeed, sociologists argue that some welfare measures boost economic performance by improving labor mobility and productivity (Kolberg and Esping-Andersen 1990; Janoski 1990). In this context, the effects of welfare effort fluctuate with the business cycle. The dynamic analysis of this paper implies a fundamentally different logic for incarceration in which the short-run and long-run effects are closely interdependent. Low U.S. unemployment through the mid-1990s suggests that the short-run effect of incarceration currently dominates its long-run effect. The increased unemployment risk of ex-convicts is more than compensated by the escalating incarceration rate. High rates of recidivism help explain the predominance of the short-run effect. About two-thirds of young state prisoners are rearrested within three years, cycling many of those at risk of unemployment out of the labor market and back into custody. However, new entrants to the labor force also face a high incarceration rate, which ultimately raises their unemployment risk. Under these conditions, the appearance of strong employment performance is assisted by an ever-increasing correctional population. In striking contrast to the usual picture of the unregulated U.S. labor market, incarceration—as a labor market intervention—can thus be understood as “super-regulatory.” High incarceration rates lower conventional unemployment statistics by hiding joblessness but create pressure for rising unemployment once inmates are released. Sustained low unemployment depends, in part, not just on a large stage intervention through incarceration but on a continuous increase in the magnitude of this intervention.

This account of low U.S. unemployment sustained by an expansive and regressive state intervention contrasts sharply with earlier research. The economic analysis of Eurosclerosis that emphasizes the free play of market forces and sociological research focusing on industrial relations and the welfare state both treat the U.S. labor market as largely unregulated (Calmfors and Driffill 1988; Crouch 1985). The broader definition of labor market institutions used here shares with other economic sociology an interest in the influence of noneconomic social relations on economic outcomes (Polanyi 1944; Granovetter 1985). From this perspective, the economic model of competitive markets cannot even be approximated empirically because of the pervasive influence of the surrounding social context.

More generally, this analysis suggests that labor markets are embedded in a broad array of social arrangements that extend well beyond the employment relationship. Although these social arrangements may not directly regulate markets like labor unions or social policy, this broader in-

stitutional context still strongly influences labor market outcomes. Institutions are thus as fundamental to the operation of the ostensibly unregulated U.S. labor market as they are to the centralized industrial relations regimes of Western Europe. While some policy analysts celebrate the free market principles of the U.S. model, these same principles should be assessed in light of the significant and coercive reallocation of labor through the expansion of U.S. prisons and jails.

APPENDIX

TABLE A1
MALES IN THE CUSTODY OF STATE AND FEDERAL PRISONS
AND LOCAL JAILS, BY RACE, 1982-95

Year	Total	White	Black
1982	596,000	325,100	263,100
1983	611,800	331,800	272,000
1984	644,800	351,700	284,400
1985	701,400	382,700	309,800
1986	789,700	417,600	342,400
1987	805,200	439,000	356,300
1988	887,300	469,200	407,400
1989	1,001,200	518,000	472,800
1990	1,087,900	545,000	508,800
1991	1,139,500	566,700	551,000
1992	1,204,700	598,000	580,300
1993	1,269,800	627,100	624,000
1994	1,367,600	669,100	677,500
1995	1,466,700	728,700	713,500

SOURCE.—Unpublished data from Bureau of Justice Statistics, compiled by Allan Beck and Darrell Gilliard (1997)

TABLE A2

DESCRIPTIVE STATISTICS FOR ALL CONTROL VARIABLES IN NLSY ANALYSIS

Variable	Description	Mean
Probation (youth)	On probation in or before 1980	.09
Charged (youth)	Charged with a crime in or before 1980	.07
Convicted (youth)	Convicted of a crime in or before 1980	.09
Stopped by police (youth)	Stopped by police in or before 1980	.28
Age	Age of respondent in years	27.28
Black	A dummy variable for black respondents	.29
Was married	Not married at time of interview, but was married at some time	.10
Currently married	Married at time of interview	.45
Less than high school	Dropped out of high school at time of interview	.18
Some college	Graduated from high school and completed at least some college at time of interview	.32
Low unemployment locality	Lived in an area with unemployment under 4.5% at time of interview	.38
High unemployment locality	Lived in an area with unemployment over 10.5% at time of interview	.08
South	Lived in the South at time of interview	.37
West	Lived in the West at time of interview	.20
Midwest	Lived in the Midwest at time of interview	.25
Urban residence	Lived in an urban area at time of interview	.79

TABLE A3
REGRESSION RESULTS FOR CONTROL VARIABLES IN NLSY ANALYSIS

Variable	All Respondents	Blacks	Whites
Constant ...	375 (19 16)	.372 (8.64)	.388 (18 05)
Probation (youth) ...	-.039 (5.16)	-.051 (3 17)	-.032 (3.87)
Charged (youth)007 (.89)	.036 (1.89)	.001 (.14)
Convicted (youth)008 (1.08)	.025 (1.28)	.000 (.03)
Stopped by police (youth) ...	-.009 (2.28)	-.020 (2.32)	-.005 (1.12)
Age001 (.98)	-.002 (1.30)	.002 (2.58)
Black ..	-.042 (10 55)
Working at last interview ..	.221 (56.04)	.256 (31 96)	.199 (43 79)
Working at second last interview008 (1.86)	.010 (1.25)	.007 (1.43)
Was married	-.002 (.43)	.010 (.90)	-.013 (2.30)
Currently married031 (9 31)	.044 (5.66)	.022 (6.20)
Less than high school ..	-.042 (9.27)	-.037 (4.10)	-.041 (7.87)
Some college ..	.022 (5.83)	.039 (4.50)	.016 (4.13)
Low unemployment locality ..	.013 (4.37)	.013 (2.04)	.010 (3.21)
High unemployment locality ..	-.021 (4.14)	-.072 (4.81)	-.012 (2.17)
South007 (1 52)	.007 (.68)	-.001 (.27)
West000 (.05)	-.022 (1.46)	.000 (.04)
Midwest	-.004 (.81)	-.019 (1.49)	-.007 (1.24)
Urban residence004 (1 40)	.005 (.64)	.005 (1.62)

NOTE.—Absolute *t*-ratios are given in parentheses

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The Enduring Effects of Cohort Characteristics on Age-Specific Homicide Rates, 1960–1995¹

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In the past decade, young people in the United States have been two to three times more likely than in the two previous decades to commit homicides, while those 25 years and older have been less likely to commit homicides than were members of their age groups in the earlier time period. These changes in youth homicide rates are associated with two cohort characteristics that are theoretically linked to criminality: relative size of cohorts and the percentage of cohort members born to unwed mothers. These effects persist throughout the life span, are independent of age and historical period, and can explain fluctuations in homicide arrest rates before the recent upturn.

INTRODUCTION

Until the mid-1980s, it appeared that the relationship between age and homicide was relatively stable, so stable that some scholars described it as "invariant" (Hirschi and Gottfredson 1983). This relationship, however, changed markedly in the past decade as the likelihood of a young person in the United States committing homicide increased two- to threefold. For those 14–17 years of age, the homicide rate climbed from 6.2 per 100,000 in 1984 to 19.1 per 100,000 in 1994, and, for those ages 18–24, the rate rose from 15.3 per 100,000 in 1984 to 25.3 per 100,000 in 1994. In contrast to the pattern for younger cohorts, homicide rates for people 25 years old and older declined during this same period, with rates dropping from 6.3

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per 100,000 in 1984 to 4.7 per 100,000 in 1994 (Maquire and Pastore 1996).²

This epidemic of youth homicide has received much attention, with most explanations focusing on the introduction of crack cocaine and the ensuing increase in the possession of guns by adolescents (Blumstein 1995; Blumstein and Cork 1996; Cook and Laub 1997).³ While such explanations seem plausible, they essentially are ad hoc. A more comprehensive explanation would apply not just to the period after the mid-1980s but to earlier historical periods as well.

In this article, we explain the relative increase in youth homicides in comparison to those age 25 and over without invoking an explanation specific to the period after the mid-1980s. We build on the traditions of cohort theory and research as well as research in crime and delinquency and examine changes in the characteristics of cohorts born during 1915–19 and 1975–79. We account not only for the relative increase in youth homicides since 1985 but for more subtle changes in the relationship between age and homicide rates between 1960 and 1985. Changes in these cohort characteristics are associated with the homicide rates of each of the cohorts throughout their life spans independent of the effects of age and historical period.

RELATED LITERATURE

Over 30 years ago, Norman Ryder (1965) described how birth cohorts move in a two-dimensional space of time and age. This makes one cohort's experiences different from those of any other's. For instance, infants experience historical events, such as an economic depression or war, differently than those entering the labor force, those of middle age, and those who have retired. A major tenet of cohort theory and research asserts that certain events can produce *lasting changes* in the attitudes and behaviors of cohort members. These changes depend upon cohort members' ages when the events occur, and these effects are analytically distinct from those associated with age and historical period. Thus, living through the Depression produced lasting changes, which varied from one birth

² Homicide rates for the total population of the United States rose dramatically from 1962 (4.6 per 100,000) to 1974 (9.8 per 100,000) then fluctuated moderately, reaching a high of 10.2 per 100,000 in 1980 and a low of 7.9 per 100,000 in 1985 and 1986. From 1974 to the present, homicide rates have fluctuated up and down but do not appear to be systematically increasing or decreasing (O'Brien 1996).

³ The entire 1996 fall issue of *Law and Contemporary Problems*, entitled "Kids, Guns, and Public Policy," was devoted to this epidemic. The 1995 fall issue of the *Journal of Criminal Law and Criminology*, entitled "Guns and Violence Symposium," also contained several articles focused on this problem.

cohort to another (e.g., Elder 1974, 1979; Elder, Modell, and Parke 1993). Such cohort effects have been found in a variety of areas such as political orientation and voting (Firebaugh and Chen 1995; Alwin and Krosnick 1991), racial and sex role attitudes (Mason and Lu 1988), intellectual skills (Alwin 1991), parental values (Alwin 1990), and criminal behavior (O'Brien 1989). Some of these effects may reflect changes in strategic styles—variations in behavioral strategies including criminality (Vila 1994).

Determining that cohorts vary is different than explaining why these effects occur. One way of considering this question is to view the historical periods in which cohorts are born as opportunity structures, settings that provide both opportunities and constraints for members of a birth cohort (e.g., Elder 1996). These opportunities and constraints may result from historical events, such as wars or economic depressions. They may also derive from demographic characteristics associated with birth cohorts such as sex ratios, relative size, and family structures. Two demographic aspects of cohorts that are theoretically linked to criminal behavior are relative cohort size and family structure.

Relative Cohort Size

Many analyses of cohort variations involve Richard Easterlin's (1978, 1987) hypothesis that members of relatively large cohorts experience disadvantages that persist throughout their life spans. Some of these disadvantages involve economic opportunities—the relatively greater number of workers in these cohorts leads to a buyers' market for employers and thus to lower wages and greater unemployment for members of these cohorts. These in turn may lead to delayed marriage and smaller families (Easterlin 1987). These consequences of relative cohort size might be labeled job-market effects.

We theorize that other effects of relative cohort size are related to community and family resources. Relatively large cohorts overload institutions of social control and stretch the family and community resources available to cohort members. Members of these cohorts grow up with more children per parent, more children per classroom, and more children per counselor (O'Brien 1989). Just as children in families with excessive demands relative to their caregivers' resources receive fewer family resources, children in relatively large birth cohorts have available to them diminished community resources. This involves lower levels of supervision and attention from parents, teachers, counselors, and other adults as well as more crowded homes and schools.

Researchers have found some evidence of a relationship between relative cohort size and homicide arrest rates (O'Brien 1989; Steffensmeier,

Streifel, and Shihadeh 1992). Given this evidence, and because of job market, community, and family resource effects, we hypothesize that relatively large cohorts will have higher crime rates in comparison to relatively small cohorts, once the effects of age and period are controlled.

Family Structure

At the individual level, the relationship of family interactions and structure to delinquent behavior is well documented. Research indicates that children from homes with insufficient supervision and attention are much more likely to be delinquent, and because it is harder for one parent to provide as much supervision, consistent discipline, or attention as two can provide, children from single-parent homes are more at risk than those from two-parent homes (e.g., Dishion, Patterson, Stoolmiller, and Skinner 1991; Farrington 1989; Furstenberg and Hughes 1995; Hetherington, Cox, and Cox 1978; Huesmann, Eron, Lefkowitz, and Walder 1984; Loeber 1982; Nagin and Paternoster 1991; McLanahan and Sandefur 1994; Patterson, DeBaryshe, and Ramsey 1989). A number of contemporary theories based in sociology, criminology, and developmental psychology have incorporated the links between ineffective child-rearing practices, the development of low self-control, and later criminal propensities or behavioral strategies (e.g., Gottfredson and Hirschi 1990; Sampson and Laub 1993; Moffitt 1993; Patterson and Yoerger 1993; Vila 1994).

A literature within the broad perspective of social structure and personality further supports the emphasis on the relationship between childhood family relations and criminal behavior but focuses on males and aggression. These theorists point to the special role of fathers in helping young boys control aggressive tendencies and develop a secure gender identity. They suggest that when fathers are absent from their lives, boys are more apt to exhibit signs of "compulsive masculinity," including heightened levels of aggression (Coltrane 1988, 1992; Holter 1970; Parsons [1947] 1954). In support of this approach, cross-cultural data indicate that males in subcultures and societies with less father-son contact, especially in the early years of life, are much more likely to display characteristics typical of compulsive masculinity and hyperaggression (Tiller 1958; Coltrane 1988, 1992).

Researchers have also highlighted the relationship between single-parent families and poverty. Especially important from our perspective is the relationship between single-parent families and youths growing up in poverty. For instance, in 1994, the rate of poverty for children under 18 who lived in married-couple families was 8.3 per 100. For children who lived in female-headed households, the rate was 44.0 per 100 (O'Hare

1996). These children are less likely to live in "safe and desirable" neighborhoods, to obtain adequate medical care, to be successful in school, and to have adequate day care and after-school care (National Research Council 1993; McLanahan and Sandefur 1994). As more members of a cohort grow up in poverty, more children in that cohort are likely to have peers who have experienced poverty.

At the macrolevel, a number of studies document the relationship between family disruption and crime (Blau and Blau 1982; Jacobs and Helms 1996; Huff-Corzine, Corzine, and Moore 1986; Messner 1983; Messner and Sampson 1991; Sampson 1985, 1986; Sampson and Groves 1989; Williams 1984; Williams and Flewelling 1988) and find that the relationship is especially strong for juvenile crime (Sampson 1987). Sampson (1987, pp. 352–53) accounts for this relationship with a theory of community social organization, noting that increased family disruption decreases the effectiveness of both formal and informal social controls within the community. This contextual effect is independent of the influence of individual family composition, since all youngsters in a community are influenced by the decreased social control or "social disorganization" (see esp. Sampson and Wilson 1995, p. 44).

None of this research explores the influence of family disruption on cohort variations in criminal behavior across the life span of cohorts, although several authors have hypothesized that family disruption in an earlier period may result in increases in criminal behavior in later periods. For instance, Sampson and Wilson (1995) state that "the roots of urban violence among today's 15–21-year-old cohort may stem from childhood socialization that took place in the late 1970s and early 1980s" (p. 53), a time when family disruption began increasing sharply. Similarly, Vila (1994, p. 342) hypothesizes that individual life experiences, such as those related to family life, nurturance, and social control, will have a lagged effect on criminality and crime rates. Although their analysis focused on other issues, results in Jacobs and Helms (1996) indicate that a five-year moving average of the percentage of children born out of wedlock, which is lagged 19 years, is related to prison admissions in the United States from 1950 to 1990.

Based on both the microlevel and macrolevel analyses and theorizing, it is reasonable to expect that an increase in the proportion of a cohort growing up in single-parent homes is associated with an increase in that cohort's criminogenic tendencies. This relationship is produced in at least two contexts. First, within the single-parent family, there is an increased likelihood of poverty, which diminishes a family's abilities to provide adequate day care, after-school care, and other valuable resources for its children. There also is a decreased likelihood of the parental-child contact

and parental monitoring of children's activities that facilitates the development of self-control. We anticipate a rising level of criminal violence to accompany the rise of single-parent families, especially single-parent families that contain young children (children in their first 8–10 years). This simply reflects the aggregated experiences of those growing up in single-parent families.

Second, building on Sampson and Wilson's (1995, p. 44) analysis of community contextual effects and Coleman's (1990) discussion of "closure" and social capital, we suggest that all children within cohorts with relatively more single-parent families experience less social control and social capital, whether or not they are members of single-parent families.⁴ For example, two-parent families are more likely to contain an adult (parent) who knows one or more parents of their children's friends. This increases closure (social organization) of the community and more specifically the system made up of parents and children. As a result, the monitoring and supervisory abilities of parents, which are crucial in the development of self-control in children, are increased.

In addition, in settings with fewer intact families, the importance of peers relative to parents and other adults increases. The larger proportion of children who experience poverty and the diminished parental contact and supervision in the home alter interactions in peer groups and the nature of peer-based socialization experienced by all children, regardless of their own individual family background. Peer groups would be more likely to include others with a greater probability of having antisocial tendencies, and peer-group socialization influences the behavior of cohort members, whether or not they are from a single-parent family.⁵

These forms of social control and social capital are important aspects of community and family resources. Our use of these terms is akin to Sampson and Wilson's (1995) use of the phrase "structural dimensions of community disorganization" to refer to "the prevalence and interdepen-

⁴ Coleman (1990, p. 300), describing Loury's (1977, 1987) initial use of the term *social capital*, refers to it as "the set of resources that inhere in family relations and in community social organizations and that are useful for the cognitive or social development of a child or young person." Most analysts today would expand this definition to include more than those relationships that are useful to children and young people. Adults, too, can benefit from increases in social capital (Coleman 1990).

⁵ A recent study of contextual effects on student achievement lends support to this contention. Pong (1998) found that children who attend schools where a greater proportion of students come from single-parent homes tend to have lower levels of academic achievement than would be predicted given other characteristics such as their socioeconomic status and their own family structure. Pong found that this contextual effect can be explained, in part, by social capital, particularly parents' involvement in the school.

dence of social networks in a community.” These include formal and informal networks and “the span of collective supervision that the community directs toward local problems” (p. 45). We suggest that it is useful to conceive of social capital (those resources that inhere in family relations and social organization) as residing not only on the microlevel of individual relationships or at the macrolevel of the community but also as a characteristic of birth cohorts.

Predictions

To summarize, we expect cohort effects on homicide rates due to both the aggregation of the effects of families with more children and single-parent families on the individuals from those families *and* because of the effects of relatively more children and the availability of relatively less parental and community resources for the members of those cohorts whether or not these members are from such a family.

We test the hypothesis that variations in age-period-specific violent criminal behavior, specifically homicide, are associated with variations in two cohort characteristics: one designed to examine the Easterlin (1978, 1987) hypothesis and another to assess (*and serve as a proxy for*) several related changes in family structure. Specifically, we examine the relationship of the homicide arrest rates for cohorts throughout their life spans to the cohorts' size relative to other cohorts and the percentage of births to unwed mothers. Both of these cohort characteristics relate to factors that contribute to the community and family resources available to cohorts, such as the number of parental figures per child, the likelihood that parents of children's friends know each other, and the number of students per teacher. We hypothesize that both relative cohort size and the percentage of births to unwed mothers are positively related to the homicide arrest rates for cohorts and that the effect of these cohort characteristics is independent of both age and period effects.⁶

Finally, while we expect the effects of relative cohort size and the percentage of births to unwed mothers to occur throughout the life span, it is possible that these effects will be most marked at younger ages. For instance, some authors (Kahn and Mason 1987; Steffensmeier et al. 1992) argue that the effects of relative cohort size should be especially pronounced for those who are young, when they are most in need of support

⁶ We realize that discussions of difficulties faced by children of single mothers have produced political controversy in the past. No single study can establish whether such difficulties do or do not exist. If they do not, there is no problem. If they do, then it is important that researchers identify the nature of these problems so they can be addressed.

from family and communities. Similarly, while noting the importance of early family experiences in promoting criminal behavior, Sampson and Laub (1993) note that some young people who engage in crime in adolescence and young adulthood are able to alter these behavior patterns when they are older. We thus test the possibility that the effects of these cohort-related characteristics are especially marked at younger ages.

DETECTING COHORT EFFECTS

A close reading of Easterlin (1987) reveals a number of criteria to use when looking for the effects of relative cohort size specified in his theory. We extend this logic to the examination of nonmarital births.

Easterlin emphasizes that any examination of the effects of cohort characteristics must involve cohort-specific dependent variables, such as age-period-specific homicide rates. As Easterlin says, "[T]he changing age structures of the population—technically dubbed 'age-composition effects'—are important, but are not my concern. . . . My primary interest, in other words, is in the conditions specific to the group whose number is changing—what are called 'age-specific' effects" (Easterlin 1987, p. 6).

Second, it is important to examine characteristics of cohorts relative to other cohorts within the population, for instance, the relative size of the cohort and the relative number of children born to unwed mothers—not the absolute size of the cohorts or the absolute number of cohort members who were born to unwed mothers. Easterlin (1987) argues that it is the "bulge" created by the cohort as it passes through the system that creates problems for the cohort. If the cohort is much bigger (relatively) than the cohorts that immediately precede it through the system, then there is not enough capacity for it in terms of teachers or counselors per cohort member, and when they enter the job market there are not enough jobs per cohort member. For a relatively small cohort, there is an excess capacity in schools and a greater demand for their skills when they enter the job market. In the case of cohort members born to unwed mothers, we suggest that it is their relative number that directly affects the relative rates of homicide. In addition, it is their relative number that is important in operationalizing group processes, such as peer socialization or the average amount of closure, that diffuse beyond the individuals who were born to unwed mothers.

Third, cohorts should be based on the grouping of individuals born in more than a single year. Easterlin states that, "I use 'generation' and 'cohort' interchangeably. . . . My interest is primarily in those born in periods of low or high birth rates, rather than in any one high or low year" (1987, p. 7). Easterlin's reasoning suggests that the schools can absorb a one-year bulge in the population or the labor market but not a five-year bulge.

Similarly, an increased percentage of nonmarital births in a single year should not have as great an impact as a high percentage for several years. With a high percentage for several years, not only do relatively more children grow up in families in which they were born to unwed mothers, but a larger proportion of the children in that cohort engage in peer socialization with a broad group of peers from these families.⁷

Fourth, relative cohort size affects cohorts throughout their life spans. "The effects of generation size, good or bad, persist throughout the life cycle. Every generation follows the pattern of below-average earnings in early working life and above-average earnings later. But the earnings pattern of a small generation is more favorable throughout its career than that of a large generation—during early working life, the earnings of a small generation are not so far below average; at mid-career, the earnings are further above average" (Easterlin 1987, pp. 29–30). We predict lower homicide arrest rates for cohorts that are relatively small and that have lower percentages of nonmarital births. These decreased rates should persist throughout the life spans of the cohort members.

Fifth, Easterlin (1987) suggests that the effect of relative cohort size is most likely for data from the United States for the period after World War II. He selected this period, in part, because of its large variation in relative cohort size and relatively low level of immigration. Thus, for models that include relative cohort size, the test should ideally involve data from that period, as our data do.

Finally, it is crucial to control for the effects of period and age before testing for the effects of cohort characteristics on age-period-specific rates. When these controls are ignored, it becomes difficult to disentangle the effects of cohort characteristics from age and period effects.

We meet each of these criteria. (1) The dependent variable is cohort specific, that is, age-period-specific homicide arrest rates. (2) We use relative measures for the independent variables: relative cohort size and the percentage of nonmarital births. (3) Our age groupings cover five-year periods. (4) We examine the effects of relative cohort size and the percentage of nonmarital births on homicide rates throughout the life span of the cohorts. (5) Our data cover a period after World War II. (6) We control for both age and period effects.

⁷ It is perhaps worth mentioning that we are not contending that all children who are born to unwed mothers are poorly socialized (see also Furstenberg, Brooks-Gun, and Morgan 1987; Furstenberg and Hughes 1995). We do maintain that it is easier for families and communities to provide adequate supervision and consistent discipline when two-parent families (or those with two other responsible adults) are more common, and that the percentage of nonmarital births is related to the number of parents or parent substitutes available within the cohort

DATA AND MEASURES

Homicide Arrests

We would have liked to use the rates of homicides, known to the police, that were committed by different age groups in different years. However, these data, broken down by age groups, are not available over an extended time period. We instead use arrest rates. Homicide rates, fortunately, are not only the best reported Part 1 Uniform Crime Reports (UCR) crime rates, they are likely to be the ones for which crimes known to the police and arrest data are the most consistent.⁸ We examined this relationship for the years covered in our analysis, correlating five-year moving averages for total homicide rates and total homicide arrest rates for the years 1960 to 1995. The resulting correlation was .99.⁹

Our data for homicide arrests come from the UCR (FBI 1961, 1966, 1971, . . . , 1996) and are based on five-year age groupings. This particular grouping of ages results, in part, from the way the published reports group arrest rates. From 1960 until the present, the UCR grouped arrest rates for those ages 25–29, 30–34, 35–39, 40–44, and 45–49. The ages 15, 16, 17, through 24 have been reported separately for arrests since 1960, and we grouped them to obtain equal-size age groups that would correspond to equal-length periods (five-year periods).

The series begins in 1960 because that is the earliest year for which data are reported on homicide arrests based on a reasonably representative group of law enforcement agencies using a fairly consistent basis for reported arrests over the entire period. For example, in 1950, fingerprint records kept by the Federal Bureau of Investigation (FBI) formed the basis for the data on arrests by age groups, but by 1955, reports to the FBI and not fingerprints formed the basis of the arrest by age-group counts. Unfortunately, data were reported only for cities with populations over 2,500. Since 1960, data based on the UCR system have been reported for both rural areas and cities, separately or combined.

The arrest data taken from the FBI need to be corrected for the population of the United States covered in a given year, since the arrest figures for each age group are based on the number of reporting agencies, which varies from year to year. We computed a correction factor by dividing the total population of the United States by the number of residents in the areas reporting to the FBI that year. This ratio was multiplied by the

⁸ The age-period-specific arrest data in the UCR are not broken down by race or gender. For that reason, all of our analyses use arrest data broken down by age group for the entire population.

⁹ The simple correlation between yearly homicide rates and homicide arrest rates was .97.

number of homicide arrests in each of the age groups. This corrected number of homicide arrests was then divided by the number of U.S. residents in the particular age group and multiplied by 100,000 to obtain an estimate of the number of homicide arrests in the age group per 100,000 individuals in the age group. Data on the number of U.S. residents in each age group were drawn from the Current Population Surveys: Series P-25 (U.S. Bureau of the Census, various dates). We logged the homicide arrest rates (natural log) for two reasons. First, the distribution of these rates was markedly skewed to the right without such a transformation. Second, we felt that it was important that a doubling of the homicide rate for those ages 45–49 should be weighted as much as a doubling for the homicide rates for those ages 15–19. The log transformation provides such weighting when the dummy variables for age groups are in the equations.

Cohort Characteristics

Both of the variables that we use to characterize cohorts are clearly associated with each birth cohort: the percentage of nonmarital births (%NB) and relative cohort size (RCS). Although we have provided theoretical linkages between the percentage of a cohort reared by single parents during early childhood and the homicide rate for that cohort compared with other cohorts, we need to mention why we chose to represent this family structure by the %NB. The main reason, in addition to the partial fit of this variable with what we would ideally like to measure, is the practical one of data availability. Data on single-parent families with young children (say, 0–9 years of age) does not exist for the range of cohorts that we examine, but data are available on %NB for all but the cohort born between 1910 and 1914. This is not a perfect measure, but it does indicate the likelihood that young children were raised by a single parent during at least part of their first 8–10 years. As we will see later, the %NB works quite well in predicting the age-period-specific homicide arrest rates of cohorts.

Data for %NB (the number of births to unwed mothers per 100 live births) were obtained from two different volumes of *Vital Statistics of the United States* (U.S. Bureau of the Census 1946, 1990). Data for the years 1917–40 were taken from the earlier volume, and data for the remaining years were drawn from the second volume. To obtain %NB for each cohort, we summed the appropriate percents and divided by the number of years.¹⁰ For example, to obtain %NB for those who were ages 15–19 in

¹⁰ Nonmarital birth data in *Vital Statistics of the United States* are presented as rates per 1,000, so we first converted these to percentages before we summed them. In these census publications these rates are called “illegitimacy ratios.” We averaged the percentages across the five years in each birth cohort rather than calculating a “grand”

1960, we summed %NB for the years 1940–44 and divided by five. The %NB for the oldest birth cohort was not available, since that cohort was born between 1910 and 1914. The %NB for the second oldest cohort was derived by summing %NB for 1917, 1918, and 1919 and dividing by three. All other %NB values were based on five years of data.

RCS is operationalized in two ways: (1) the percentage of the population age 15–64 that is in the cohort when the cohort is age 15–19, and (2) the percentage of the population age 15–64 that is in the age group 15–19 when the cohort is 15–19, the percentage of this population that is age 20–24 when the cohort is 20–24, and so on throughout the age range covered in the analysis. Both of these operationalizations have been used before (O'Brien 1989). We use these operationalizations in separate analyses to determine if using them makes any substantive differences in the results. We label the first operationalization as the fixed operationalization for RCS, because its value is constant for each cohort at all ages. The second is labeled as the variable operationalization for RCS, because its value varies for a cohort at different ages. Data for the RCS measures were obtained from the Current Population Surveys: Series P-25 (U.S. Bureau of the Census, various dates).¹¹

ANALYSIS

We analyzed the data using a modified age-period-cohort model. Figure 1 depicts an age-period-cohort model corresponding to our situation and

percentage across the five years, because the data for the years before 1938 are available in the published reports only as ratios for the *reporting areas* rather than as estimates for the nation as a whole. In 1951, estimates for the nation as a whole were made for the number of nonmarital births (and nonmarital birth ratios) for the years 1938–50. After that year, estimates were reported for the United States as a whole regardless of the reporting areas. Given these shifts, and the difficulty of tabulating the number of live births in the areas that reported nonmarital births for each of the years since 1917, we decided to average the percentages based on the *reporting areas* or (in later years when only estimates for the nation as a whole were available) the percentages based on the nation as a whole, across the appropriate five-year cohorts.

¹¹ O'Brien (1989) notes, in the context of testing for the Easterlin effect, that it would be inappropriate to include variables such as cohort unemployment rates, delays in marriage, declines in cohort wages, and so forth that Easterlin (1978, 1987) predicts are affected by RCS as separate independent variables in the age-period-cohort-characteristic model designed to predict age-period-specific homicide rates. To the extent that these variables are caused by RCS and %NB, which are arguably causally prior, we would then be partialling the effects of these variables from themselves. An agenda for future work could include a thorough examination of the way in which these variables may intervene in the relationship between relative cohort size and homicide rates.

	15-19	20-24	25-29	30-34	35-39	40-44	45-49	
1960	12.43 7 8.98	10 14 6 14.00	10 09 5 13 45	11 10 4 10.73	11 64 3 9:37	10 85 2 6 48	10 17 1 5 71	
1965	14 62 8 9 07	11.58 7 15 18	9 70 6 14.69	9 54 5 11.70	10 32 4 9 76	10.70 3 7.41	9.81 2 5 56	14 40 1 N.A
1970	15.27 9 17.22	13 16 8 23 76	10.80 7 20 09	9 14 6 16 00	8 80 5 13.13	9.50 4 10 10	9.64 3 7 51	13.89 2 2.10
1975	15.33 10 17.54	13 96 9 25.62	12 41 8 21 05	10 21 7 15 81	8 37 6 12.83	8.07 5 10.52	8.51 4 7.32	13.69 3 2.57
1980	14.03 11 18.02	14.25 10 23.95	13 09 9 18 91	11 82 8 15.22	9 38 7 12.31	7.81 6 8 79	7 35 5 6 76	12.39 4 2 93
1985	11 72 12 16 32	13 27 11 21.11	13 74 10 16.79	12 81 9 12 59	11 19 8 9 60	8.88 7 7.50	7 36 6 5.31	10 80 5 3 92
1990	10.82 13 36.52	11 66 12 29.10	12.94 11 17 99	13.35 10 12.44	12 18 9 9 38	10 84 8 6 81	8 42 7 5.17	10 87 6 4.08
1995	10.53 14 35 24	10.43 13 32.34	11 08 12 16 75	12.75 11 10 05	12.97 10 7.27	11 79 9 5 48	10.17 8 3.67	12 43 7 3.62
	10.53 14 15.59	10 82 13 12 11	11.72 12 8 97	14 03 11 5.99	15 33 10 4 82	15.27 9 4.06	14.62 8 3.82	

FIG. 1.—Age-period-cohort model with cohorts numbered in the cells (cohort 7 is the cohort born between 1940 and 1944). Within the cells: top, RCS of the cohort at each age; middle, cohort number; bottom, homicide rate. In the margins: top, RCS when the cohort was ages 15–19; middle, cohort number; bottom, percentage born out of wedlock.

provides the data used in our analyses. The rows and columns indicate period and age; each of the cells indicates the experiences of a particular cohort at a given age and year. Different birth cohorts are identified by number (the boldface number). Cohort 1 was born between 1910 and 1914, cohort 2 between 1915 and 1919, and so on. By following a particular cohort diagonally through the table, one can see the way in which cohorts move through the time/age space described by Ryder (1965). The top and bottom entries in each cell represent, respectively, the *variable* RCS measure (the percentage of the population ages 15–64 who were in the cohort during the period represented by the row) and the age-period-specific homicide arrest rate for the cohort during that period. The margins contain values that remain the same for a cohort over time. The top and bottom entries represent, respectively, the *fixed* RCS measure (the percentage of the population ages 15–64 who were in the cohort when the cohort was 15–19) and the percentage of the cohort members who were born to unwed mothers.

Inspection of figure 1 indicates substantial variation on each of our measures. Age-period-specific homicide rates range from a low of 3.7 for cohort 8 (those born between 1945 and 1949) in 1995 (when they were 45–49 years of age) to a high of 36.5 for cohort 13 (those born between 1970 and 1974) in 1990 (when they were 15–19 years old). The %NB ranges from 2.1% for cohort 2 to 15.6% for cohort 14. Note, however, that the increase in this variable is not strictly linear, with cohort 6 (born between 1935 and 1939) having a value that is higher than that for the cohorts 7, 8, and 9 (born between 1940 and 1954). Finally, the measure of RCS varies from 10.5 to 15.3 for the fixed measure and 7.4 and 15.3 for the variable measure. The cohorts with the smallest relative size are 5 and 6 (born in the depression years of 1930–39) and cohorts 13 and 14 (born between 1970 and 1979). The cohorts with the largest relative size are those in the post–World War II baby boom (cohorts 8, 9, and 10, born between 1945 and 1959).

In our analysis, the set of age-period-specific homicide arrest rates associated with each cohort in each cell is the dependent variable; the various time periods, age groupings, and measures of RCS and %NB associated with each cohort are the independent variables. This approach allows us to associate changes in the dependent variable (age-period-specific homicide arrest rates) with particular cohort characteristics while controlling for both age and period.¹² Examining figure 1, we see that, for an analysis

¹² Including variables that measure cohort characteristics rather than dummy variables for each cohort avoids the problem of linear dependency that would occur if one included dummy variables for $T - 1$ of the time periods, $A - 1$ of the age group-

involving age-group dummy variables, period dummy variables, RCS, and %NB, there are $[(7 - 1) + (8 - 1) + 1 + 1 =]$ 15 independent variables to predict $[(7 \times 8) =]$ 56 age-period-specific homicide arrest rates. We test this model with both measures of RCS to see if our results are consistent across both of these operationalizations. We also test our model for data from 1960 through 1995 and for the period 1960–85. Examination of this shorter and earlier time period allows us to determine the extent to which our model can be generalized to an earlier time period that does not overlap with the epidemic in youth homicides.

Test for Autocorrelation

A feature of the age-period-cohort-characteristic model that has not been considered previously is the possibility of autocorrelation generated by cohorts. A cohort's homicide arrest rate "moves diagonally" from the upper left to the lower right in figure 1. The cohort labeled 7 in figure 1, for example, contributes seven observations to the data: the first in 1960 when it was ages 15–19 and the final one in 1990 when it was ages 45–49. This raises an important issue—the possibility that the residuals for cohorts may be correlated. Systematic differences in even a single cohort that affect its age-period-specific homicide arrest rates and are not predicted by the independent variables in the model will result in residuals for that cohort that exhibit a systematic relationship to each other. Anything about a particular cohort that makes its age-period-specific homicide arrest rates higher than expected, given the independent variables, will result in its residuals being positive.

We test for autocorrelation due to cohorts using the following procedure. There are some observations on cohorts that cannot be checked for autocorrelation. For example, both cohort 1 and 14 have only one observation, so we cannot check how much the residuals for cohort 1 are correlated with each other or how much the residuals of cohort 14 are correlated with each other. For all of the other cohorts, we can examine the correlation between one or more pairs of residuals. We do this by examining the correlation of the cohort residuals at time t with those at time $t - 1$. Using this procedure, we can correlate a pair of residuals for cohort 2 and a pair for cohort 13, two pairs of residuals for cohort 3 and two pairs for cohort 12, and so on. The total number of pairs of residuals correlated is $[(A \times T) - (A + T - 1)] = 42$, where A is the number of age groups and T is the number of periods. If there is a systematic under-

ings, and dummy variables for each of the $C - 1$ cohorts, a problem noted by Mason et al. (1973)

or overprediction of the homicide arrest rates for different cohorts, this procedure should result in a positive correlation between residuals.

Control Variables

An important strength of the age-period-cohort-characteristic model is the inclusion of dummy variables for both age groups and periods. From our perspective, these two sets of dummy variables are control variables. They control for age and period directly, and they control for other variables that are associated with age and period, thus providing a clearer indication of the effect of cohort-related variables. For instance, by including a dummy variable for period, we control for the effects of many variables not explicitly included in our model, such as the yearly unemployment rate, the number of police officers per capita, changes in media, and improvements in medical technology that might prevent deaths. The period dummies serve as a proxy for these and other variables that change over time. The same argument applies to the age-group dummy variables, which control for factors that are associated with age, such as physical strength, social maturity, and life stage.¹³

The inclusion of these dummy variables also controls for effects of variables associated with linear trends of cohort characteristics. This occurs because the age group and period dummy variables perfectly predict the cohort numbers (1, 2, . . . , 14) in figure 1. Thus, we have, in essence, included the time period in which the cohort was born as a control variable. If a cohort characteristic were related to the homicide arrest rate simply because both of these variables were linearly related to the time of the cohorts' birth, that effect would be controlled for. Together, the age dummies, period dummies, and the implicit inclusion of the time of the cohorts' births provide a very strong set of controls. As we shall see, these independent variables explain a large portion of the variation in age-period-specific homicide arrest rates without introducing our two cohort characteristics.

Two additional control variables are used in later analyses to test the robustness of our results. Since males are more likely than females to commit homicides, we add the age-period-specific sex ratio (number of males

¹³ More technically, we control for the effects of variables associated with periods that are constant across age groups and control for the effects of variables that are associated with age group that are constant across periods. To the extent that effects of some of these variables are only relatively constant across age groups (in the case of effects associated with periods) or across periods (in the case of effects associated with age groups), the control is incomplete. The controls extend to the effects of variables associated with period and age group that are not explicitly included in the equation.

per 100 females) for each age group and period as a control variable. Similarly, since African-Americans have higher homicide rates than other groups, we add the age-period-specific percentage of the population that is African-American as a control variable.

The confounding of age, period, and cohort makes it *necessary* to control for age and period in order to untangle the effects of cohorts. The best way to achieve this control is through dummy variable coding. This requires a set of 13 "control variables" and might lead to the charge that we have overfit our data, even in the basic model in which we include these dummy variables and just two variables representing cohort characteristics. We do not find the use of these 15 variables to be problematic. The 13 dummy variables were used as a set to allow us to examine the effect of cohort characteristics. We did not try different combinations of these variables in an attempt to provide support for our theory. Nor did we engage in a "search" for significant results by trying to fit our model with a number of cohort characteristics that we did not report. Controlling age and period effects using dummy variables makes it more difficult to find cohort effects.

Most important, controlling for age and period is essential to testing our theory, and we think that it is usually worse to leave out theoretically relevant variables than to include too many variables. As Johnston (1984) notes, "It is more serious to omit relevant variables than to include irrelevant variables since in the former case the coefficients will be biased, the disturbance variance overestimated, and the conventional inference procedures rendered invalid, while in the latter coefficients will be unbiased, the disturbance variance properly estimated, and the inference procedures properly estimated" (p. 262). In our case, we believe that all of the 15 variables in our basic model are relevant variables.

RESULTS

Table 1 presents the results of regressing the natural log of the age-period-specific homicide arrest rates on age group and period dummy variables, %NB, and the fixed version of RCS. Because age group and period are orthogonal in this design, the amount of variance that they explain in the homicide arrest rates is not dependent on which of the two sets of dummy variables is entered first into the regression equation. The R^2 associated with the set of period dummy variables is .09 ($P > .20$). The R^2 associated with the age-group dummy variables is .76 ($P < .001$). Together, the age and period dummy variables explain nearly 84.7% of the variance in logged age-period-specific homicide arrest rates (adjusted $R^2 = .79$). Adding %NB to the regression equation that contains only age and period dummy variables increases the explained variance to 97.6%, while adding

TABLE 1

REGRESSION ANALYSIS OF LOGGED AGE-PERIOD-SPECIFIC HOMICIDE ARREST RATES,
1960-95

VARIABLES	MODEL 1		MODEL 2	
	B	t	B	t
Intercept	- 200	-1.241	-.345	2.031
Period:				
1960712	10.819***	.750	11.379***
1965732	12.244***	.767	12.810***
1970	1.044	18.442***	1.074	19.055***
1975989	18.641***	1.015	19.319***
1980825	16.876***	.846	17.603***
1985515	11.748***	.529	12.398***
1990450	11.765***	.454	12.339***
1995000 ^a	.000 ^a	.000 ^a	.000 ^a
Age.				
15-19.....	.331	5.794***	.291	5.002***
20-24.....	.832	17.760***	.805	17.153***
25-29.....	.767	18.840***	.749	18.648***
30-34.....	.609	16.217***	.596	16.289***
35-39.....	.464	12.896***	.455	13.069***
40-44.....	.251	7.185***	.246	7.324***
45-49.....	.000 ^a	.000 ^a	.000 ^a	.000 ^a
RCS.....	.054	6.533***	.061	7.057***
%NB.....	.186	19.078***	.194	19.002***
Cohort 7.....	—	—	.059	2.060*
R ²989		.995
Adjusted R ²984		.990

NOTE.—N (number of cells minus one cell with missing data) = 55.

^a Dummy variable omitted for purposes of estimation* $P < .05$ (for two-tailed test).*** $P < .001$

the fixed version of RCS to the equation containing the age and period dummies increases the explained variance to only 87.4%. Each of these increases in explained variance is statistically significant ($P < .01$). Adding both %NB and RCS increases the explained variance to 98.9%.¹⁴

Controlling for the period and age-group dummy variables, both RCS and %NB are positively and statistically significantly related to the age-

¹⁴ One way to think about explaining this variance is to think of fig. 1 as a "contingency table." Note that we are explaining the age-period-specific homicide arrest rates or "cell rates." We do this with the "main effects" of the columns (age groups) and rows (periods) and explain the "interaction" with two cohort characteristics.

period-specific homicide arrest rates. To gain a sense of the extent of the relationship represented by these coefficients, we can transform the coefficients into "effective rates of return" (Stolzenberg 1979) or percentage change form. The RCS coefficient of .054 means a change of one unit in RCS is associated with a change in the natural log of the homicide arrest rate of .054. This is equivalent to an $(e^{.054} - 1) \times 100\%$ increase or a 5.6% increase.¹⁵ Since RCS is measured as the number in the cohort per 100 people in the age range 15–64 when the cohort was ages 15–19, we can say a one point increase in RCS (say from the cohort making up 8% of the population to 9%) is associated with a 5.6% increase in the homicide arrest rate (controlling for the other variables in the model). A similar interpretation is available for the coefficient associated with %NB. The regression coefficient of .186 means that an increase of one person born to an unwed mother per 100 live births is associated with an increase in the age-period-specific homicide arrest rate of $[(e^{.186} - 1) \times 100\% =] 20.4\%$.

We checked the residuals from model 1 for autocorrelation due to cohorts and found that the correlation between the lagged residuals (lag one) was .20. As described earlier, we calculated the lag-one correlations between all pairs of residuals where there was a preceding residual within the cohort: 42 pairs of residuals. An examination of the residuals indicated that this autocorrelation mainly was due to the residuals for cohort 7 (the cohort born between 1940 and 1944). All of the residuals for this cohort were positive; that is, the homicide arrest rates based on age, period, %NB, and RCS were underpredicted for this cohort in all seven periods. This suggests that there is something special about cohort 7 that made its homicide arrest rates higher than expected given the independent variables in model 1.¹⁶

In model 2, we included a dummy variable for cohort 7. This provided a test to determine if the under prediction for cohort 7 was statistically significant (albeit, we selected this cohort dummy variable on the basis of the residuals). The results for model 2 in table 1 indicate that the regres-

¹⁵ For example, if the age-period-specific homicide rate is 10, the natural log of that rate is 2.303. The unstandardized regression coefficient of .054 indicates that the predicted natural log of the homicide rate would increase by .054, or in this case it would be $(2.303 + .054 =) 2.357$. In terms of antilogs, the increase is from 10 to 10.55 or a 5.55% increase. Although we used an age-period-specific homicide rate of 10 in this case, we could have just as easily used a rate of 20. A .054 increase in the natural log is always a 5.55% increase.

¹⁶ The positive residuals associated with the 1940–44 birth cohort are consistent with our theoretical contentions that alterations in family structure can lead to diminished social capital for children that affect them throughout their life spans. This cohort may have experienced a high rate of father absence during infancy and early childhood due to the disruptions associated with World War II. But this, admittedly, is an ad hoc explanation.

sion coefficient associated with the dummy variable for cohort 7 is statistically significant at the .05 level. If this cohort is mainly responsible for the correlated residuals, this cohort dummy variable should substantially reduce the autocorrelation. This is important, because the correlated residuals bring into question the significance tests used in model 1. When we checked the autocorrelation due to cohorts for model 2, the correlation between the residuals was only .04. It is important to note that other regression coefficients in model 2 and their associated *t*-values were affected only slightly by the inclusion of the dummy variable for cohort 7.

The relationship between homicide arrest rates and age from 1960 to 1995 is depicted in figure 2, where the solid line represents the observed homicide arrest rates. Note the near doubling of rates for those ages 15–19 between 1985 and 1990 and the startling increase for those ages 20–24 between 1985 and 1990. These rates remained at nearly as high a level in 1995.

The dashed line in figure 2 indicates the predicted value of the homicide arrest rates based on a regression equation containing only the age and period dummy variables.¹⁷ If the relationship between age and homicide arrest rates were invariant across periods (i.e., if the relative rate of homicide arrests remained constant for the different age groups), then these predictions would perfectly fit the observed rates. The estimated values dramatically under predict the homicide arrest rates for those ages 15–19 and ages 20–24 in 1990 and 1995. Otherwise, the predictions do not seem to be “too bad” based on a visual inspection. Graphically, this is the problem faced by scholars who attempt to explain the sudden increase in youth homicide and why that sudden increase has been referred to as an epidemic (Zimring 1996): the relationship between age and homicide rates had been very stable up until 1985.

The stippled line in figure 2 represents the predictions based on model 1 in table 1—the model that includes the age and period dummies in addition to relative cohort size and the percentage of nonmarital births. This model does a much better job of predicting the sudden increase in youth homicides, although it underestimates the rate of 15- to 19-year-olds in 1990 by about 6 per 100,000. Nevertheless, the estimated and observed rates are quite close. Note that including RCS and %NB improves predictions of rates throughout *all* of the age groups covered in our analysis, not just the youngest age groups. For example, the predictions for age groups over 30 in 1995 are much better given estimates based on the full model as are those for the young age groups in the earlier periods.

¹⁷ We took antilogs of the predicted values from our regression model to transform our logged predicted values into predicted homicide arrest rates per 100,000.

FIGURE 2: PREDICTED AND OBSERVED HOMICIDE ARREST
RATES FOR AGE GROUPS FROM 1960 TO 1995

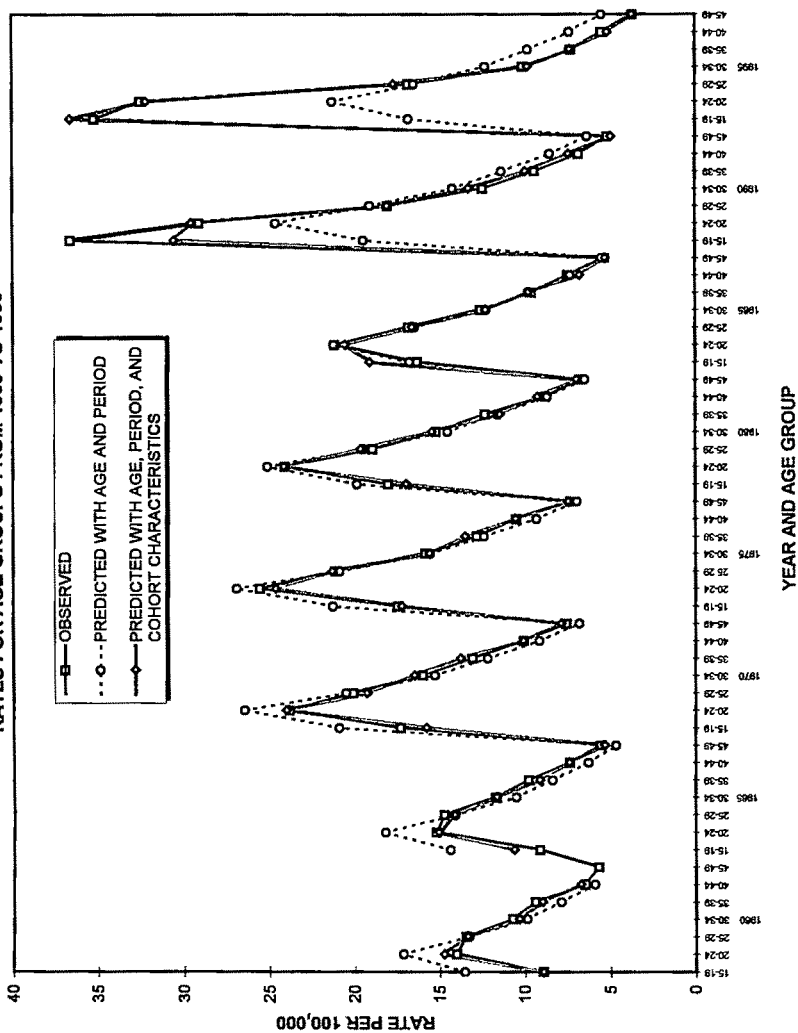


Fig. 2.—Predicted and observed homicide arrest rates for age groups from 1960 to 1995

Some Additional Tests

Kahn and Mason (1987) and Steffensmeier et al. (1992) have argued that the effects of RCS should be especially pronounced for those who are young, although our theory is mute on this point. We tested this by interacting ages 15–19 and ages 20–24 with RCS and with %NB. This resulted in four interactions that indicate whether the effects of RCS and %NB are especially large for those in the two youngest age groups. All four of these interactions are positive. The coefficients for the interactions of ages 15–19 and ages 20–24 with %NB were statistically significant at the .05 level (one-tailed tests), and the main effects for RCS and %NB remained statistically significant ($P < .005$) when the interaction effects were included. This means that although the effects of RCS and %NB are stronger for those ages 15–24, these effects remain positive and are statistically significant for the remaining age groups.

When we conducted the same series of analyses reported in table 1 but used the variable measure of RCS, the percentage of the population from ages 15–64 during a particular period, that is, in the cohort's five-year age group during that period, the results were nearly identical. The %NB and RCS were both strongly related to the log of the age-period-specific homicide arrest rates, the cohort variable for those born from 1940 to 1944 was statistically significant, and the same four interactions described in the preceding paragraph were positive. The coefficients for ages 15–19 and for ages 20–24, interacted with %NB, were both statistically significant at the .05 level, and the main effects for RCS and %NB remained statistically significant ($P < .005$) even with these interaction terms in the model.

We tested two other specifications for our model using data from the 1960 to 1995 periods. Using age-period-specific homicide rates in unlogged form yielded results that were substantively similar to those reported in table 1. Again, RCS and %NB were statistically significant in the full model, and the dummy variable for cohort 7 was statistically significant. We also added age-period-specific measures of the sex ratio and the percent African-American as independent variables in the full model (model 2 of table 1). As noted above, both of these variables might well account for variations in age-period-specific homicide arrest rates, since males are more likely to commit homicides than females and African-Americans have higher homicide rates than other groups. Controlling for age, period, RCS, and %NB, neither the sex ratio nor the percent African-American was a statistically significant predictor of homicide arrest rates in this full model. More important, including these two additional control variables did not substantively change the results reported in table 1. The effects of both RCS and %NB remained statistically significant, and the unstan-

standardized regression coefficients were of nearly the same magnitude. (These results are available from the authors on request.)

We do wish to insert a note of caution regarding the results obtained when the interaction effects and these additional control variables are added to our basic model. While there is not a problem with multicollinearity in our basic model, the addition of the other controls such as the sex ratio and the percentage black produce regression diagnostics that indicate the presence of serious multicollinearity. The introduction of the interactions of age groups with our two cohort measures produces what we believe is an excessive amount of multicollinearity.¹⁸ Thus, while the main effects of RCS and %NB remain consistent and strong across all of our analyses, the coefficients associated with the additional control variables should be interpreted with care.

Finally, and perhaps most important, if our suppositions concerning the effects of RCS and %NB on homicide arrest rates are correct, we should find these relationships prior to the dramatic increases in youth homicides that occurred in the mid-1980s. The relationships might be weaker (in terms of statistical significance) given both the reduction of variability in homicide arrest rates and the number of cases that accompany such a truncation of the data, but they should be present. To test this, we used our model to predict homicide arrest rates for the years preceding the dramatic recent increase in youth homicide arrest rates: the years from 1960 to 1985. The results appear in table 2.

¹⁸ We assess multicollinearity with the variance inflation factor [$VIF = 1/(1 - R_k^2)$] and its square root. The square root of VIF indicates the degree to which the SE of an independent variable's regression coefficient is inflated due to multicollinearity. If the k th independent variable had a squared multiple correlation of .75, when regressed on the other independent variables in the analysis, it would have a VIF of 4 [$=1/(1 - .75)$]. This indicates that the SE of the regression coefficient for the k th variable is twice as large as it would have been if there had been no multicollinearity between it and the other independent variables. In our basic model (model 1 of tables 1 and 2), the greatest inflation of a SE due to multicollinearity is 2.77: associated with non-marital births. Squaring this coefficient yields a VIF of 7.67. Although this degree of collinearity indicates some degree of inefficiency in our estimation, Rawlings (1988, p. 277) and others cited in Rawlings suggest that serious collinearity problems do not occur when the VIFs are less than 10. The VIFs do not exceed 10 for any of the models in tables 1 or 2. Adding the other controls results in serious multicollinearity. When the sex ratio and the percentage black are added to model 2 in table 1, the VIF for %NB rises to 11.39. Of more concern, the VIF for percentage black is 17.43. This degree of inflation might help explain why the coefficient associated with this variable is not statistically significant. When the interactions for age groups by %NB and age groups by RCS for the two youngest age groups are added to model 2 in table 1, the VIFs are greater than 100 for the two youngest age groups and for both the age group by RCS interactions. The VIF for %NB in this model is 62.40. As argued by O'Brien and Gwartney-Gibbs (1989), the use of these sorts of interactions raises difficult issues concerning the interpretation of the partial regression coefficients.

TABLE 2

REGRESSION ANALYSIS OF LOGGED AGE-PERIOD-SPECIFIC HOMICIDE ARREST RATES,
1960-85

VARIABLES	MODEL 1		MODEL 2	
	B	t	B	t
Intercept567	3.167**	.553	3.591**
Period:				
1960	100	1.736	.075	1.501
1965135	2.687**	.124	2.872**
1970460	10.250***	.451	11.655***
1975420	10.684***	.413	12.178***
1980278	8.245***	.273	9.403***
1985000 ^a	.000 ^a	.000 ^a	.000 ^a
Age				
15-19426	6.463***	.453	7.913***
20-24915	17.529***	.937	20.646***
25-29840	18.754***	.861	22.051***
30-34662	16.852***	.683	19.866***
35-39509	13.950***	.528	16.547***
40-44274	7.871***	.278	9.270***
45-49000 ^a	.000 ^a	.000 ^a	.000 ^a
RCS048	5.675***	.052	7.014***
%NB142	7.116***	.134	7.760***
Cohort 8	—	—	-.084	3.245***
R^2988		.991	
Adjusted R^2982		.987	

NOTE.—N (number of cells minus one cell with missing data) = 41

^a Dummy variable omitted for purposes of estimation.* $P < .05$ (for two-tailed test)** $P < .01$ *** $P < .001$

Unlike the full data set, the adjusted R^2 for the age and period dummies explains a greater percentage of the variance in homicide arrest rates for these data: adjusted $R^2 = .95$. This is not surprising given the data shown in figure 2, where the relationship between age and homicide arrest rates is quite consistent across periods until 1990. Thus, the age and period dummy variables are able to "predict" homicide arrest rates rather well during this limited time frame. However, both RCS and %NB are positively and statistically significantly ($P < .001$) related to the age-period-specific homicide arrest rates. Again, the model fits the data extremely well (adjusted $R^2 = .98$). This means that even in this era the relationship between age and the homicide arrest rate was not invariant and that we can account for a statistically significant portion of the variance in that relationship using RCS and %NB.

One difference between the 1960–95 period and the 1960–85 period is that in the latter period the autocorrelation due to cohorts is generated by cohort 8, the cohort born between 1945 and 1949. Members of this cohort have lower homicide arrest rates than expected given the independent variables in model 1.¹⁹ Including the dummy variable for this cohort in the model reduced the autocorrelation from .21 to .10. The results from this analysis are presented as model 2 in table 2. It is important to note that the size of the relationships of RCS and %NB to the homicide arrest rate, as well as the statistical significance of those relationships, shows little change from model 1 to model 2 in table 2.

DISCUSSION

Age groups and periods explain much of the variation among age-period-specific homicide arrest rates from 1960 to 1995 (adjusted $R^2 = .79$). This is not surprising, given the well-known relationship between age and crime. A greater challenge to researchers involves explaining *changes* in the relationship between age-period-specific homicide arrest rates and age across periods. Our results indicate that two characteristics of birth cohorts can account for changes in the relationship between homicide arrest rates and age from 1960 to 1995. A model that contains age group and period dummy variables, in addition to the percent nonmarital births and relative cohort size, results in an adjusted R^2 in excess of .98. The effects of these two cohort characteristics are important, even after controlling for age and period effects. Further, these two cohort characteristics help explain age-period-specific homicide arrest rates both before and after the dramatic increase in youth homicide, indicating that our model is not specific to the most recent period.

Theoretical Issues

Family structure and crime.—These results lend support to both macrolevel and microlevel theories regarding the importance of alterations in family structure in explaining criminal propensities. At a macrolevel, when birth cohorts include relatively more children born to unwed mothers or when birth cohorts are relatively large, families, schools, churches, and neighborhoods must stretch their resources. Most children within these birth cohorts, no matter how large their own family or the marital

¹⁹ By 1985, not enough time had passed for either cohort 7 or 8 to have traced their life courses as represented in fig. 1. Cohort 7 is 40–44, and cohort 8 is only 35–39. Yet, as shown in fig. 1, cohort 8 had the lowest age-period-specific homicide arrest rate for any time and period included in our analysis (3.67 in 1995).

status of their parents, are likely to feel some impact of the diminished family and community resources available to members of their cohort. Just as all children who live in communities with higher levels of social disorganization are affected by higher rates of family disruption and other indicators of social disorganization (see Sampson and Wilson 1995, p. 44), all children in these birth cohorts seem to be at higher risk. To use Vila's (1994) terminology, people born into these birth cohorts may be more likely than people in other birth cohorts to adopt a criminal strategic style because they experience different social environments (see esp. p. 324).

At the microlevel, it is not easy to separate the effects of childhood poverty, adult supervision and monitoring, and other factors that may account for the strong association between nonmarital births and homicide rates. Poverty certainly plays a role in denying resources to children for day care, after-school care, medical care, and other necessities. Our results are consistent with the self-control aspects of the theories of Gottfredson and Hirschi (1990), Moffitt (1993), Patterson and Yoerger (1993), and Sampson and Laub (1993) that stress the importance of early socialization within the family. The results also fit well with social structure and personality theories that point to the importance of father-son ties in stemming inappropriate aggressive tendencies among males (Parsons 1954; Coltrane 1988, 1992). We suggest that at the individual level peer socialization and a lack of closure can play an important role in reinforcing and extending the effects of RCS and %NB beyond the families in which these births occur.

Cohort characteristics.—While both of our cohort characteristics are significantly related to age-period-specific homicide arrest rates, %NB has a much stronger association. For example, in model 1 of table 1, a change of one unit in the %NB results in a 20% change in the estimated homicide arrest rate, while a change of one unit in the percentage of those ages 15–64 who were ages 15–19 when the cohort was 15–19 results in a 6% change. In terms of the impact that these two variables have had from 1960 to 1995, it is important to note that the range of variation of RCS is much smaller than the range of variation in %NB. Across the cohorts in our sample, RCS has ranged from 10.53 to 15.33 (with an SD of 1.77), while %NB has ranged from 2.10 to 15.59 (with an SD of 4.05). Thus, an SD change in RCS is associated with an increase in the predicted homicide arrest rate of 10% [= $(e^{0.54 \times 1.77} - 1) \times 100$]. A one-SD change in %NB is associated with an increase in the predicted homicide arrest rate of 115% [= $(e^{1.86 \times 4.05} - 1) \times 100$].

Both RCS and %NB reflect changes in family structure. Yet, following the work of Easterlin (1978, 1987), most analyses of cohort variation focus on RCS and exclude other cohort-related variables. Our analysis raises

the possibility that these other cohort characteristics, such as %NB and conceivably others, may provide even more effective explanations of variations between cohorts. It is possible that these other cohort characteristics are especially important in explaining traits that are closely linked to childhood experiences, such as criminal behavior, academic achievement, and occupational aspirations. Given the strength of the relationship between %NB and the age-period-specific homicide arrest rate in our analysis, examining the relationship between RCS and this rate, without including %NB, is likely to be an incorrect specification of the model underlying this relationship.

Age and crime.—Our results also speak to the controversy surrounding Hirschi and Gottfredson's (1990) hypothesis regarding the invariant relationship between age and crime: "The age effect is everywhere and at all times the same" (p. 124). Controversy has surrounded this hypothesis (Cohen and Land 1987; Greenberg 1985; Farrington 1986; Blumstein, Cohen, and Farrington 1988; Steffensmeier et al. 1989) since first introduced in its most extreme form by Hirschi and Gottfredson in 1983.²⁰

If we compare the homicide arrest rates by age for each of the periods depicted in figure 2 with a simple conception of the invariance hypothesis, we would conclude that the invariance hypothesis is not supported. After all, in 1960, 1965, 1970, 1975, 1980, and 1985, the highest arrest rates for homicide were for those ages 20–24, but this changed dramatically in 1990 and 1995, when the highest rates were for those ages 15–19. This same shift in the age-period-specific homicide arrest rate relationship is seen for the age-period-specific homicide rate data based on crimes known to the police from the UCR. These data contributed to the initial concern about the *epidemic* of youth violence. Further, if we compare rates in 1960 and 1995 for those ages 45–49, we see a slight drop for this age group, but there is a dramatic increase in the rates from 1960 to 1995 for those ages 15–19. To the extent that the validity of the invariance hypothesis hinges on the observed rank order of age-specific homicide rates (arrests or homicides known to police) or the observed ratio of rates between age groups across periods, these results do not support the invariance hypothesis.

From a different perspective, however, the results from our analyses may be seen as consistent with the invariance hypothesis, especially with the following caveat of Gottfredson and Hirschi (1990): "So, although we may find conditions in which age does not have as strong an effect as usual, the isolation of such conditions does not lead to the conclusion that

²⁰ It is clear from their discussion that this invariance concerns the distribution of crime rates and not their absolute size. Many of their examples of invariance involve graphs of age-period-specific arrest rates, age-period-specific conviction rates, and so on, because of the unavailability, in many cases, of age-specific rates for offenders.

age effects may be accounted for by such conditions. On the contrary, it leads to the conclusion that in particular cases the age effect may be to some extent obscured by countervailing crime factors" (p. 128). When we control for period, %NB, and RCS (and other variables in the different models presented in this paper), the age effects follow a familiar pattern: the greatest effect is for 20- to 24-year-olds. Beyond that age, the rates decrease for each age group in our data.²¹ From this perspective, shifts in the observed distribution of age-period-specific homicide arrest rates are associated with cohort characteristics that obscure the age effects. Thus, once the effects of RCS, %NB, and period are controlled, as in our model, the pattern of age effects in recent years conforms to that predicted by Hirschi and Gottfredson (1983).

Future Research and Limitations

It always is important to replicate research, but this may be particularly crucial for the research reported in this paper. The importance of RCS and %NB in explaining age-period-specific rates of homicide arrests may depend on the unusual amount of variation in both of these variables over the time period covered in this research. The postwar baby boom was much larger in the United States than in most European countries. In addition, the United States has relatively high rates of single parenthood, especially among young women and women who are not in stable relationships. An important question involves the extent to which our model would fit patterns of age-period-specific homicide rates in societies with different underlying patterns on these independent variables.

Additionally, we can ask whether, with the same amount of variation in RCS and %NB, the effects of these variables would be as strong in other settings. Pampel and his associates (e.g., Pampel and Peters 1995; Pampel and Gartner 1995) have described conditions under which the importance of cohort size may rise or fall in response to other forces of social change. These include the sexual division of labor in the family, immigration, the role of governments in smoothing business cycles, and the extent to which nations adopt collectivist policies regarding social benefits. Pampel and Gartner (1995) provide evidence that the relationship between the age structure of the population and the homicide rate is weaker in societies that have stronger collectivist social institutions (see also Gartner and Parker 1990).

Part of this cross-national variability in political practices involves the

²¹ This conclusion is based on the size of the coefficients associated with the dummy variables for age groups in each of the models in tables 1 and 2.

extent to which societies provide public support for children. For instance, most industrialized countries provide some sort of family child allowance and higher welfare payments, resulting in much lower levels of child poverty than in the United States. Many of these countries also have more extensive programs of paid maternity leaves, often for a period of several years (Bergmann 1993, 1996; Hewlett 1992; Pampel 1994). It is quite possible that in such societies %NB will have less of an impact on the homicide rates of cohorts.

On the other hand, given the results of studies of income maintenance programs in the United States (Hannan, Tuma, and Groeneveld 1977, 1978) and commentaries on the decline of the family in the Swedish welfare state (Popenoe 1987, 1988, 1991), it is also possible that such governmental programs cannot provide the resources that help foster the higher levels of both informal social control and self-control needed to combat criminogenic tendencies within birth cohorts. To test these competing views, it is important to replicate our model in societies in which governments attempt to provide greater resources for children.

As noted earlier, discussions of the consequences of RCS have focused on both job market effects and those more directly related to family and community resources and support. The recent increases in homicide arrest rates have generally involved people too young to have much experience within the job market. In addition, our findings indicated that the effects of RCS and %NB persist throughout the life course, including years when relatively few cohort members are actively searching for jobs. As a result, we suggest that our results generally lend support to theories regarding the importance of alterations in family structure for the years examined in our analysis; however, further research should be done that separates job market effects from those effects more directly related to family and community support.

Policy Implications

The youngest cohort in our analysis was born between 1975 and 1979. This particular birth cohort was not especially large relative to others in the sample (RCS = 10.5) but had the largest %NB (%NB = 15.6). It also had the second highest age-period-specific homicide arrest rate (35.2). Cohorts born since 1979 have been somewhat larger and have had substantially higher percentages of nonmarital births. For instance, 18% of all births in the United States in 1980 were to unmarried women, but this figure increased to 22% by 1985 and to 27% by 1990. The %NB for those born between 1990 and 1994 was 30. Given the continued growth in %NB and slightly increased size of younger cohorts, we expect the relatively recent pattern of an extremely large gap between the younger and

the older age groups with regard to homicide will continue in the near future.

What might be done to ameliorate the loss of community and family resources available to younger cohorts? Our analysis appears to lend support to those who propose that "nurturant strategies," which focus on childhood, may be much more effective in the long-range control of criminality than either "protection" or "deterrent strategies" (see Vila 1994, p. 337; Sampson and Wilson 1995). It is perhaps worth quoting from Gottfredson and Hirschi (1990): "In our view, the origins of criminality and low self-control are to be found in the first six or eight years of life, during which time the child remains under the supervision of the family or a familial institution. Apart from the limited benefits that can be achieved by making specific criminal acts more difficult, policies directed toward enhancement of the ability of familial institutions to socialize children are the only realistic long-term state policies with potential for substantial crime reduction" (p. 272). These policies might include some sort of family child allowance, higher welfare payments, more extensive programs of paid maternity leaves, and subsidized, high-quality child care. Schools also serve as a "familial institution," and increased support for schools and other institutions that provide support for children could be beneficial.

A different strategy involves creating the conditions that might lead to fewer single-parent families. A number of studies attribute the sharp increase in nonmarital births since the 1970s to the declining wages and high unemployment rates of young men, especially those with low levels of education (Cready, Fosset, and Kiecolt 1997; Lichter, LeClere, and McLaughlin 1991; Lichter et al. 1992; South 1996; Tucker and Mitchell-Kernan 1995; Wilson 1980, 1987).

Summary

Our results provide a precise and parsimonious explanation of recent changes in age-period-specific homicide rates, and explaining these changes motivated this research. We suggest that cohorts may experience different levels of family and community resources. Two measures that tap family and community resources are used in an age-period-cohort-characteristic model. The model controls for the effects of age groups and periods and variables that are associated with these categories as well as the linear effects of the time at which the cohorts were born (1910–14 to 1975–79). The results suggest that past and recent changes in age-period-specific homicide arrest rates are closely associated with cohorts' relative size and the percentage of nonmarital births. The effects of these cohort characteristics are both statistically and substantively large. In addition,

the data indicate that their effect on homicide arrest rates lasts throughout the cohort's life course.

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Low Socioeconomic Status and Mental Disorders: A Longitudinal Study of Selection and Causation during Young Adulthood¹

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This article examines low socioeconomic status (SES) as both a cause and a consequence of mental illnesses by investigating the mutual influence of mental disorders and educational attainment, a core element of SES. The analyses are based on a longitudinal panel design and focus on four disorders: anxiety, depression, antisocial disorder, and attention deficit disorder. The article shows that each disorder has a unique relationship with SES, highlighting the need for greater consideration of antisocial disorders in the status attainment process and for further theoretical development in the sociology of mental disorders to account for disorder-specific relations with SES.

Mental disorders are overrepresented in the lower social strata (Kessler et al. 1994; Dohrenwend et al. 1992; Link and Dohrenwend 1989; Neugebauer, Dohrenwend, and Dohrenwend 1980; Wheaton 1978; Holzer et al.

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1986). Efforts to distinguish low socioeconomic status (SES) as a cause or consequence of mental disorder address some of the most vexing problems in social demography and medical sociology. On the one hand, mental disorders may play an important role in determining who gets ahead in society, a topic pursued by sociological research in the "selection" tradition that examines the extent to which disorders impair status attainment (Eaton 1980; Eaton 1985; Dohrenwend 1975). On the other hand, adversities linked to low SES may damage the psychological functioning of individuals and play a role in the etiology of mental disorders, a topic pursued by sociological research in the "causation" tradition (Turner, Wheaton, and Lloyd 1995; Link, Lennon, and Dohrenwend 1993; Kohn 1981; Wheaton 1978). Despite 50 years of research, key theoretical issues regarding the causal direction between low SES and mental disorders still remain unsettled (Dohrenwend et al. 1992; Fox 1990; Ortega and Corzine 1990).

In this article, we examine the selection and causation hypotheses by using a panel study to investigate the association between mental disorders and educational attainment, a key component of social status (Sewell, Hauser, and Featherman 1976). Our prospective study of adolescents as they make the transition to adulthood allows us to evaluate the temporal ordering of mental disorders and educational attainment more directly than past studies. As a consequence, we are able to test whether the relation between educational attainment and nonpsychotic mental disorders results from selection, social causation, or a combination of both processes—three alternative interpretations that have not yet been disentangled with the research designs used in the current literature. The data come from the Dunedin Multidisciplinary Health and Development Study ($N = 1037$), which has followed a group of children from birth to age 21 and includes psychiatric diagnoses for all study members at ages 15 and 21 using criteria from the Diagnostic and Statistical Manual of Mental Disorders (the DSM-III and DSM-III-R; see APA 1980, 1987).

To the extent that we find evidence for selection effects, our results speak to mental disorders as an important yet neglected factor in the process of status attainment. In general, social demographers have not been interested in studying mental disorders. This is surprising, however, be-

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cause mental disorders are not uncommon in the general population, and they may play an important role in maintaining social inequalities. Although some disorders, such as schizophrenia, are indeed rare, affecting only 1% of the population, epidemiological studies reveal that the prevalence rate of mental disorders in young adulthood is alarmingly high and that 25%–40% of young adults in industrial countries meet diagnostic criteria for a psychiatric disorder. These high rates are of special concern for at least three reasons. First, afflicted young adults are not trivially afflicted but are indeed functioning more poorly than their nondisordered peers (Newman et al. 1996). Second, rates of psychiatric disorder far exceed the rates of service use; it is estimated that only 10%–30% of cases with disorder receive any treatment (Institute of Medicine 1994). Third, the onset of most disorders peaks between adolescence and young adulthood (Institute of Medicine 1994), a developmental period when disorders have the potential to disrupt educational transitions and thereby compromise this initial and consequential first step in the status attainment process. If mental disorders are as common as psychiatric epidemiological studies suggest, and if they are as nontrivial as the data also show, they may well be an important source of variation in determining social inequalities.

By testing whether causation acts alone or jointly with selection effects, our results are also important for specifying how SES influences mental health, a central issue in the social stress literature. If social causation acts alone, it suggests that people from lower-SES environments are “battered and bruised” by SES-linked social stressors and end up with higher levels of mental disorders. If we find joint effects, in contrast, this would suggest a more complex process whereby SES-linked mental disorders lead afflicted individuals to fall further into the lower social strata and, over time, become further exposed to social stressors that foster disorders, a cycle of disadvantage that can accumulate over the life course (Caspi, Elder, and Bem 1987). Current research investigating the SES-based social stressors that induce disorders such as anxiety and depression operates under the assumption of exclusive social causation (Turner et al. 1995; Aneshensel 1992), but the need to revisit this assumption—which is based on only scarce longitudinal evidence—is suggested by recent research that contradicts it (Kessler et al. 1995). Further, it is not known if this assumption of exclusive social causation applies to other disorders. In this study, we address the call to expand stress research into disorders other than anxiety and depression (Aneshensel, Rutter, and Lachenbruch 1991) by investigating selection and causation in regard to conduct disorder and attention deficit disorder, illnesses that are associated with troubled relations with peers, parents, schools, and other institutions during the

transition to adulthood (Robins 1966; Weiss and Hechtman 1986; Elliott 1974).

SELECTION AND CAUSATION

The Hypotheses

Research in the "selection" perspective suggests that mental disorders are overrepresented in the lower socioeconomic strata as a consequence of impaired social mobility. Selection processes operate both within and across generations. Within a generation, mental disorders may cause downward mobility among adults and lead them to "drift" into the lower socioeconomic strata (Jarvis 1971, pp. 55–56; Eaton 1980). Across generations, a disorder may be transmitted to offspring, as suggested by recent twin and adoption studies that indicate a role for both genetic and environmental factors in mental disorders (Kendler et al. 1995). A transmitted disorder that impairs status attainment may have cumulative effects across successive generations of a lineage, ultimately leading to the creation of a "residue" of people with mental disorders in the lower socioeconomic strata (Gruenberg 1961; Dohrenwend et al. 1992). All studies that investigate selection processes are characterized by a causal arrow that points from mental disorders to SES, unlike research in the causation perspective, which points the arrow the other way.

Research on the causation hypothesis suggests a wide array of mechanisms through which SES may affect psychopathology. These mechanisms may either cause illnesses or serve as catalysts to people with genetic predispositions for disorders, and they include stress (Turner et al. 1995; Kessler 1979; Pearlin and Johnson 1977; Link, Dohrenwend, and Skodol 1986), poor social or psychological coping resources (Kohn 1981; Kessler and Cleary 1980; Dohrenwend and Dohrenwend 1970; Liem and Liem 1978), and lack of occupational direction, control, and planning (Link et al. 1993). While the very different processes of selection and causation might at first appear easily distinguishable, disentangling their effects has proven an elusive goal.

Selection and causation research to date has faced two main obstacles that have prevented a clear test between the interpretations of causation, selection, and combined effects. First, the temporal ordering of mental disorders and low SES has been difficult to establish without longitudinal research designs. Second, the absence of a standardized psychiatric nosology has hindered the ability of researchers to compare findings about social status and mental disorders across different studies that use different measures. In the present study, we address these two limitations by conducting a longitudinal analysis of selection and causation and by examin-

ing these processes in relation to multiple, specific mental disorders diagnosed in accordance with a standardized nosology.

The Research Strategies

The study of selection and causation centers on temporal ordering and whether the onset of mental disorders occurs before or after low socioeconomic experience: evidence that it occurs before provides strong support for selection, while evidence that it occurs after is consistent with a social causation interpretation. With temporal ordering in mind, an initial research strategy focused on whether adults with mental disorders came disproportionately from lower-SES family backgrounds. If they did, this evidence suggests that low socioeconomic experience preceded the onset of mental disorder and, consequently, favors a causation interpretation. Yet, while many studies find that the parents of people with mental disorders are concentrated in the lower social strata (see, e.g., Kessler et al. 1994), the interpretation of this finding is more complex than originally expected. For example, an illness such as conduct disorder may impair a person's status attainment and then be passed on to offspring, either through parental socialization or through increased exposure to socioeconomic-based stressors that foster the illness. In this case, the finding that children with conduct disorder are overrepresented among parents with low SES stems both from causation processes that operated on the child, and, in addition, selection processes that operated on the parents (Dohrenwend 1975). In short, this initial research strategy can suggest preliminary evidence for causation effects, but it is limited because it does not evaluate their size relative to selection effects, nor does it determine whether causation acts to the exclusion of selection.

Subsequent research has pursued two main research strategies to determine the temporal order between mental disorders and SES. The first relies on retrospective reports of study members. Kessler et al. (1995), for example, sought to determine the temporal order between mental disorders and educational dropout by relying on the respondents' ability to recall their mental state before major educational transitions, such as high school completion and college entrance. On average, study members were asked to recall their mental state 15 years before the date of interview. Using these retrospective reports, they find support for the selection perspective to the extent that study members who failed to make major educational transitions reported an overrepresentation of anxiety disorders, mood disorders (including depression), and conduct disorder that predated their educational dropout. Conceivably, this research design could be extended to measure simultaneously the influence of causation processes. It

is important to note, however, that conclusions based on analyses using this research design rest on the assumption that retrospective reports of the occurrence and timing of childhood and adolescent mental disorders are valid and reliable, a matter of considerable debate (see Aneshensel et al. 1987; Henry et al. 1994; Rogler, Malgady, and Tryon 1992; Holmshaw and Simonoff 1996).

The questionable validity of retrospective reports has led some researchers to forgo them and to pursue a second research strategy instead. This approach does not measure temporal order directly but rather infers it from the insight that ethnic discrimination has distinctly different consequences for selection and causation outcomes (for a detailed discussion see Dohrenwend [1975] and Dohrenwend et al. [1992]). In brief, the selection perspective leads to the expectation that disorders will have smaller, diluted prevalence rates across all SES levels of disadvantaged groups because ethnic discrimination hinders social mobility and keeps greater numbers of healthy members at lower SES levels. In contrast, the causation perspective leads to the competing expectation that disorders will have higher prevalence rates among disadvantaged groups, who presumably experience higher stress levels. Analysis employing this research design supports the presence of causation processes for nonpsychotic mental disorders such as depression and antisocial personality disorder (Dohrenwend et al. 1992) but provides little information regarding the possibility that smaller selection effects are operating at the same time. The design succeeds in evaluating the relative strength of selection and causation, but it fails to provide a test to determine if either process is operating exclusively. This limitation is critical because the assumption of exclusive social causation underpins much of the social stress literature.

A longitudinal research design offers the opportunity to test between selection, causation, and combined-effects interpretations without relying on retrospective information. However, such prospective studies are rare due to the cost and logistics involved in repeated evaluations of large samples. Wheaton (1978) conducted one of the few panel studies of psychological disorder and social status, using occupational prestige to index social status in the adult years. Relying on a general measure of mental illness that combines symptoms of both anxiety and depression (the Langner index, see Langner 1962), he finds evidence for causation effects and no evidence for selection effects among adults. More recent longitudinal research supporting causation in regard to clinical levels of depression and anxiety comes from the study of Bruce, Takeuchi, and Leaf (1991), based on a six-month follow-up of 5,000 adults ages 18 and over. The existing field of longitudinal research leaves room for advances in at least two ways, however, as we discuss below.

The Transition to Young Adulthood

The transition from adolescence to adulthood has not yet been examined with a longitudinal research design, although adolescence is a strategic time period for the study of both selection and causation. In terms of selection, adolescence is a period when individuals make decisions about their educational attainment and, consequently, their social status. Educational attainment is both itself a primary component of SES indices and also a major predictor of subsequent income and occupational prestige over the life course (Blau and Duncan 1967; Sewell, Hauser, and Featherman 1976; Hauser 1994; Jencks et al. 1979). Indeed, as an initial and powerful step, education is the "key" factor in the status attainment process (Sewell and Hauser 1976, p. 13). Through limited educational attainment, adolescents may have already selected themselves into the lower social strata before adulthood, a process that represents selection effects to the extent that it is influenced by adolescent mental illness.² In the present study, we investigate this potential process in the framework of a longitudinal study designed to capture the transition from adolescence to adulthood.

In terms of causation, the peak risk period for the emergence of new cases of DSM mental disorders is during the transition from adolescence to young adulthood (Burke et al. 1990; Institute of Medicine 1994), suggesting that the factors that cause mental disorders are especially influential during this developmental period of the life course. Young adult cohorts have higher DSM period-prevalence rates than older cohorts in national cross-sectional studies (Kessler et al. 1994; Robins and Regier 1991), and the Dunedin cohort analyzed in this study shows a large increase (almost twofold) in the rates of mental disorder between ages 15 and 21 (Newman et al. 1996). Family SES origins appear to play an influential role during this formative period, as the literature on child and adolescent psychopathology documents a significant association between SES and mental disorders (Costello et al. 1997; Velez, Johnson, and Cohen 1989; Rutter et al. 1974). However, this literature has yet to examine the association in detail and to separate causation from selection effects, and, as such, relatively little is known about the SES-linked factors that are

² Wheaton (1978) presents one finding regarding mental illness and educational attainment using a longitudinal design, but it is evidence that he highly qualifies. One of the cohorts he studied was initially enrolled at age 17 and presented, in theory, the opportunity to measure the effects of psychological disorder on later educational attainment. Unfortunately, it was necessary for Wheaton to produce an ad hoc psychological scale from the baseline survey of this cohort. While analysis using the scale suggests that psychological disorder impairs educational attainment, as Wheaton points out (1978, p. 394), there is little certainty about what psychological domain the scale measures.

associated with increased risk of mental disorder from adolescence to young adulthood (Institute of Medicine 1994).

Are Selection and Causation Effects Disorder Specific?

We examine the ways in which selection and causation processes vary across different mental disorders in the transition to adulthood, a topic that has not yet been examined with a longitudinal research design. Current research suggests that different mental disorders are related to social status in different ways (Dohrenwend et al. 1992). Detailing differences across disorders can better inform selection and causation theories, many of which are not disorder specific but have only been evaluated with regard to depression or anxiety (Aneshensel, Rutter, and Lachenbruch 1991). The standardized DSM classification system that we use in this study is well suited for an investigation of disorder-specific relations with socioeconomic status because it provides formal guidelines to distinguish between different mental disorders.

It is important to note, as well, some disadvantages of the DSM system. Two issues in the diagnosis and classification of psychopathology are especially relevant to the study of selection and causation processes. First, an increasing number of investigators are questioning the adequacy of a categorical system to classify psychopathology (Krueger et al. 1998; Kirmayer, Robbins, and Paris 1994; Clarkin and Kendall 1992; Widiger and Shea 1991), and some advocate the development of an alternative system based on dimensions (Clark, Watson, and Reynolds 1995). Categorical and dimensional approaches are not mutually exclusive, however, and for this study we use both; we present one set of analyses employing DSM diagnoses as categorical measures and a parallel set of analyses using DSM symptom scales as quantitative, dimensional measures (a strategy recommended by Ollendick and King [1994] and Aneshensel et al. [1991]).

Second, epidemiological studies using the DSM classification system have revealed high rates of comorbidity: the simultaneous presence of two or more mental disorders. Approximately half of all individuals with a DSM disorder meet criteria for at least one additional comorbid disorder (Newman et al. 1998; Kessler et al. 1994; Robins and Regier 1991), an issue that has implications for clinical practice and research design (Clark et al. 1995). If comorbid cases are present in a research sample (as they will be in unselected samples such as the one studied here), it is important to control for the potentially confounding effects of multiple disorders before interpreting the unique influence of a particular target disorder on later outcomes (Sher and Trull 1996). In our multivariate analyses, we include statistical controls for the spectrum of symptoms (and diagnoses)

before evaluating findings about associations between a specific mental illness and educational attainment.

We focus on the internalizing and externalizing disorders, the major mental disorders that afflict adolescents (Achenbach and Edelbrock 1983). Internalizing disorders involve emotional distress that is turned inward and include anxiety and depression. These are the disorders that have received the most sociological attention in studies of adults, and our study of adolescents serves to complement this literature. The applicability of adult anxiety and depression symptoms to adolescents was once a matter of controversy (Rutter 1986) but is now generally accepted in the field (Reynolds 1992).

Externalizing disorders, in contrast, are characterized by "acting out" behavior, such as poorly controlled, impulsive behavior, as well as attention problems and hyperactivity. In adolescence, they are composed mainly of conduct disorder and attention deficit disorder. In adulthood, the antisocial behavior that characterizes conduct disorder may manifest itself as antisocial personality disorder (Lynam 1996; Moffitt 1993). We extend our knowledge of selection and causation processes by examining these disorders, which have not yet been formally examined in the sociological literature to date.³

In sum, the longitudinal Dunedin study enables us to avoid retrospective reports, extend the selection and causation literature by focusing on the formative transition from adolescence to adulthood, and examine disorder-specific relations with SES. This study both complements the literature on adults and, more generally, adds to the underdeveloped field of adolescent mental health (Institute of Medicine Committee 1995).

SAMPLE AND METHOD

The Dunedin Study

Subjects for this follow-up study were members of a complete birth cohort that has been studied extensively in the Dunedin Multidisciplinary Health and Development Study since birth. The sample and the history of the study have been described elsewhere (Silva 1990; Silva and Stanton 1996). Briefly, the study is a longitudinal investigation of the health, development, and behavior of children born between April 1, 1972, and March 31, 1973, in Dunedin, New Zealand, a city of approximately 120,000. Peri-

³ Our mental health measures do not include schizophrenia, which has been extensively studied in previous research on social status and mental illness. Adolescence is too early in the life course to study selection and causation effects in regard to psychoses such as schizophrenia, which tend to onset after age 20 (Robins and Regier 1991).

natal data were obtained at delivery. When the children were later traced for follow-up at age 3, 1,037 (52% males and 48% females, 91% of the eligible births) participated in the assessment, forming the base sample for the longitudinal study. Prevalence rates of psychiatric disorders such as major depression and conduct disorder in the Dunedin sample match rates from national U.S. surveys (Costello 1989; Kessler et al. 1994; Newman et al. 1996).

The Dunedin sample has been assessed with a diverse battery of psychological, medical, and sociological measures at ages 3, 5, 7, 9, 11, 13, 15, 18, and 21. The basic procedure for data collection in the Dunedin study involves bringing each sample member into the research unit within 60 days of his or her birthday for a full day of data collection in which various research topics are presented as standardized modules (e.g., mental health interview, Life History Calendar, physical examination) by different trained examiners in counterbalanced order. Auxiliary data are also collected from parents, teachers, peers, and official records.

The present study uses data collected at ages 15 and 21. At the age-15 follow-up in 1987–88, data were missing for 8 study members who had died since age 3, 9 who could not be located, 33 who did not participate, and 11 living overseas who were unable to come to the research unit. In addition, 33 subjects participated in the assessment but have missing data on a DSM-III diagnostic category at age 15 and are not included in the analysis pool. Our final sample for the analysis of selection effects consists of the 939 for whom we obtained complete longitudinal data about mental disorders and educational attainment, a response rate of 91%. Of these 939 study members, 96% were reevaluated for mental disorders at age 21 using DSM-III-R criteria for mental disorders and are included in the analysis of causation effects.

Mental Health Measures

Mental health data were collected at ages 15 and 21 using the most current version of the DSM available at the time of interview. All interviews were privately conducted by trained staff with university degrees in social work, nursing, or clinical psychology. Age-15 interviews were conducted using the Diagnostic Interview Schedule for Children (DISC-C; Costello et al. 1982), an instrument designed to assess reliably the criteria of the DSM-III (APA 1980). Age-21 interviews were conducted using the Diagnostic Interview Schedule (DIS; Robins et al. 1989), an instrument designed to assess reliably the criteria of the DSM-III-R (APA 1987). The DISC-C and DIS were used to obtain diagnoses of mental disorders in the 12 months prior to the study member's fifteenth and twenty-first birthday interview. The modifications, psychometric properties, and descrip-

tive epidemiology of the DISC-C and DIS in this sample have been described in detail by McGee et al. (1990) and Newman et al. (1996), respectively. Of the sample, 22% met the requisite DSM-III criteria for disorder at age 15, and 40% met DSM-III-R criteria for disorder at age 21, point-prevalence rates that are consistent with other epidemiological studies throughout the world (Costello 1989; Kessler et al. 1994; Robins and Regier 1991).

For the current study, we investigated the internalizing disorders of anxiety and depression and the externalizing disorders of conduct disorder, attention deficit disorder, and antisocial personality disorder. At age 15, the anxiety disorder group ($n = 100$) consisted of study members who met criteria for the DSM-III anxiety disorders of childhood: overanxious disorder, separation anxiety, simple phobia, social phobia, or any combination of these disorders. At age 21, the anxiety disorder group ($n = 185$) consisted of study members who met the criteria for the DSM-III-R anxiety disorders of adulthood: generalized anxiety disorder, obsessive-compulsive disorder, panic disorder, agoraphobia, social phobia, simple phobia, or any combination of these disorders.⁴ The depression disorder group ($n = 37$ at age 15, $n = 163$ at age 21) consisted of study members who met DSM-III criteria for a major depressive episode, dysthymia, or both. The conduct disorder group consisted of study members who met DSM-III criteria for conduct disorder or oppositional disorder at age 15 ($n = 81$), and our antisocial disorder group ($n = 50$) consisted of study members who met DSM-III-R criteria for either antisocial personality disorder (ASPD) or conduct disorder at age 21.⁵ Finally, the attention deficit disorder (ADD) group consisted of study members who met DSM-III criteria for ADD at 15 ($n = 20$).⁶

In addition to categorical measures, we created continuous scales by summing the study member's scores on interview symptom items relevant to each disorder (see Krueger et al. 1996). The reliability (internal consistency) of the scales for all disorders is higher than 0.7, except for antisocial personality disorder, which has a scale reliability of 0.55. Because symptom counts are highly skewed with a mode of zero, they were transformed by taking the log of the count plus one. This transformation changes their

⁴ DSM anxiety disorders of adulthood that do not overlap with the DSM anxiety disorders of childhood were not assessed at the age-15 follow-up, due to the low expected prevalence rates among subjects not yet adult.

⁵ The DSM-III-R specifies that a diagnosis of antisocial personality disorder (ASPD) is an adult disorder and should not be given to individuals under 18 years of age. Conduct disorder may be diagnosed for an individual 18 or over if the individual does not meet criteria for ASPD.

⁶ Attention deficit disorder was not measured at age 21 because it is not regarded as an adult disorder in the DSM-III-R (APA 1987).

interpretation when used in regression equations so that they reference relative, rather than absolute, changes in mental disorder. For example, when used as an independent variable, the beta coefficient refers to the change in Y given a 1% change in the disorder scale. When used as a dependent variable, the beta coefficient refers to the percentage change in the disorder scale given a change in the independent variable (Gujarati 1988).

Social Status Measures

We indexed familial socioeconomic status at age 15 with information obtained directly from parents, using New Zealand-specific measures of parents' occupational socioeconomic status, parents' education, and family income. Parents' occupational status was measured with the Elley and Irving (1976) scale, a 6-point scale based on the average income and education levels for 546 occupations of the New Zealand labor force. We assigned scores on the basis of the higher status of either caregiver, whether father or mother. Parents' education was measured with the same scale used by Elley and Irving (1976), which categorizes attainment into three levels on the basis of primary, secondary, and tertiary degrees. As with occupational status, we assigned the score of the higher-ranking parent. Familial income was measured as the combined gross income of both parents from all sources. The SES index was a linear composite of occupational status, educational attainment, and familial income, using weights from confirmatory factor analysis (the loadings were 0.80, 0.68, and 0.67, respectively).

Study members' educational attainment by age 21 came from their own self-reports. We broke overall educational attainment by age 21 into three separate transitions because the factors influencing educational dropout may differ across educational levels (Mare 1980; Mare 1981). We first examined whether mental disorders influenced study members' performance on the New Zealand school certificate examinations. Almost all students take these national exams by age 16 because they determine promotion in secondary school and technical schools, and passing also helps secure better employment in the labor market (Kennedy 1981). Of the sample, 87% earned a school certificate in at least one subject, and among this subsample we then examined the influence of mental disorders on the study members' ability to earn a sixth form certificate, which is comparable to a high school degree in the United States (Kennedy 1981). Of those who earned a school certificate, 76% also earned a sixth form certificate, and among this subsample we then examined the effects of mental disorders on continuing to tertiary education at a university (37% of the study members who earned a sixth form certificate continued to a university

education). The variable "Educational Attainment at 21" represents study members' highest educational transition achieved by age 21 (0 = no school certificate, 1 = school certificate, 2 = sixth form certificate, 3 = university attendance).

Four additional measures were used in our analysis to control for confounding influences. We included controls for gender and the study member's ability and motivation to continue education. Intelligence was assessed with the Wechsler Intelligence Scale for Children-Revised (Wechsler 1974) between ages 7 and 11 ($M = 108$; $SD = 15$). Academic ability was assessed with the Burt Word Reading Test (Scottish Council for Research in Education 1976) when the subjects were 15 years old ($M = 91$; $SD = 15$). School involvement, an important predictor of educational attainment (Kerckhoff 1993), was measured at age 15 with a visual analogue scale. Respondents were shown a card with five concentric circles and were asked to suppose that the "circle represents the activities that go on at your school" and asked to rank their distance from "the center of things." A value of "1" indicated little involvement in school activities and a value of "5" indicates study members who considered themselves at "the center of things" (mode = 3).

Analytic Strategy

Our analysis centers on mental disorders during the 12 months prior to the age-15 birthday, the birthday after which the New Zealand adolescents in our study were first legally entitled to leave school and consequently the age at which they were first at risk of "selection" into the lower social strata. We employed three empirical tests to examine the influences of selection and causation. First, we examined the association between mental disorders at age 15 and family SES background. Second, we examined the extent to which these mental disorders impaired social mobility by evaluating their influence on subsequent educational attainment, using models that contained traditional status attainment controls such as IQ, family SES background, and gender. Third, we examined the extent to which increases in mental disorder between ages 15 and 21 were associated with early adulthood SES, as indexed by educational attainment at age 21. No test by itself provides enough information to discriminate between the three interpretations of selection, causation, and joint effects, but, taken together, they lead to discerning patterns of expected results, as outlined below.

Table 1 provides a summary of empirical tests and expected patterns of findings. Exclusive evidence for selection would be indicated by the pattern of findings in which, over time, mental disorders impaired the status attainment of study members but were uninfluenced by socioeco-

TABLE 1
EXPECTED PATTERNS OF EVIDENCE FOR SELECTION AND CAUSATION PROCESSES

Type of Evidence	Association between Parental SES and Disorder at Age 15	Effect of Disorder at 15 on Subsequent Educational Attainment	Effects of Truncated Education on Increased Disorder between 15 and 21
Exclusive evidence for selection	No	Yes	No
Exclusive evidence for causation	Yes	No	Yes
Evidence for joint effects	Yes	Yes	Yes
Evidence for no effects during early adulthood .. .	No	No	No

nomic standing of origin and socioeconomic standing of early adulthood. Exclusive evidence for social causation would be indicated by the contrasting pattern, in which mental disorders were influenced by both SES of origin and SES of early adulthood but exerted no significant influence on status attainment. Evidence for the joint effects of selection and causation processes would be indicated by a pattern in which mental disorders were influenced by SES of origin and impaired status attainment, and additionally were influenced by SES of early adulthood. Finally, evidence for the lack of both causation and selection effects in early adulthood would be indicated by nonsignificant associations across all three tests.⁷

RESULTS

Family SES Background and Adolescent Mental Disorders

The well-documented relationship between SES and mental disorders replicated among the adolescents in our sample. We found that adolescent mental disorders were more likely to be found among youth in families with low SES than would be expected by chance alone.

We limited our methods in this first section to simple bivariate associations because the association between family SES and mental disorders may represent both selection and causation (Dohrenwend 1975), and we did not know the direction of causality *a priori*. The results are presented separately for diagnostic categories in table 2 and symptom scales in table 3. In each table, we show the association between measures of mental disorders and the overall family SES index as well as its constituent parts; i.e., parents' occupation level, education level, and income. Overall, adolescents with any DSM-III diagnosis were approximately 0.18 of a SD lower in family SES origins than their peers (table 2, col. 4; $-.07 - .11 = -.18$). Adolescents' total psychiatric symptom scores on the DISC-C schedule were also significantly correlated with their SES background, at a magnitude of 0.16 (table 3, col. 4).

⁷ We had longitudinal data about mental disorders and educational attainment for 939 study members. Of these, no study members were missing data on parental occupational status or parental education, and 12% were missing data on familial income. For study members with missing data on family income, the linear SES composite included family income measures assigned on the basis of parents' occupational status and education. In addition, 2% of the study members were missing data on IQ, and 0.5% and 0.7% were missing data on reading ability and school involvement, respectively. In our multivariate models, we flagged missing data for income, IQ, reading ability, and school involvement with dummy indicators and assigned scores to replace the missing data so that none of the 939 study members would be ejected from our analyses (Little and Rubin 1987). Missing data indicators that were significant in our models are noted in the text.

TABLE 2

DSM-III DIAGNOSES AT AGE 15 AND SOCIAL CLASS: MEAN-LEVEL COMPARISONS BETWEEN DISORDERED AND NONDISORDERED ADOLESCENTS^a

	Parents' Mean Occupational Status	Parents' Mean Education Level	Parents' Mean Income	Parents' Mean Composite SES ^b	N
Mean	4.00	.89	36,070	.03	939
SD	1.25	.63	16,082	.99	939
Range	1-6	0-2	0-80,000	-2.49-2.30	
Any DSM-III disorder	3.78**	.83	35,487	-.11*	207 ^c
No disorder	4.06	.90	36,235	.07	732
Internalizing disorders:					
DSM-III anxiety	3.74*	.81	34,338	-.16*	100
No anxiety	4.03	.90	36,276	.05	839
DSM-III depression	3.81	.84	36,383	-.07	37
No depression ..	4.01	.89	36,057	.04	902
Externalizing disorders:					
DSM-III conduct disorder ..	3.78*	.89	37,292	-.07	81
No conduct disorder	4.02	.84	35,954	.04	858
DSM-III attention deficit disorder ..	3.45*	.60*	31,275+	-.42*	20
No attention deficit disorder	4.01	.89	36,174	.04	919

^a Differences between group means are evaluated using *t*-tests for all variables except for parents' education, which is categorical and evaluated using the Mantel-Haenszel chi-square

^b The mean composite SES measure has been transformed to *z* scores

^c Due to comorbidity, the number of study members who met DSM criteria for "any disorder" is less than the sum of study members who met criteria for individual disorders

* $P < .10$

* $P < .05$

** $P < .01$

TABLE 3

NUMBER OF DSM-III MENTAL DISORDER SYMPTOMS AT AGE 15 (LOGGED) AND SOCIAL CLASS. BIVARIATE CORRELATIONS

Types of Symptom	Parents' Occupational Status	Parents' Education	Parents' Income	Parents' Composite SES
Total symptoms	-.16**	-.12**	-.10**	-.16**
Internalizing symptoms:				
Anxiety	-.12**	-.10**	-.07*	-.12**
Depression02	.05	.03	.03
Externalizing symptoms:				
Conduct disorder	-.15**	-.11**	-.11**	-.15**
Attention deficit disorder	-.13**	-.12**	-.08*	-.13**

NOTE.— $N = 931$ * $P < .05$ ** $P < .01$

Differences in the relation between SES and mental disorders emerged when mental disorders were considered individually. Among the internalizing disorders, anxiety and depression had different associations with family social status. Whether assessed categorically or continuously, anxiety was disproportionately found in families with lower SES as expected, but depression was not. This is consistent with evidence that SES is more strongly related to anxiety than depression across the general population, ages 15–54 (Kessler et al. 1994). However, the complete lack of an association between SES and depression among adolescents differs from research indicating the presence of a modest SES/depression association among adults (Kessler et al. 1994; Dohrenwend et al. 1992; but see also Weissman et al. 1991, who report a nonsignificant SES/depression association).

Among the externalizing disorders, support for an association between family SES and mental disorders was more robust in analyses of attention deficit disorder than conduct disorder. Based on symptom scales, both disorders showed a significant association with SES (table 3), but only attention deficit disorder showed the expected association when measured categorically (table 2). The DSM-III categorical measure of conduct disorder was not significantly related to overall family SES, but it was negatively associated with parents' occupational status (table 2, col. 1), providing some evidence for an association with SES.⁸

⁸ Our finding of only a weak association between SES and externalizing behavior problems at midadolescence is consistent with other literature reviewed by Tittle and Meier (1991) and Tittle, Villemey, and Smith (1978).

In general, the relation between mental disorders and social status was more robust using continuous symptom scales rather than categorical classifications of mental disorders. For example, table 3 shows that the symptom scales for disorders related to the overall SES measure were associated with all three individual components of the SES measure. In contrast, the diagnostic categories in table 2 were usually associated with only one or two SES components. We suspect that this difference stems more from methodological than substantive reasons and reflects the fact that categorical classifications lose information and statistical power by compressing information on mental disorders into a dichotomous measure (see Mirowsky and Ross 1989).

Mental Disorders and Subsequent Educational Attainment

We next examined the influence of adolescent mental disorders on educational attainment. We found effects that varied by mental illness: internalizing disorders had no effect on educational attainment, while externalizing disorders exerted a strong negative influence.

We used mental disorders at age 15 as predictor variables in logistic regression equations modeling educational transitions. We present the results separately for the two different measures of mental disorders: table 4 presents results using categorical diagnoses, and table 5 presents results using symptom scales. In each table, we analyzed the effects of adolescent mental disorders on three educational transitions: the acquisition of at least one school certificate, completion of the sixth form certificate, and continuation to university training. For each transition, we present two models. The first includes only measures of mental disorders and family SES as predictors of educational achievement. The second includes all the predictors included in the first model and adds status attainment controls to isolate the unique effects of adolescent mental disorders on educational attainment from other factors implicated in the status attainment process.

The internalizing disorders of anxiety and depression did not significantly affect educational attainment in any of the models, whether using DSM diagnoses or DSM symptom scales. These results suggest that adolescents with internalizing disorders are not "selected" into the lower social strata through truncated education.

In contrast, we found strong evidence for selection processes among adolescents with externalizing disorders. Conduct disorder impaired achievement at every educational transition in this study. Adolescents who met the DSM criteria for a diagnosis of conduct disorder were less likely to earn a school certificate by an odds ratio of 4.53, even after controlling for family socioeconomic background and the presence of other

TABLE 4
DSM DIAGNOSES AT AGE 15 AS PREDICTORS OF EDUCATIONAL ATTAINMENT: UNSTANDARDIZED COEFFICIENTS
FROM LOGISTIC REGRESSION EQUATIONS

VARIABLE	TRANSITION 1 FAILURE TO EARN ANY SCHOOL CERTIFICATE (N = 939)		TRANSITION 2 FAILURE TO EARN SIXTH FORM AMONG SCHOOL CERTIFICATE RECIPIENTS (N = 815)		TRANSITION 3 FAILURE TO ENTER UNIVERSITY AMONG SIXTH FORM RECIPIENTS (N = 617)	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Internalizing diagnoses.						
Anxiety	-.14	-.23	.26	.28	.29	.25
Depression28	.48	-.04	.16	-.62	-.49
Externalizing diagnoses:						
Conduct disorder	1.51**	1.59**	.92**	1.21**	.84	1.04*
Attention deficit disorder	1.72**	.36	1.98**	1.57
Controls:						
Family SES at 15	-.92**	-.76**	-.57**	-.40**	-.52**	-.32**
Female		-.77**	..	-.41**	...	-.08
IQ		-.04**	...	-.06**	...	-.04**
Reading ability		-.04**	...	-.03**	..	-.04**
School involvement	-.49**	..	-.20	...	-.22
Intercept ..	-2.57**	6.72**	-1.37**	8.22**	.66**	10.01**

NOTE.—The New Zealand sixth form certificate is comparable to a US high school diploma (Kennedy 1981). Ellipses indicate variable excluded in model 1, and included in model 2.

* $P < .05$

** $P < .01$.

TABLE 5

NUMBER OF DSM-III MENTAL DISORDER SYMPTOMS AT AGE 15 (LOGGED) AS PREDICTORS OF EDUCATIONAL ATTAINMENT: UNSTANDARDIZED COEFFICIENTS FROM LOGISTIC REGRESSION EQUATIONS

VARIABLE	TRANSITION 1: FAILURE TO EARN ANY SCHOOL CERTIFICATE (<i>N</i> = 931)		TRANSITION 2 FAILURE TO EARN SIXTH FORM AMONG SCHOOL CERTIFICATE RECIPIENTS (<i>N</i> = 806)		TRANSITION 3 FAILURE TO ENTER UNIVERSITY AMONG SIXTH FORM RECIPIENTS (<i>N</i> = 613)	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Internalizing diagnoses:						
Anxiety	-.14	-.24	-.11	-.25	-.05	-.16
Depression	-.19	.03	-.18	-.07	-.08	-.00
Externalizing diagnoses:						
Conduct disorder54**	.47**	.35**	.40**	.29*	.35*
Attention deficit disorder90**	.64*	.92**	.74**	.25	.11
Controls:						
Family SES at 15	-.86**	-.70**	-.52**	-.34*	-.49**	-.32**
Female	-.59*	.	-.19	.	-.03
IQ	-.04**	.	-.06**	.	-.04**
Reading ability	-.04**	.	-.03**	.	-.04**
School involvement	-.48**	.	-.17	.	-.24
Intercept	-4.47**	4.97**	-3.08**	6.63**	.13	9.24**

NOTE.—The New Zealand sixth form certificate is comparable to a U.S. high school diploma (Kennedy 1981). Ellipses indicate variable excluded in model 1 and included in model 2.

* $P < .05$

** $P < .01$.

comorbid mental disorders (table 4, model 1 of the first transition; $e^{1.51} = 4.53$).⁹ This effect was still strong after status attainment controls were entered into the equation (table 4, model 2 of the first transition). Adolescents meeting DSM criteria for a diagnosis of conduct disorder who earned a school certificate were then significantly less likely to earn a sixth form certificate by an odds ratio of 2.51, after controlling for family socioeconomic background and the presence of other mental disorders (table 4, model 1 of the second transition; $e^{.92} = 2.51$). Again, this effect was still significant after introducing status attainment controls into the equation (table 4, model 2 of the second transition). In the final educational stage of the analysis, we found evidence that adolescents with conduct disorder who overcame the odds against them and received both a school certificate and a sixth form certificate were later less likely to continue to a university education. Using a DSM diagnosis, this effect is significant only in the full model, at the .10 level (table 4, model 2 of the third transition). When substituting DSM symptom counts for diagnoses, however, the effect of conduct disorder on educational attainment is significant for all educational transitions, in both the abbreviated and full models. In sum, by the time adolescents with conduct disorder reach adulthood, they appear to be "selected" into the lower socioeconomic strata through restricted educational attainment.

Attention deficit disorder also impaired educational attainment, although its effects in our models were contingent, in part, on its measurement. Using a DSM diagnosis, we found that adolescents with attention deficit disorder (ADD) were less likely to earn a school certificate by an odds ratio of 5.58 (table 4, model 1 of the first transition; $e^{1.72} = 5.58$), although this effect lost statistical significance after introducing the status attainment controls into the equation (table 4, model 2 of the first transition). Those adolescents who met DSM criteria for ADD that did earn a school certificate were then significantly less likely to earn a sixth form certificate (table 4, model 1 of the second transition), although, again, this effect was not significant after status attainment controls were introduced into the equation (table 4, model 2 of the second transition). We found that adding only IQ and reading ability to model 1 of the first and second transition was enough to make the effects of ADD lose statistical significance (analyses not shown). The effect of ADD on educational attainment past the sixth form was difficult to measure because the sample size be-

⁹ Study members who were missing family income data were significantly less likely ($P < .01$) to earn any school certificate in model 1 of the first transition in both tables 3 and 4 ($\beta = 0.89, 0.85$, respectively). Study members missing data on family income did not significantly differ in their educational attainment in any of the other models.

came extremely small. Only three (15%) adolescents with ADD earned both a school certificate and sixth form certificate, a number too small to enter into our models predicting university attendance.

The results differed in one important way when ADD was measured with a symptom count instead of a dichotomous diagnosis. Namely, the symptom count continued to predict failure at educational transitions, even after the status attainment controls were added to the equation (table 5, model 2 in transitions 1 and 2). The only models in which ADD did not exert a significant influence were in the equations predicting university attendance (table 5, models 1 and 2 of the third transition), when the number of sixth form recipients displaying serious ADD symptoms was so small that we believe it precluded us from finding significant effects. In sum, our analysis of ADD indicates strong support for selection effects early in the educational career of adolescents.

Overall, the controls acted in their predicted directions. Lower socioeconomic background was significantly associated with impaired educational attainment in all models. Higher academic ability, as measured by IQ and reading ability, was associated with greater educational attainment at all educational stages. School involvement at age 15 significantly predicted successful completion of a school certificate, although its effects were not significant at later educational stages. Finally, we found that women were more likely to earn a school certificate than men, a finding consistent with government statistics (Department of Education in New Zealand 1987, table 39).

In models not shown, we examined whether the effects of mental disorder on educational attainment differed significantly between men and women. We included multiplicative interaction terms between gender and each disorder in the full models for the three educational transitions.¹⁰ Only one out of 22 interaction terms reached statistical significance at the .05 level, an interaction we do not interpret here because it may have occurred by chance alone. These results suggest that the effects of mental disorder on educational attainment are similar for men and women.

Finally, we also ran models to examine the extent to which comorbidity influenced our results. We found that with and without controls for comorbid disorders in the models, all disorders acted in the same direction and at the same significance levels.

¹⁰ Only one woman met criteria for attention deficit disorder in our model predicting sixth form completion, and no women met it in our model predicting university attendance. These small *ns* precluded us from including these two gender interactions in our models using DSM diagnoses as measures of mental illness. We were, however, able to include all gender interactions in our models using DSM symptom scales.

Early Adulthood SES and Mental Disorders

In the third part of the analysis, we focused on early adulthood SES, as indexed by educational attainment at age 21, and examined its association with increased disorder between ages 15 and 21. We found that respondents with lower educational attainment were more likely to experience increases in anxiety and antisocial disorders but not depression.

We used logistic and ordinary least squares (OLS) regressions to predict age-21 disorders from educational attainment, statistically controlling the influence of age-15 disorders, gender, and parental SES. For all disorders, model 1 in table 6 presents results using symptom scales and OLS regression, while model 2 in table 6 presents results using dichotomous diagnoses and logistic regression.

We found that study members with low educational attainment at age 21 reported significantly higher levels of anxiety, after statistically controlling for age-15 levels of anxiety, parental SES, and gender (table 6, anxiety models 1 and 2). Upon further inspection (not shown), we found that the relationship between educational attainment and anxiety was monotonic: increases in anxiety between ages 15 and 21 were highest among study members with the least educational credentials and declined monotonically with higher educational attainment. These findings are consistent with the prediction of the causation hypothesis that greater exposure to lower socioeconomic experience increases anxiety disorders.

Slightly higher levels of depression were also found among study members with low educational attainment at age 21. This effect was not significant using either DSM diagnoses or symptom scales (table 6, depression models 1 and 2).

In our final analysis, we focused on antisocial disorders and found that they were overrepresented among respondents with low educational attainment. Because conduct disorder at age 15 may continue as antisocial personality in adulthood (APA 1987), we used both age-21 conduct disorder and antisocial personality disorder as measures of antisocial disorder in early adulthood. At age 21, study members with low educational attainment were more likely to meet DSM criteria for either conduct disorder or antisocial personality disorder, a finding that persists after statistically controlling the influence of conduct disorder at age 15, parental SES, and gender (table 6, antisocial disorder models 1 and 2). Further, we examined age-21 conduct disorder and antisocial personality disorder in two separate models, and in both cases educational attainment significantly ($P < .05$) predicted disorder using both DSM diagnoses and symptom scales, after controlling the influence of age-15 conduct disorder, parental SES, and gender (analysis not shown). In all antisocial disorder models, the relationship between education and mental disorder was monotonic.

TABLE 6

EDUCATIONAL ATTAINMENT AS A PREDICTOR OF MENTAL DISORDER AT AGE 21: UNSTANDARDIZED COEFFICIENTS FROM REGRESSION EQUATIONS^a

VARIABLE	ANXIETY AT 21		DEPRESSION AT 21		ANTISOCIAL DISORDER AT 21	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Educational attainment at 21	-.20 ⁺	-.15*	-.08	-.72**	-.08**
Controls:						
Disorder at 15 ^b	1.35**	.57**	1.56**	1.27**	.24**
Family SES at 15	-.18	.01	-.02	.15	.01
Female28**	.86**	-.252**	-.33**
Intercept	1.05**	-1.93**	-1.31**	.92**
N	903	893	903	905	916

* Model 1 uses dichotomous DSM diagnoses as measures of mental disorder at ages 15 and 21, and coefficients are from logistic regression equations. Model 2 uses continuous DSM symptom scales (logged) as measures of mental disorders at ages 15 and 21, and coefficients are from OLS equations.

^b In models predicting age-21 anxiety, depression, and antisocial disorders, control disorders are age-15 anxiety, depression, and conduct disorder, respectively.

 $+P < .10$

* $P < 0.05$

$$^{**}P < 0.01$$

These findings are consistent with the hypothesis that greater exposure to low socioeconomic experience fosters antisocial disorders.

Focusing on the control variables, we found that age-15 levels of disorder strongly predicted age-21 levels of disorder in all models, indicating substantial continuity of mental health problems from adolescence to adulthood (Ollendick and King 1994; McMahon 1994). Low parental SES had no significant effect on mental disorder at age 21, net of the effect of the study members' own educational attainment and past history of mental disorder at age 15. Finally, consistent with the literature, we found that women reported significantly higher levels of anxiety and depression than men and significantly lower levels of antisocial disorder. The only significant gender interaction we found (not shown) was in the model using DSM symptom scales of antisocial disorder, and it suggests that the effects of low educational attainment on antisocial disorders may be greater for men than women.

DISCUSSION

The aim of this study was to examine whether low SES serves as a cause or consequence of mental illness, and we focused our inquiry on a cohort of adolescents as they made the transition to adulthood. Consistent with previous research in this area, we used educational attainment as a proxy for SES in early adulthood, a time period when many young adults have not yet realized their potential in terms of other status measures such as income and occupational prestige. Our analysis was especially well suited to study mental disorders and educational attainment for four reasons. First, we used a prospective longitudinal study that allowed us to test between the three interpretations of selection, causation, and joint effects. Second, we evaluated subjects for a wide array of DSM-III disorders, allowing us to examine selection and causation processes across different psychiatric disorders. Third, we studied the sample during the transition from adolescence to young adulthood, a developmental period that includes both the peak onset of psychiatric disorders and the normative educational transitions that constitute a key first step in the status attainment process. Finally, the high response rate of the longitudinal Dunedin study diminished any bias introduced by selective missing data.

We found that the relation between mental disorders and SES is unique for every disorder examined in this study. The main difference centers on selection effects—the extent to which adolescents with mental disorders “select” themselves into the lower social strata through curtailed education. We found no evidence for selection effects among youth with the

internalizing disorders of anxiety and depression. In contrast, we found strong evidence for selection effects among youth with the externalizing disorders of conduct disorder and attention deficit disorder. Below, we discuss these and more detailed differences in selection and causation processes across the different disorders and point out implications for methodology, treatment, and theory.

Before turning to a discussion of the findings, it is important to note three limitations of this study. First, the sample size is too small to support analyses of specific disorders such as social phobias or dysthymia. Accordingly, we limited our analyses to families of specific disorders, that is, anxiety, depression, antisocial disorders, and attention deficit disorder. Our sample size is also too small to provide a reliable analysis of psychoses such as schizophrenia, which generally do not onset until later in the life course (Robins and Regier 1991). Due to these sample size constraints, we have limited our inquiry about selection and causation processes to the most prevalent adolescent mental health disorders.

Second, subjects who dropped out of school may return in the future and finish uncompleted degrees. This potential bias is nominal in our analyses of school certificate and sixth form certificate completion because very few people attempt to acquire these certificates in later life by returning to school (Department of Education in New Zealand 1987) or through correspondence courses (Department of Labor in New Zealand 1990). However, students who return to education later in life for university training are more common (Department of Labor in New Zealand 1990). Our analysis of university attendance thus represents conservative estimates, and the effects of adolescent mental disorders may be greater than our results suggest if they also deter people from returning to university training in adulthood.

Third, this study requires future replications to support its generalizability beyond New Zealand. To date, the selection and causation literature has proceeded on the unstated assumption that cross-national differences in selection and causation processes throughout the Western world are minimal. Fox (1990), for example, presents results from both British and American samples in his analysis of selection processes without noting the possibility of cross-national biases. Dohrenwend et al. (1992) do not explicitly consider country-specific effects in their Israeli-based research and neither do Kessler et al. (1995) or Wheaton (1978) in their American-based research. The assumption that selection and causation effects are similar across countries has yet to be tested explicitly; such tests are, of course, contingent on the design of mental health studies with comparable measures and methods. With these qualifications in mind, we discuss our findings by disorder.

Anxiety

In regard to anxiety, our pattern of results conforms to the predictions of exclusive social causation (summarized in the second row of table 1). The influence of causation processes is supported by our findings that SES of origin predicted anxiety at age 15 and that SES of destination by age 21, as measured by educational attainment, predicted increases in anxiety from ages 15 to 21. At the same time, we did not find evidence to indicate the presence of selection processes. The selection interpretation predicts that disorders will impair educational attainment, but anxiety did not hinder young adults' progress through any of the educational transitions analyzed in this study.

These results in support of exclusive social causation are particularly important to the social stress literature. Much of the theory and methodology in this tradition is based on the assumption that social causation alone accounts for the disproportionately high prevalence rate of anxiety in the lower social strata. If our analysis had indicated that anxiety exerts selection effects, it would have provided evidence for the interpretation of joint causation and selection effects, and, if true, much of the theoretical and empirical research in the stress literature would be misspecified. Our analysis provides evidence that the assumption of social causation is appropriate in early adulthood, although it is important for future research to investigate the influence of selection effects through other potential processes later in the life course.

Our research is not consistent with that of Kessler et al. (1995), who conclude that anxiety impairs status attainment through a negative effect on educational attainment. At least three differences between this study and the work of Kessler et al. (1995) may account for the discrepant outcomes. First, cross-national differences may play a role, as discussed above. Second, differences in the measurement of anxiety may also have an effect. The results of Kessler et al. (1995) may reflect the effects of specific anxiety disorders, such as post-traumatic stress disorder, that were not assessed in our study. Third, and perhaps most importantly, we used prospective disorder measures, while those of Kessler et al. (1995) are retrospective. Analyses based on retrospective disorder measures may incorrectly portray causation effects as selection effects because of "state-dependency"—the tendency for individuals to construct their past psychological states to be consistent with their present ones. Current distress may evoke reminders of past disorder, and people who are not currently distressed may forget or deny past disorder (Aneshensel et al. 1987). As a consequence, the fact that adults' retrospective reports of anxiety are inversely related to educational attainment may represent not only selec-

tion effects, as Kessler et al. (1995) argue, but also causation processes in adulthood projected into the past.

Depression

Our analysis of depression provides support for neither causation nor selection processes, suggesting that SES and depression have little influence on each other before age 21. Depression at age 15 was not overrepresented among lower SES families, it did not influence subsequent educational attainment, and increases in depression between ages 15 and 21 were not significantly overrepresented among study members with low educational attainment. As with anxiety, our outcomes differ from Kessler et al. (1995) in that we did not find evidence for selection effects, a discrepancy that may stem from cross-national differences, psychological measurement differences (our inventory of depressive disorders did not include bipolar disorder, which contains psychotic symptoms such as delusions and hallucinations), or differences between retrospective versus prospective assessments of mental disorders.

Our findings, showing no association between SES and depression in adolescence, suggest that the SES/depression association found in some studies of adults (Kessler et al. 1995; Dohrenwend et al. 1992, but see also Weissman et al. 1991) may be specific to adulthood, reflecting the consequences of adult-specific processes (Macintyre and West 1991; Rosenberg and Pearlin 1978). For example, the young adults in this sample are not yet old enough to experience the full impact of two of the main factors that place lower-SES adults at increased risk for depression—divorce or separation and becoming trapped in lower status jobs (Link, Lennon, and Dohrenwend 1993; Turner, Wheaton, and Lloyd 1995; Weissman et al. 1996). We highlight these processes as only a starting point for the development of theory that explains age-specific relations between SES and depression; a more detailed treatment may benefit from life course analysis that examines age-specific mechanisms linking social conditions and mental disorders (e.g., Elder, George, and Shanahan 1996).

Antisocial Disorders

The pattern of results for antisocial disorders conforms to the prediction of joint selection and causation effects (summarized in the third row of table 1). Consistent with the selection hypothesis, we find that adolescents with conduct disorder at age 15 are at a higher risk to fail at every educational transition analyzed in this study and thereby select themselves into the lower social strata. At the same time, the results are also consistent

with the interpretation of strong causation effects, a finding consistent with Dohrenwend et al. (1992). We found that SES of origin predicted conduct disorder at age 15 and that low SES in early adulthood, as measured by educational attainment, predicted increases in antisocial disorders between ages 15 and 21, as measured by both age-21 conduct disorder and antisocial personality disorder.

With regard to the sociological literature, these findings point to three issues that are of particular theoretical importance for an expansion of sociological research to the antisocial disorders. First, the evidence for joint selection and causation effects indicates the need to address the reciprocal relation between SES and antisocial disorders over the life course. This will require a shift away from current conceptualizations of individual mental health as a passive consequence of social structure and toward different research topics, such as the influence of antisocial disorders on interactions between individuals and their environment (e.g., Caspi et al. 1987). Second, evidence from the psychological literature strongly suggests that early onset of antisocial disorder in childhood identifies a more severe condition than onset during adolescence or later life stages (Moffitt 1993), suggesting that the results of this study represent an average of these two different groups. Further examination is required to determine the extent to which these groups differ both in terms of their social etiology and their life consequences. Third, the strong adverse effect of antisocial disorders on educational attainment suggests that they warrant admission to models that predict status attainment, alongside more traditional status-attainment variables such as family SES, IQ, and academic achievement.

With respect to the psychological literature, the findings provide important evidence that the effect of conduct disorder on educational attainment cannot be explained solely by its association with impaired intelligence. The relative influence of these two correlated factors has been debated heatedly (Mandel 1997). By providing evidence showing a unique, negative influence of conduct disorder on educational attainment, our findings highlight the importance of research into possible intervening mechanisms and their relative influence. Candidate mechanisms associated with conduct disorder include teenage pregnancy (Robins and Price 1991; Zoccolillo and Rogers 1991), addiction to drugs and alcohol (Elliott, Huizinga, and Menard 1989), and peer deviance (Cairns and Cairns 1994), all of which are associated with school dropout (Crane 1991; Kaplan and Liu 1994; Cairns and Cairns 1994) and which may mediate the far-reaching effect of conduct disorder on status attainment. In addition, the effects of conduct disorder on truncated education may also be mediated by the effect that this disorder has on other persons, such as teachers or authority figures; for example, the disruptive behavior of conduct-disor-

dered youth make them more likely to be “thrown out” of school (Cairns and Cairns 1994).

Attention Deficit Disorder

Our analysis also points to selection processes among youth with attention deficit disorder. We found evidence that these youth are much less likely to earn a school certificate or a sixth form certificate, which is comparable to a high school degree in the United States. As with antisocial disorder, evidence for selection effects strongly suggests that the relation between ADD and SES should not be studied in the same manner as the relation between internalizing disorders and SES.

The extent to which IQ and reading ability mediate the effects of ADD on educational underachievement is a matter of debate (Hinshaw 1992) and is sensitive, we show, to the disorder’s measurement. When ADD is measured using a DSM diagnosis, its effects on educational attainment are entirely mediated by IQ and reading ability, but, when measured with a DSM symptom scale, its effects are only partially mediated. There is a need for a more definitive conception and measurement of ADD before empirical research can shed light on its influence on educational achievement relative to intellectual ability.

Conclusion

Different mental health problems are differently related to social status. Each adolescent disorder in this study (anxiety, depression, conduct disorder, and attention deficit disorder) bore a different relation to educational attainment, indicating that differences between disorders extend beyond the simple psychotic/nonpsychotic dichotomy currently recognized in sociological research on social selection and social causation. In terms of methodology, our findings suggest that research on social status and mental health has much to gain by incorporating measures of specific mental illness, rather than relying on the omnibus concept and measure of “psychological distress,” which combines symptoms of different disorders. Of course, available interview schedules are expensive to administer—and the methods of diagnoses recommended by different classification systems are imperfect—but the alternative to well-conceived, mental health measurement may be misspecified theories of the relations between social status and mental illness.

In terms of treatment, the finding that different psychiatric diagnoses are differently related to social status suggests that the need for interventions and therapeutic strategies varies by disorder. Our results indicate

that anxiety and depression do not significantly impair educational attainment and consequently do not require special intervention programs to counteract their effect on educational attainment. By contrast, conduct disorder and attention deficit disorder uniquely impair educational attainment and thereby damage the future life chances of the persons they afflict. It is particularly important for treatment regimens targeted at these latter adolescent mental health problems to include interventions that improve school performance and curb the initial slide into downward drift.

In terms of theory, our findings highlight the need for disorder-specific explanations of the relations between social status and mental disorders. Our findings about anxiety and antisocial disorders underscore the importance of social-causation processes in the life course; the elevated prevalence rate of these disorders in the lower social strata is partly a product of social inequalities and class-related social conditions. These results thus confirm a role for social structure in the etiology of anxiety and antisocial disorder and highlight the need for research to identify the mechanisms that account for these SES differentials. We also find evidence that conduct disorder and ADD exert strong selection effects, indicating that they disrupt the status attainment process during the transition from adolescence to adulthood. To the extent that these disorders are passed to offspring, either through socialization or biological factors, they play a heretofore overlooked and powerful role in the reproduction of class structure and social inequality. These selection processes violate the assumption of social causation and point to the need for research to identify the mechanisms that lead to their disruptive intra- and intergenerational patterns of influence.

Sociological research offers a unique perspective on the social causes and consequences of mental disorders. While anxiety and depression have captured much of the sociological imagination, our results suggest that other psychiatric conditions—historically neglected in sociology—merit careful scrutiny in order to enable a fuller understanding of how social conditions influence individual lives.

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Market Partitioning and the Geometry of the Resource Space¹

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This article gives a new explanation for generalist and specialist organizations' coexistence in crowded markets. It addresses organizational ecology's resource-partitioning theory, which explains market histories with scale economies and crowding, and it shows that some main predictions of this theory can be restated in terms of structural properties of the N -dimensional Euclidean space. As resource-space dimensionality increases, the changing niche configurations open opportunities for specialists. The proposed approach draws upon the sphere-packing problem in geometry. The model also explains new observations, and its findings apply to a range of crowding and network models in sociology.

INTRODUCTION

Multidimensional spaces are well understood tools of social scientists to represent objects with several attributes. They have several names in different crowding models of sociology and economics. The N -dimensional space is called *sociodemographic space* in affiliation models of individuals' group formation (McPherson 1983; Popielarz and McPherson 1995). *Market topology maps* visualize intermarket transactions, where distance measures similarities in input-output dependencies (Burt and Carlton 1989; Burt 1992). The Euclidean framework is called *competence space* if organizational competencies are in focus (Nooteboom 1994; Péli and Noote-

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boom 1997). It is labeled as *product characteristics space* when spatial axes stand for descriptors of commodities (Lancaster 1966), and its name is *resource space* in organizational ecology, when customers with different tastes constitute the key resource for organizations (Hannan and Freeman 1989; Carroll 1985).

If customer tastes are operationalized in terms of product characteristic preferences, then the last two representations coincide. We adopt this interpretation: customers' purchasing power is the key resource for the organizations in question, and customers intend to buy the products that best match their preferences. Our goal is to give a geometry-based explanation for the long-term coexistence of generalist and specialist organizations in different markets. We demonstrate that most of the main claims of an empirically justified sociological theory, the resource-partitioning model of organizational ecology (Carroll 1985, 1997; Carroll and Hannan 1995), can be explained and extended if some structural features of multidimensional niche configurations are taken into account.

The Euclidean space stands for the market, it is the framework in which interactions take place. We model our entities as spatial objects with certain geometric properties that stand for sociological attributes. Spatial configurations represent relations between organizations, and geometry specifies constraints on their possible market positioning. Organizations are characterized by the customer tastes they address, that is, by the resource space locations they exploit, called niches or catchment areas. Competition is modeled in terms of niche overlap. Under certain conditions, markets are partitioned between a number of organizations, just as the Earth's surface is partitioned between countries. Our work intends to contribute to the understanding of these processes.

The simplest and most widely studied way of market partitioning is to assign the same size and shape of catchment area to each organization. A classic one-dimensional example is Hotelling's (1929) linear city model of product differentiation, also readdressed as *circular city* by Salop (1979). Nooteboom (1993) investigated the possibilities of the multidimensional generalization of the problem: how can the Euclidean N -space be partitioned between congruent and regular polytopes, the N -dimensional generalizations of polyhedra? The goal was to specify multidimensional "honeycombs" with spherelike cells that completely fill up the space, or in socioeconomic terms, organizations with equal catchment areas in a product characteristics space. However, our survey of the mathematical literature revealed that the underlying tessellation problem has no regular and spherelike solution beyond two dimensions (Coxeter 1948). Under spherelikeness, we mean that the catchment areas have similar extensions in each direction. For example, soccer balls are not really spheres,

but spherelike (not regular) polyhedra, their surface being composed of pentagons and hexagons.

We address the space-partitioning problem differently. We assume that catchment areas are N -dimensional spheres (hyperspheres). A hypersphere is a simple geometric object, its volume and surface depend only on its radius.² However, spaces cannot be completely covered by spheres without overlap: if $N > 1$, there is always some residual left between the objects. Putting it differently, some demand is always left unsatisfied by organizations with spherical, nonoverlapping niches. The ubiquitous presence of leftout space around spheres may seem a mathematical inconvenience. However, it has an explanatory function if one allows for the existence of specialist organizations. Specialists are characterized by their narrow niches (Brittain and Freeman 1980; Freeman and Hannan 1983; Péli 1997). They can populate residual regions or holes between organizations of broader niches (generalists).

We concentrate on these holes. We show that a field in geometry, the *sphere-packing problem*, provides new insights for the sociology of organizations. Applying this field to resource-partitioning theory, one can explain a lot of the dynamics of generalist-specialist markets. While the original theory explains resource-partitioning processes on the basis of scale economies and market center occupation, now the results will be obtained from the properties of N -dimensional arrangements.

The original and proposed resource-partitioning explanations are complementary: each explains aspects for which the other, alone, could not give an account. Resource inhomogeneity is assumed in the scale economy-based model: demand has uni- or polymodal distribution in space. On the contrary, the geometric approach assumes that demand is homogeneously distributed.³ The two models can be seen as two layers of explanation for the same phenomena. The geometric explanation (flat demand distribution) serves as a background. The second layer adds complexity to the first in the form of peaks in the demand distribution.

This article is organized as follows. The following section summarizes the original resource-partitioning theory. What are the main predictions and explanatory elements? The next section comes up with the geometric model: similar outcomes but a different explanatory structure. The last section assesses the methodological benefits of the proposed explanation,

² We use the words *hypersphere* and *sphere* synonymously, though literally the second denotes 3-D objects.

³ Actually, Carroll (1985) used first circular niches and homogeneous resource distributions in two dimensions in his first resource-partitioning work.

and it gives empirical illustrations for the outcomes. We also propose some further sphere-packing applications in other models of sociology.

RESOURCE PARTITIONING WITH SCALE ECONOMIES

Model and Ramifications

Carroll (1985) analyzed newspaper publishing in several local American markets, explaining the temporal dynamics of markets composed of generalist and specialist organizations. The market is an N -dimensional Euclidean space with axes that denote taste descriptors; thus, each point in space stands for a certain customer taste. Generalist organizations make appeals to a broad range of customer tastes, while specialists address specific ones. Accordingly, a generalist's niche is a broad region in the resource space, while specialists occupy small spots. The taste distribution is uneven over the population, and the market has a center (or a few centers) composed of mainstream tastes. Resources are abundant in the center, so the organizations there can grow to be large.

This setting gives rise to the following population history. Early in the market, the surviving firms are mainly generalists. To increase sales, generalists tend to differentiate themselves by differentiating their product offers, positioning their niches apart from each other. Product differentiation is a way to reduce competition (Eaton and Lipsey 1989), since niche overlap yields intense price competition. The occupant organizations of the resourceful central regions grow bigger than the others, and the induced increase in size yields scale economy advantages: the big firms get even bigger, forcing medium-size generalists out from the market (Rosse 1980). The number of generalist organizations falls, while their average size grows. Market concentration increases.

A crucial element in the model is that the life chances of the emerging small specialists are attached to the concentration level of generalists: high concentration opens little resource pockets for specialists. This happens as follows. As the relatively smaller generalists disappear, resources become unutilized. The surviving big generalists take the best chunks of the residual space, positioning themselves into the market centers. As the fight between generalists dies out, product differentiation loses its importance. The winner organizations now adjust their offers to the mainstream needs at the center. The surviving generalists increase their niche width, taking over the best parts of the extinct competitors' market segments. But as they move toward the market centers, they leave some customers unsatisfied at the edges. Small specialist organizations establish footholds in these market pockets. Taste distributions also often get flatter as markets develop, thus further increasing resource abundance at the edges. In the end,

there is no competition between the survivor generalists and the new-comer specialists.⁴

Empirical Evidence and Questions

A rapidly growing research program in a broad variety of industries gives empirical support to the outlined theoretical picture. Predicted effects of resource partitioning were detected in the brewing industry (Carroll and Swaminathan 1992, 1993; Swaminathan and Carroll 1995), in banking cooperatives (Freeman and Lomi 1994; Lomi 1995), in wine production (Swaminathan 1995), in medical diagnostic imaging (Mitchell 1995), and in microprocessor production (Wade 1996). Earlier, Barnett and Carroll (1987) observed a symbiotic relation between telephone companies occupying different niches in the same location: these organizations exerted a positive influence on each other's fate. Recently, Dobrev (1997) analyzed the restructuring process of the Bulgarian newspaper industry during the era of political transition, applying the resource-partitioning framework to an environment substantially different from American markets. The research of Torres (1995) and Seidel (1997) found, respectively, resource-partitioning processes in the British automobile and American airline industries.

Studies on size-localized competition also display similar effects to those claimed by resource-partitioning theory (Hannan and Freeman 1977; Hannan and Ranger-Moore 1990; Hannan, Ranger-Moore, and Banaszak-Holl 1990). Organizations of very different sizes usually also differ in structure, and competition tends to be stronger among structurally similar organizations. Since generalists and specialists are usually quite different in size and in structure, the losers of size-localized competition are mostly the medium-size generalists.⁵

Some market effects are hard to explain only with scale economies. For example, scale economy effects magnify even minor size differences between generalists, finally leaving a single organization in place. In reality, more than one big generalist can be sustained in several types of markets. If we assume polymodal taste distributions, then a handful of generalists may survive in the resulting landscape, each occupying one market center.

⁴ Boone and Witteloostuijn (1995), in analyzing the connection between organizational ecology and industrial organization, find similarities between the resource-partitioning model and Sutton's (1991) dual structure theory of industry concentration.

⁵ Taking into account other aspects, medium-size generalists may have their chances. Investigating the Californian savings and loan industry, Haveman (1993) found that medium-size organizations are the most willing to diversify their activities into new markets.

The low-demand ditches around the centers keep the incumbent organizations away from appropriating the neighbor's domain. Though such outcomes do occur, still each local center can be occupied by only one player. More general solutions would require further aspects that affect survival, for example, institutional settings (Meyer and Scott 1983; DiMaggio and Powell 1983) or status and network positions (Podolny, Stuart, and Hannan 1996).

Two other effects to explain are specialists' presence in early markets and also in nonmarginal market segments. How can specialists make a foothold before the winning big generalists position to market centers and leave behind resources at the margins? If there is a market center, then how can some specialists persist in this region tightly controlled by big generalists? We will explain these phenomena as well as other resource-partitioning processes on the basis of niche positioning in densely packed markets.

THE GEOMETRIC RESOURCE-PARTITIONING MODEL

We recapitulate the market history phases to explain why mainly generalists compete in the market, why their number decreases with time, how the surviving generalists broaden their niche, what resource pockets open up for specialists, and how generalists and specialists coexist in the market.

Competing generalists seek to cover as much resource space as possible under some restrictions. The first restriction is that generalist niches are symmetric in all directions. The second restriction is that the market is sufficiently resourceful to recover entry costs of setting up production and distribution. The third restriction is that generalist niches do not overlap as a free-entry equilibrium is reached. A fourth assumption adds dynamics to the model: customer tastes get elaborated with time. We address the conditions under which these restrictions are met and show how they lead to resource partitioning. Our model applies mathematical results from the sphere-packing field of geometry that we summarize next. Then, we address the conditions of niche sphericity and explain resource-partitioning processes in terms of the proposed model.

The Sphere-Packing Problem

This field in geometry is concerned with ways of filling up N -dimensional Euclidean space with hyperspheres of equal size (Conway and Sloane 1988).⁶ The main issue is to find dense packings, configurations where the

⁶ Currently, see also on the Internet at <http://www.astro.virginia.edu/~eww6n/math/Hypersphere.html>

proportion of space occupied by spheres is high. The efficiency of a sphere packing is measured by *packing density* (Δ), the ratio of the volume occupied by the spheres to total space volume ($0 \leq \Delta \leq 1$). Unfortunately, the solution of the sphere-packing problem is not known beyond three dimensions. In one dimension, the hyperspheres are sections of equal length along a line. If the neighboring sections meet, then packing density is unity (fig. 1a). In two dimensions, hyperspheres are circles. The densest packing has $\Delta = 0.9069$ (fig. 1b). In three dimensions, the cannonball packing is the densest (fig. 1c). Fortunately, upper bounds on packing density can be calculated for each N , and the known densest packings approximate these bounds quite well (table 1). Maximal packing density converges fast to zero with N . This finding will play a central role in our argument.

Niche Symmetry

Stinchcombe (1991) calls *mechanisms* those pieces of scientific reasoning that connect lower- and higher-level entities in theories. He argues that objects at the lower level can be conceptualized as very simple if this characterization sufficiently explains the higher-level outcomes (as molecules are modeled as little balls in classical gas theory). Resource-partitioning theory has two levels: organizational events appear as cumulative outcomes at the population level. We represent our lower-level objects (organizations) with spheres, thus obtaining a simple and powerful explanatory mechanism. However, the assumption that the organizations under investigation have spherical niches needs justification.

Organizational ecology makes a distinction between *fundamental* and *realized* niche (Hannan and Freeman 1989). The fundamental niche represents those resource configurations under which organizations persist in lack of competition. The realized niche is the subset of the fundamental niche in which organizations are perceived in case of competition.

The symmetry of the fundamental niche is a consequence of isotropy, the invariance of spatial directions. Isotropy follows from the homogeneity of the resource distribution. If spatial directions do not count, then other things being equal, organizations develop the same niche breadth in any direction. In reality, taste descriptors do differ in importance. This fact can be incorporated into the model by assigning a set of weights to the dimensions and performing affine transformations along each axis with these weights. Instead of having hyperspheres, then we arrive to the N -dimensional analogues of ellipses (in 3-D: rugby balls). Affine transformations do not affect volume ratios or the topology of the spatial arrangements, therefore the forthcoming geometric arguments also apply when

Figure 1a

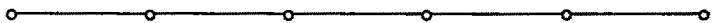


Figure 1b

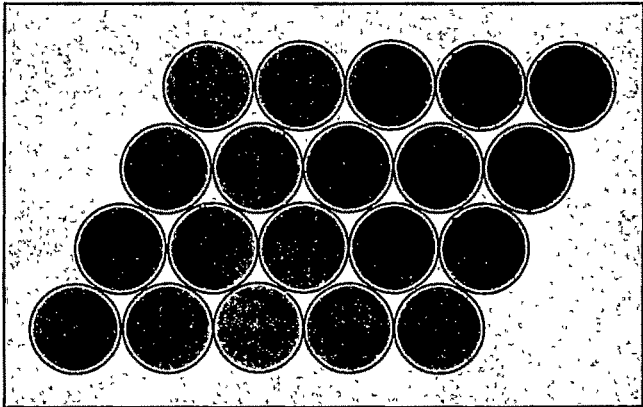


Figure 1c

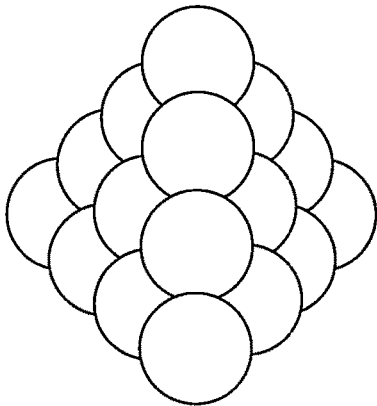


FIG. 1.—Dense sphere packings in 1–3 dimensions

TABLE 1

THE KNOWN DENSEST PACKINGS WITH KISSING
NUMBERS AND THE KNOWN THINNEST COVERINGS

Dimensions (<i>N</i>)	Packing Density (Δ)	Kissing Number (τ)	Thinnest Covering (Θ)
1	1	2	1
290690	6	1.2092
374048	12	1.4635
461685	24	1.7655
546526	40	2.1243
637295	72	2.5511
729530	126	3.0596
825367	240	3.6658
914577	272	4.3889
1009962	372	5.2517
1106624	519.78	6.2813
1204945	756	7.5101
1303201	1,060.67	8.9768
1402162	1,422	10.727
1501686	2,340	12.817
1601471	4,320	15.311
17008811	5,346	18.288
18005958	7,398	21.841
19004121	10,668	26.082
20003226	17,400	31.143

SOURCE.—Conway and Sloane (1988, pp. 15, 38).

NOTE.— τ is not always an integer. If hyperspheres have different number of neighbors in a packing, then τ is calculated as an average.

taste aspects differ in importance. For the sake of convenience, we assume that all taste variables are standardized, and then we can proceed with spheres instead of ellipses.

The shape of the realized niche is affected by the neighboring organizations that compete for the same resources. Realized niches also take a symmetric shape in case of a free-entry equilibrium. First, we assume that price discrimination is prohibited: one cannot offer the same product for different prices for different customers at the same time. So, the lowest price anywhere in the niche applies throughout the niche. Second, we assume that to the price for the product consumers add a cost related to the distance between their position in resource space (which represents the characteristics of their "ideal product") and the position of the product as represented by the center of the niche. This cost reflects the compromise

consumers make in taking a product that does not exactly match their preferences. Given the impossibility of price discrimination, the price throughout the niche is now determined by the maximum product price plus distance cost that is still acceptable to the most distant customer. The bigger the distance to the most distant consumer, the lower the product price has to be to compensate for the higher distance cost. The trade-off in niche extension between a larger niche volume and a lower price (to “pull in” more distant customers) yields an optimal niche size, having the given number of spatial dimensions. Further niche extension reduces profit. Asymmetric niche extension is even more unprofitable: the price reduction needed to extend “reach” is insufficiently compensated by additional sales only in selected directions. For this reason, generalists tend to maintain symmetric niches.

Dimensional Expansion: New Opportunities for Specialists

Now, we apply the sphere-packing field to organizational markets to explain resource partitioning. We assume that total demand is constant in time. When demand expands rapidly, then organizations easily find free resource, making the partitioning problem irrelevant. If demand decreases in time, then all forthcoming arguments hold a fortiori. As approximating their optimal niche breadth, generalists arrange their niches in a way to minimize competition. So generalist niches realize a sphere packings. Later, we will specify forces that penalize deviations from such an arrangement. Moreover, we consider tight market packings, when generalists’ niche configuration is close to the optimal. If huge market segments were left unexploited by generalists due to loose configuration, then specialists’ presence would be obvious.

We consider the increasing number of spatial dimensions as an explanatory variable. Since axes stand for taste descriptors, an increase in N reflects customer taste elaboration.⁷ As customer demand gradually becomes more diversified, the resource space extends into new dimensions. Organizational bids also fold out into the extended space. For example, circular niches take spherical shape in moving from two to three dimensions. Maximal packing density persistently falls with N (fig. 2). This means that the percentage of total resource accessible for generalists becomes less and less. This is in line with the observation that the number of generalists decreases in the market with time.

⁷ This article does not address other forms of demand elaboration like scale extension (offering extra size products) or scale refinement (intermediate sizes, qualities).

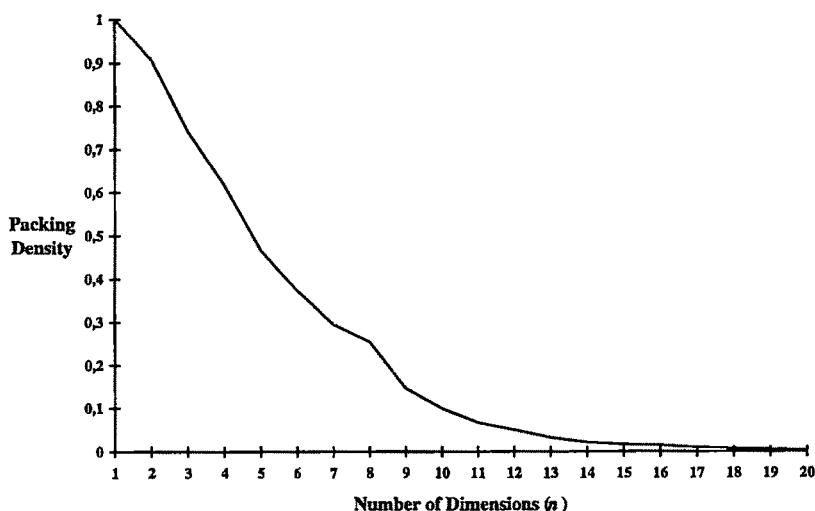


FIG. 2.—The density of the known densest packings

The most surprising conclusion in resource-partitioning theory, specialists' emergence in tightly packed markets, can be also explained with dimensional change. As N increases, pockets open between the generalist niches in which specialists can take footholds. The portion of residual space between spheres increases steeply at each dimension change (fig. 2). In one dimension, spheres are linear sections that can fill up the space without a residue: there are usually no specialists in markets where products are differentiated only in one dimension (or in none, like in some classical shortage economies, Kornai 1980). Moving to two dimensions, generalists' resource utilization decreases to about 90% (table 1). Since tastes develop in time, this prediction is in line with specialists occasional presence in early markets. When moving from two to three dimensions, generalists' maximal resource share falls to 75%. The next few dimension shifts yield roughly a 20% loss per step. The decline even gets steeper from $N = 8$. In 10 dimensions, generalist niches would cover less than 10% of the resource space.

Note that the generalist organizations leave unabsorbed resources in every market segment. So, specialists can make footholds at any taste regions, not only at the margins. However, because we assumed flat resource distribution, our approach does not explain why specialists' occurrence is more frequent at nonmainstream tastes. Here, the original and the geometric resource-partitioning explanations complement each other. Imagine the homogeneous resource distribution as an elastic membrane in two di-

mensions. Add inhomogeneities to the flat surface; then humps will stand for market centers. According to Carroll's (1985) model, competition is stronger at the center, and generalists are better competitors than specialists due to size effects. So, opportunities do open for specialists in every market segment (geometric explanation). However, not all these opportunities are necessarily taken by specialists, because their survival chances are much better at the margins than in the center (original explanation).

Broader and Nonoverlapping Generalist Niches

Resource-partitioning theory claims that the survivor generalists' increase their niche breadth. Niche extension is unprofitable beyond a limit having a given number of spatial dimensions. However, generalists have to increase their niche width if the number of spatial dimensions increases. In higher N s, customer demand is distributed along a higher number of taste "cells" (just like a hectare, a square with 100 meter edges contains 100^2 square meters). Resource density gradually thins out having constant total demand and more spatial dimensions. A spherical niche in the $N + 1$ dimensional market typically occupies a much lower share of total resource than a niche of the same radius in N dimensions. Losing volume percentage means losing sales. To preserve market share, generalist organizations have to increase their niche width after each dimensional shift. In the original resource-partitioning model, generalists occupy new territories as a reward of winning the competition. In the new model, generalists have to increase their niches not to lose sales.

Resource-partitioning theory also claims that competition between generalists lessens with time. Thick niche overlap could exclude specialists, since any space can be completely covered by overlapping hyperspheres. But niche overlap causes head-on price competition, which further reduces price. In pure price ("Bertrand") competition, profits are eroded to zero, which, as a result of the impossibility of price discrimination, applies throughout the niche. In "Cournot" competition, on the basis of sales volume, profits are still positive but less than when niche overlap is evaded. Here profit declines with an increasing number of competitors in the overlap.⁸

Overlapping generalists face a steeply increasing number of competitors as space dimensionality increases, and the overlap also becomes thicker with N . If generalists cover the whole resource space by overlapping niches, then another mathematical notion, the *thickness of the covering* (Θ) applies. While the sphere-packing problem is about densely filling up

⁸ See more on Bertrand and Cournot competition in Shapiro (1989).

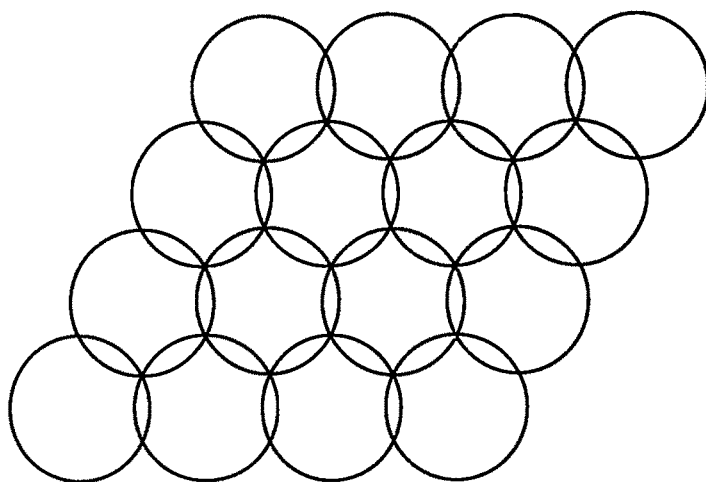


FIG. 3.—The thinnest space covering in two dimensions

the space with touching spheres, the space-covering problem searches the thinnest covering with overlapping hyperspheres of equal size. Covering thickness (Θ) measures overlap, telling the average number of spheres that contain a given point in space. If each point is occupied by exactly one sphere, then $\Theta = 1$. But beyond one dimension, $\Theta > 1$.

The arrangement with least overlap in two dimensions has $\Theta = 1.2092$ (fig. 3). Just like densest packings, the thinnest coverings are only known up to two dimensions. But lower bounds on Θ are given for each dimension, and the known best values well approximate these bounds if N is not very high. Covering thickness steeply increases with N (table 1; fig. 4). Beyond four dimensions, $\Theta > 2$; that is, more than two producers compete for a customer on the average. Since there is no overlap near the sphere centers, this means much more than two offers for a customer in the overlap. Complete market covering with generalists ignites an increasingly strong competition as the resource-space dimensionality increases.

Generalists also have many more potential competitors as the number of taste dimensions increases with N . The *kissing number* (τ) denotes the number of neighboring spheres that touch a certain sphere in a packing. The kissing number that now measures the number of competitors increases extremely fast with N . For example, for the known best packings, the kissing number is 24 at $N = 4$, but it is 240 at $N = 8$ (table 1; fig. 5). Massively growing covering thickness and high kissing numbers with increasing resource-space dimensionality: these effects make the occupa-

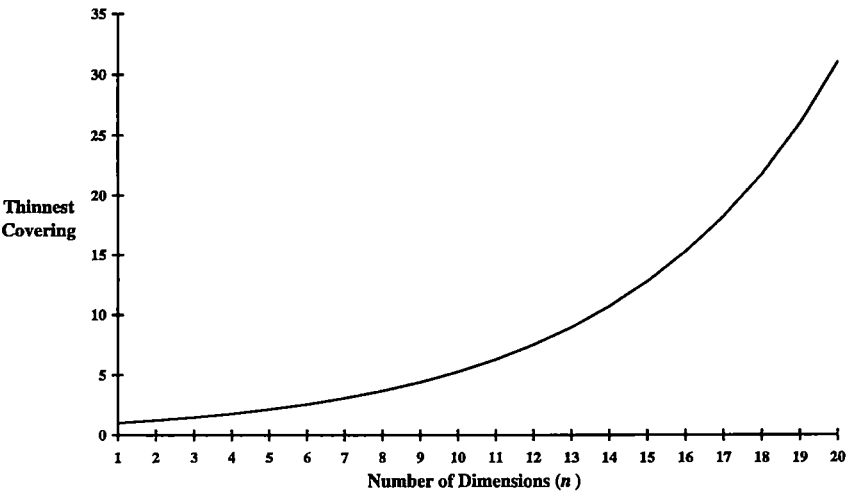


FIG. 4.—The known thinnest coverings

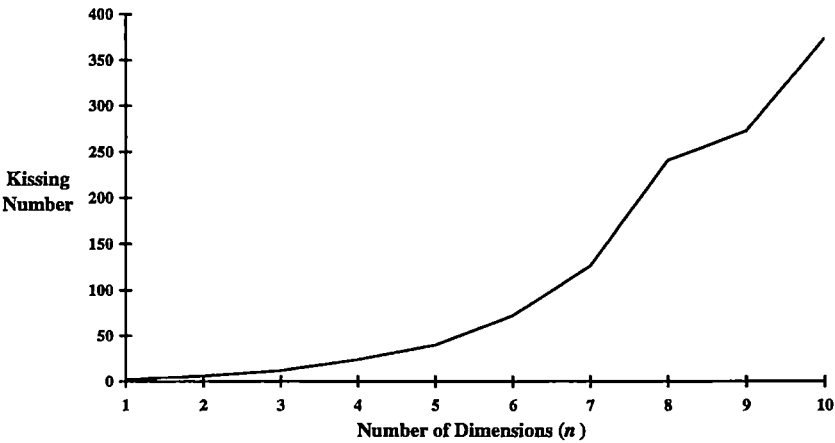


FIG. 5.—Kissing numbers in the known densest packings

tion of residual space increasingly disadvantageous for generalists. This allows for some specialists to utilize the resource pockets and to survive.

DISCUSSION

We address three topics in this section. First, we give empirical illustrations to specialist organizations' emergence when new spatial dimensions

open. Second, we compare the proposed model to other space models in sociology and sketch some applications of the sphere-packing approach beyond the ecology of organizations. Finally, we summarize the core conclusions and mention some future research directions.

Empirical Illustrations

One aspect for the empirical testing of the model is the measurement of niche shape: is it symmetric when market equilibrium is reached? This task would require detailed sales distribution data along the relevant taste dimensions for each firm. The empirical studies on the standard resource-partitioning model do not address niche sphericalness. However, our model can be tested by checking if its main predictions conform with the empirical findings in this field. This section illustrates some core predictions with examples from industries in which resource-partitioning processes have been pointed out. Since the predictions of the original and the geometric explanations mostly coincide, the forthcoming examples support both models. But there is one important aspect in which the predictions of the two models differ: the geometric approach predicts resource pockets also at nonmarginal tastes. The forthcoming examples support this picture: specialists can be observed even at mainstream positions but much less frequently than at marginal tastes.

In the beer market, production is subject to economy of scale, as is generally the case in process technology industries, so in principle generalist producers have a cost advantage. There is also a scale effect in access to distribution channels and in building up brand names through advertising. In the beer industry, brand image is of great importance and is not easily built up. Evidence for this is given by the fact that Heineken, for many years, produced beer at home and shipped it to the United States, thus incurring the enormous cost of transporting mostly water across the ocean, to maintain its credibility as a Dutch brand. However, an interest in special tastes has emerged, yielding opportunities for specialist producers (Carroll and Swaminathan 1992, 1998; Carroll, Preisendörfer, Swaminathan, and Wiedenmayer 1993). Generalists cannot outcompete specialized tastes by price discrimination, and they have trouble selling specialty beers of their own. Moreover, it is obvious in the U.S. market that this is a temporal coincidence between the rise of specialty brewers on the one hand and an increase in the number of dimensions that customers use in evaluating and purchasing beer on the other hand. Specialty beers reside in an expanded product space that did not exist earlier; new dimensions of flavor, color, and ingredients have become operative in consumer decision making. According to the geometric model discussed here, this expansion makes it easier for specialists to enter and survive.

In retailing, there are economies of scale in unified purchasing and logistics, shop design, and fittings across many shops, which yields an advantage for chain store corporations. This requires a certain standardization in the range of products and qualities offered. Small, specialized independents have a potential advantage in tailoring their product range more finely to local demand. Large shops cannot counter this with price discrimination. And it would cost them too much to obtain similar fine-grained knowledge of all the peculiarities of local demand, and, to cater to it, they would have to compromise too much on the standardization of product range from which they obtain their cost advantage. This explains the existence of small independents next to chain stores. Note, however, that developments in information and communication technology (ICT) offer opportunities for “mass customizing,” whereby large generalists can more easily differentiate their products. This may yield a countervailing effect. Some specialist products of yesterday can be generalist products today, the content of specialization is subject to change in time. Specialists have to come up with new product aspects, thus contributing to the introduction of new dimensions to the market.

Newspaper publishing offers economies of scale in production, news gathering, printing, and distribution. But local publishers can tailor the news to local conditions. Generalist publishers would compromise the economies of large-scale production by tailoring the news to local readers. They would encounter search problems in gathering the news and may encounter problems of acceptance and credibility as a nonlocal producer. Here again, recent developments in information and communication technology bring new elements into the picture. On the one hand, ICT offers flexible combinations of local, national, and global news that are locally offered by generalist producers. On the other hand, generalists might still encounter the problems of local search and credibility. Economies of scale in printing and distribution can be offset by making specialized, idiosyncratic information available on the Internet. The availability of such specialized information might stimulate even greater demand for local news. A recent development is the boost of electronic publishing on the Internet: a new, resourceful spatial dimension is emerging, giving rise to a broad variety of “local papers” in the cyberspace (newsgroups, electronic bulletins, Web sites of specific interest groups) that coexist with the professionally designed and frequently visited sites of reputable “generalist” publishers.

Sphere-Packing and Crowding Models in Social Science

Bioecologists like Levins (1968), Hutchinson (1978), and Roughgarden (1979) consider niches as the set of environmental conditions, represented

as a part of the resource space, under which populations of a species are sustained. Hannan and Freeman (1977, 1989) adopted this view for organizational populations. Since the objects under investigation (species, organizations) often can be observed in certain parameter ranges (sections) along each resource axis, a convenient way to conceptualize niches is to consider them as N -dimensional rectangles (Hutchinson 1978; McPherson 1983). We have already emphasized why niche sphericalness is crucial in our model. Now, we argue that an approach that assumes rectangular niches only works if N is low.

The niche dwellers that stay far from the niche center in several dimensions face multiple disadvantages. For example, if someone is not pleased with the color of a pair of trousers, then that person will be even more displeased if the size does not fit either. Geometrically: if staying close to a niche edge is bad, then staying close to a niche vertex is even worse in rectangular niches. The misfit gets bigger as N increases. Consider hypercubes of unity edge as special rectangles. In N dimensions, the Euclidean center-vertex distance is $0.5\sqrt{N}$, while the smallest distance from the center to the cube's surface is 0.5. That is, the ratio of the biggest to smallest center-edge distances is \sqrt{N} , that goes to infinity with N . This ratio is 1.41 in two dimensions, and it exceeds two beyond $N = 4$. Spherical niches are exempt from this problem. Note, however, that niches can be rectangular if the problem under investigation requires some specific non-Euclidean metrics (see more in Freeman 1983).

The application of the sphere-packing problem goes far beyond bioecology and organizational ecology. It can have a bearing on political sociology: how should political parties position their catchment areas in the space of potential voters, minimizing both residual space and overlap? The sphere-packing problem has connections to multidimensional data evaluation. For example, the task of finding appropriate cluster centers in cluster analysis is related to the *quantizer problem* (Conway and Sloane 1988), which also goes back to the search for optimal sphere packings. Here, we sketch the application of sphere packings in two well-known network theories: the McPherson affiliation model (McPherson 1983; Popielarz and McPherson 1995) and the structural hole theory of Burt (1992).

The network research of McPherson and his colleagues addresses group formation dynamics. A central explanatory element in their argument is the homophily principle (Blau 1977): people with similar sociodemographic positions are more likely to form voluntary groups. Moreover, group membership duration is positively affected by the similarity of members. To represent individuals' affiliation drives, "social circles" are drawn around potential group members. If a great part of a member's social circle falls into the niche of the focal organization, then membership is stable (Popielarz and McPherson 1995). However, this line of research

applies rectangular niches for the modeled organizations. Applying spherical niches for the studied organizations instead of rectangles, one arrives at a new sphere-packing application, which we briefly indicate.

The affiliation argument says that group members in marginal niche positions typically have a higher number of destabilizing external ties, and therefore they are more likely to leave the organization. To reduce the ratio of uncertain members at the edges, it is in the interest of the organization to minimize its niche boundary surface. Hyperspheres have the minimal surface to volume ratio among N -dimensional bodies: hence, another argument for spherical niche shape. Niche overlap is also bad for voluntary organizations because it causes head-on competition for members. We showed that the minimal thickness of sphere coverings strongly increases with N . In higher dimensions, overlap is more disadvantageous, so voluntary groups will be wary of overstretching their niche. But, overlap elimination entails empty pockets in the sociodemographic space. Hence come the following two predictions: (1) High dimensional sociodemographic spaces facilitate compact group formation. (2) High dimensional sociodemographic spaces open opportunities for small human groups with very similar members.

Assuming that the texture of the sociodemographic space becomes more elaborated with time, the second prediction is in line with the perceived upsurge of a broad variety of alternative groups in the last decades. Research on human group formation operates with operationalizable dimensions. Therefore, the affiliation model offers an opportunity to test the empirical relevance of the sphere-packing approach.⁹

Another promising application is Burt's (1992) structural hole theory. Structural holes separate nonredundant ties in a network. Because maintaining ties has costs, agents in the network seek to optimize their connections by minimizing redundancy (thus maximizing the number of structural holes). This brings us to a potential application of sphere packings. In a theoretical paper, Linton Freeman (1983) investigates network embeddings into multidimensional spaces: then nodes are characterized by N coordinates. A maximal distance, δ , is introduced beyond which connection between nodes is not possible. Consider a focal agent (ego) in the network with direct access to a number of others who are all mutually insulated from each other. In Burt's terminology, ego has nonredundant ties under the criterion of cohesion (1992, p. 18). Is there an upper bound for ego's nonredundant ties in the N -space? Putting it differently, what is the densest *star* graph in N dimensions? Choosing $\delta/2$ as sphere radius, the problem reduces to the search for the maximal kissing number in the

⁹ We are indebted to a reviewer who drew our attention to this possibility.

given number of dimensions (Freeman 1983): how many touching hyperspheres can be placed around a focal sphere? The kissing number (τ) minus one delivers an upper bound for ego's nonredundant ties. Because each pair of ties is insulated by a hole, the maximal number of structural holes around ego is $(\tau - 1)(\tau - 2)/2$ in N -dimensional networks. Note that a similar argument would apply to a variety of network studies (e.g., Podolny and Stuart 1995; Stokman and Zeggelink 1996) were they rephrased in terms of network ties embedded to the N -space of sociological descriptors.

Summary and Directions for Future Research

The common underlying theme in this paper is the sociological application of the sphere-packing problem. The key concepts of this geometry domain (packing density, covering thickness, kissing number) can be coupled with some basic features of the addressed sociological models. Maximal sphere-packing density drastically decreases, while the covering thickness and the kissing number steeply increases with N . So, if space dimensionality increases in the modeled sociological theory, then the new geometric conditions necessitate reconfiguration.

We chose organizational ecology's resource-partitioning theory as a focal application and reconstructed the empirically documented phases of the resource-partitioning process between generalist and specialist organizations. How do resource pockets open for specialists? Why does the number of generalists decrease, and why do generalist niches broaden with the number of spatial dimensions? What makes niche overlap increasingly costly with N , putting a brake on generalist competition? Beyond resource-partitioning theory, the sphere-packing domain also applies to a variety of crowding models in social science. We found applications to explain voluntary group formation dynamics and to estimate the maximum number of nonredundant ties in N -dimensional star networks.

We mention three directions for future research. First, one can allow for size differences between generalist niches even if much of the mathematical rigor of the original setting would be lost. The result might resemble soap foam: there are large bubbles of different sizes, and the fluid between them is filled up with little bubbles. Second, not only the densest sphere packings can be interesting for sociology. For example, the relatively simple *square-lattice* packing (partition the space with hypercubes and place a maximal sphere into each) can reflect a copying mechanism in organizational positioning ("copy your neighbors, but differentiate your offer at least in one dimension"). A third research direction can address the reverse of the resource-partitioning story, when N decreases in time.

Then, the geometric model predicts that collapsing space dimensions will sweep away specialist organizations.

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Nominal, Ordinal, and Narrative Appraisal in Macrocausal Analysis¹

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Macrocausal analysis is often characterized as following only a single strategy of causal inference. In fact, however, at least three different techniques are used: nominal, ordinal, and narrative strategies of causal assessment. Focusing on recent works of comparative history, this article presents an evaluation of the strengths and limitations of each strategy. In addition, it considers the trade-offs involved in combining two or more strategies. Finally, the article discusses the role of scholarly tastes and skills, the research question, and ongoing research cycles in shaping the methodological approach selected by investigators.

Since the publication of pioneering works of comparative history by scholars such as Barrington Moore (1966), Reinhard Bendix (1964, 1978), Charles Tilly (1967, 1975), and Theda Skocpol (1979), comparative-historical analysis has expanded to become a well-established research tradition within sociology. Yet, recent works from this tradition have attracted far less attention than the breakthrough studies of the 1960s and 1970s. Contemporary methodological critiques of comparative-historical analysis continue to focus on studies from the earlier generation (e.g., Lieberman 1991; Kiser and Hechter 1991), and comparative-historical researchers continue to look almost exclusively to the older studies for methodological guidance. This neglect of much of the best contemporary scholarship has contributed to a loss of forward momentum within the comparative-historical research tradition (see Katznelson 1997, p. 84).

The present article offers a methodological evaluation of recent comparative-historical work. It does so by developing a typology of different

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strategies of causal appraisal based on a methodological reassessment of an older work: Theda Skocpol's *States and Social Revolutions* (1979). Despite the tendency of many scholars (including Skocpol herself) to characterize *States and Social Revolutions* as following only one basic strategy of causal appraisal, the book's explanatory argument is in fact derived from a complex mix of three different strategies. Skocpol explicitly identifies one of these strategies, what might be called causal appraisal based on *nominal comparison*, and this technique has received the most commentary and debate. But two other types of causal appraisal are also used by Skocpol: *ordinal comparison* and *narrative analysis*. Each of these three strategies represents a *different* technique that can be used for assessing the *same* causal relationship. Likewise, each of the strategies embodies its own characteristic strengths and weaknesses.

The relative merits and shortcomings of nominal, ordinal, and narrative strategies have yet to be explored in the literature on comparative-historical methods. Furthermore, methodologists have yet to analyze the *interplay* among these strategies and the diverse ways in which they have been *combined* in empirical research. As a consequence, we currently have a very poor understanding of the alternative methods of causal inference used by contemporary comparative-historical researchers. This article seeks to improve our understanding by systematically assessing both the trade-offs involved in using nominal, ordinal, or narrative appraisal and the trade-offs involved in combining two or all three of these strategies. As we shall see, each individual strategy has distinctive strengths and weaknesses but so too do different combinations of strategies. Recent works of comparative history offer excellent examples for illustrating the relative merits of both individual strategies and different combinations of them.

This article focuses specifically on different strategies of causal inference, and all of the works examined below fall within the broad logic of comparative history that Skocpol and Somers (1980) call "macro-causal analysis." In this logic, the analyst selects a small number of cases (often nation states) for investigation and moves back and forth between theory and history in an effort to identify the causes of a clearly identified outcome.² The article does not focus on alternative logics of comparative history discussed by Skocpol and Somers. For example, it does not examine works that adopt general theoretical frameworks that are then applied to cases (as in Skocpol and Somer's "parallel demonstration of theory"

² Thus, macrocausal analysis is best understood as having both inductive and deductive features (see Skocpol 1979, p. 39; Skocpol and Somers 1980, p. 182; Stryker 1996, pp. 310–13; and Goldstone 1997, pp. 112–13).

approach), such as recent rational-choice and game-theoretic works within the comparative-historical tradition (e.g., Cohen 1994; Levi 1988, 1997).³ The article likewise does not focus on works that derive causal inferences primarily through statistical analyses of large numbers of cases, such as many historically oriented network analyses (see Emirbayer and Goodwin [1994] for a review). Rather, this article is about the methodological techniques used by analysts working within the macrocausal research tradition. The diversity within this research tradition—not yet explicated in the literature—warrants analysis in its own right.

Finally, this article concentrates on methodological procedures that are actually used by practicing macrocausal analysts. The article does not examine hypothetical strategies of causal investigation; for example, it does not focus on strategies of inference that require interval or ratio data precisely because this type of data is not typically used in macrocausal analysis.⁴ Nor does it consider many ontological and epistemological issues addressed in the philosophy of social science. Philosophical excavation of basic assumptions can lead one to raise important issues, but such excavation can also lead one to lose sight of key methodological distinctions that directly influence the actual practice of sociological research. This article thus largely brackets questions of underlying ontology and epistemology in an effort to better understand the specific methodological procedures used by macrocausal analysts.

NOMINAL, ORDINAL, AND NARRATIVE APPRAISAL IN SKOCPOL'S WORK ON REVOLUTIONS

No work of comparative history has received more methodological scrutiny than Skocpol's *States and Social Revolutions* (1979). Little consensus has emerged, however, on the methods of causal assessment employed by Skocpol. Indeed, at least three different understandings are present in the literature. First, Skocpol herself asserts that she uses J. S. Mill's method of agreement and method of difference to identify the causes of social revolution in France, Russia, and China. As a consequence, much debate over the book has centered on the usefulness of Mill's methods for mac-

³ Rational-choice and game-theoretic approaches are of course not the only general theories used by historical sociologists. For example, Mann's (1986, 1993) application of a "sources of power" framework to world history might be viewed as employing a general theory to historical investigation.

⁴ These more fine-grained levels of measurement may be highly desirable to comparative-historical researchers who seek to employ a general theory or who desire to use statistical techniques of causal evaluation. However, the variables used by macrocausal analysts generally refer to large processes and structures that cannot be easily conceptualized as interval or ratio categories in a small-*N* research context.

rosocial inquiry (Nichols 1986; Skocpol 1986; Burawoy 1989; Liebersson 1991, 1994; Savolainen 1994). Second, Jack Goldstone (1997) argues that the method of agreement and the method of difference “are not used by comparative case-study analyses” (p. 108). He suggests it is “extremely unfortunate that . . . Theda Skocpol has identified her methods as Millian, or inspired by Millian logic. In fact, in many obvious ways, her methods depart sharply from Mill’s canon” (p. 109). Goldstone maintains that Skocpol’s methods follow an alternative logic in which explanatory variables take on varying levels across cases and combine together in differing ways to produce the same outcome.

Finally, William Sewell (1996*b*) argues that Skocpol attempts to use Millian methods, but this effort is a failure. He writes that “it is remarkable, in view of the logical and empirical failure of [Skocpol’s use of Mill’s methods], that her analysis of social revolutions remains so powerful and convincing” (p. 260). According to Sewell, what makes Skocpol’s argument successful is her effective use of historical narrative. “The ‘proof’ [of her argument] is less in the formal logic than in the successful narrative ordering of circumstantial detail. The true payoff of Skocpol’s comparative history, then, is not rigorous testing of abstract generalizations but the discovery of analogies on which new and convincing narratives of eventful sequences can be constructed” (p. 262).⁵

In this section, I argue that the lack of consensus over Skocpol’s methodology grows out of her use of at least three different strategies of causal appraisal. Different commentators have focused on only one particular strategy and thus have reached divergent conclusions: Skocpol and most of her critics focus on nominal comparison, Goldstone’s observations point to Skocpol’s use of ordinal comparison, and Sewell’s commentary highlights the importance of narrative appraisal in Skocpol’s argument.⁶

Nominal Comparison

Causal assessment in comparative-historical analysis often takes place by making nominal comparisons across cases. Nominal comparison involves the use of categories that are mutually exclusive and collectively exhaustive. Mill’s (1974) methods of agreement and difference, the most similar and most different systems designs (Przeworski and Teune 1970, pp. 31–39; Lijphart 1971, 1975), and Boolean algebra (Ragin 1987) are the most

⁵ Burawoy (1989, p. 771; his emphasis) makes an almost identical argument: “Were it not for [Skocpol’s] rich and compelling treatments of revolutionary process her book would never have received its well-deserved acclaim. This virtue exists *despite*, not because of, her declared method.”

⁶ Goldstone (1997, pp. 112–13) also discusses Skocpol’s use of narrative analysis.

important examples of methods based on nominal comparison. Each of these methods is fundamentally designed to locate the causes of an outcome by eliminating potential necessary or sufficient explanatory factors (see Mahoney 1998). As several methodologists have suggested, techniques of nominal appraisal have a deterministic understanding of causation in the specific sense that they make it difficult or impossible to model processes of partial and probabilistic causation (Ragin and Zaret 1983; Skocpol 1984; Nichols 1986; Ragin 1987; Lieberman 1991, 1994; Goldstone 1997; Tilly 1997).⁷

Leaving aside the stale debate about whether Skocpol actually follows Mill's methods in the precise manner that John Stuart Mill intended, it is clear that she was inspired by these methods and that her argument relies on their basic logic. In particular, Skocpol uses the method of agreement to identify a common set of causes that were present in her three cases of revolution. At the same time, she uses the method of difference to show how several nonrevolutionary cases (England, prerevolutionary Russia, Germany, Prussia, and Japan) lacked one or more of these causes and thus did not experience a revolution. Two main causes of social revolution are identified by Skocpol: "I have argued that (1) state organizations susceptible to administrative and military collapse when subjected to intensified pressures from more developed countries from abroad and (2) agrarian sociopolitical structures that facilitated widespread peasant revolts against landlords were, taken together, the sufficient distinctive causes of social-revolutionary situations commencing in France, 1789, Russia, 1917, and China, 1911" (1979, p. 154).

Table 1 summarizes these two causes as "conditions for state breakdown" and "conditions for peasant revolt." This table might be seen as evidence that both of Skocpol's main causal factors can be eliminated, given that they are each individually present in positive and negative cases of revolution and hence can be eliminated using the method of difference. However, Skocpol treats the *combination* of the two causes as a *single* factor for the purpose of using Mill's methods. Thus, both causal factors are present only in the three positive cases of revolution; at least one of the two conditions is missing from each case of nonrevolution. In this sense, *when taken together*, conditions for state breakdown and conditions

⁷ Of course, as other scholars have noted, macrocausal analysts may use additional methods that are not deterministic (this point is developed below). To my knowledge, only Savolainen (1994) has attempted to argue that existing nominal methods are not deterministic (she focuses on Mill's methods of agreement and difference). However, in making her argument, Savolainen ends up discussing a logic of inference that does not actually follow Millian methods (see the critiques of Savolainen in Lieberman [1994] and Goldstone [1997]).

TABLE 1

NOMINAL COMPARISON: SKOCPOL'S USE OF THE METHODS OF AGREEMENT AND DIFFERENCE

	MAIN CAUSAL FACTORS		EXAMPLES OF CAUSAL FACTORS ELIMINATED			OUTCOME
	Conditions for State Breakdown	Conditions for Peasant Revolt	Relative Deprivation	Urban Worker Revolts	Social Revolution	
France	Yes	Yes	Yes	Yes	Yes	
Russia 1917 .. .	Yes	Yes	Yes	Yes	Yes	
China .. .	Yes	Yes	Yes	No	Yes	
England	Yes	No	Yes	..	No	
Russia 1905 .. .	No	Yes	Yes	Yes	No	
Germany	No	No	Yes	...	No	
Prussia .. .	No	No	Yes	..	No	
Japan	No	No	Yes	...	No	

NOTE—The scoring of variables is derived from both Skocpol (1979) and Skocpol (1986). Some variables were difficult to score, but in no instance would a change in scoring lead to a violation of the logic of the method of agreement or the method of difference. The more controversial scorings are the following: (1) State breakdown in England: the scoring is based on Skocpol (1979, p. 141) and Skocpol (1986, p. 189). (2) Peasant revolt in Russia 1905: at one point in her argument, Skocpol (1979, p. 117) implies that state breakdown was a necessary cause of peasant revolt. This would suggest that it is logically inconsistent for Russia 1905 to have a yes for peasant revolt and a no for state breakdown (since the latter is necessary for the former). However, the text of her book makes it clear that the "political crisis" (not state breakdown) of 1905 was enough to propel peasants revolts in 1905 Russia (see Skocpol 1979, pp. 133–35). (3) State breakdown and peasant revolt in Germany: the scoring is from Skocpol (1979, pp. 145–47, 156) and Skocpol (1986, p. 189). (4) State breakdown in Japan: the scoring is based on Skocpol (1979, pp. 100–4, 157). In a subsequent analysis, Skocpol (1986, p. 189) suggests that a state breakdown did occur in Japan.

for peasant revolt cannot be eliminated using either the method of agreement or the method of difference.

By contrast, two other potential causal factors considered by Skocpol (1979, pp. 34, 113), relative deprivation and urban worker revolts, can be eliminated (see table 1). Relative deprivation can be eliminated because this factor is present in both positive and negative cases of revolution (in this sense, it is not a sufficient cause of revolution). Urban worker revolts can be eliminated because this factor is not present in all three cases of revolution (in this sense, it is not a necessary cause of revolution). Hence, Skocpol uses the eliminative logic of the method of difference to dispose of relative deprivation explanations, while employing the logic of the method of agreement to reject explanations centered on urban worker revolts.

From one perspective, causal appraisal based on nominal comparison is a powerful means of eliminating rival explanations: a single deviation from an overall or expected pattern of covariation provides enough basis to reject a potential causal factor. Tools for eliminating potential causal factors are especially important for macrocausal studies because these works select only a small number of cases for investigation and thus face the equivalent of a "degrees of freedom problem" (Lijphart 1971; Campbell 1975; Collier 1991). Nominal comparison therefore has the considerable virtue of helping to contribute to a parsimonious explanation, even with a small number of cases. The limitation of the strategy, however, grows directly out of this strength: it assumes a deterministic logic of causation in which one deviation is enough to eliminate a potential causal factor. For example, Skocpol argues that urban worker revolts are not causally significant simply because they are not present in one of her three cases of revolution (i.e., China). Scholars who understand causation in probabilistic terms may well find the causal determinism of nominal strategies to be untenable.

Ordinal Comparison

By concentrating on nominal comparison in *States and Social Revolutions* (1979), it is easy to overlook the ways in which Skocpol also relies on causal appraisal based on ordinal comparison. Ordinal comparison entails the rank ordering of cases into three or more categories based on *the degree to which* a given phenomenon is present. This type of comparison is the basis for J. S. Mill's (1974) method of concomitant variation, which has been largely overlooked in methodological discussions of comparative history (but see DeFelice [1986] and Collier and Collier [1991, p. 20]). Scholars who use this method assess causality by exploring the covariation between ordinal scores on an explanatory variable and an outcome variable. Unlike

the methods of agreement and difference, the method of concomitant variation does not require a pattern of *perfect* covariation to infer causality (see Mill 1974, pp. 402–6). The presence of one case with scores on an explanatory variable and an outcome variable that deviate from a general pattern of covariation does not necessarily mean that the explanatory factor must be outright rejected.⁸ In this sense, it is more difficult to eliminate potential causes with ordinal comparison than with nominal comparison. As a consequence, even though ordinal comparison permits the assessment of partial causation, it may be less helpful for developing parsimonious explanations.

Although she is not explicit about it, Skocpol uses the method of concomitant variation throughout her book on revolutions. Only by recognizing this can we make sense of Goldstone's (1997) observation that Skocpol's "cases of social revolution . . . *differ among themselves on several independent variables*" (p. 109; his emphasis). If Skocpol were exclusively employing a deterministic technique such as the method of agreement, she would have to eliminate independent variables that do not take on the same value across all positive cases of revolution. However, Skocpol employs the method of concomitant variation by disaggregating her two main causal variables—i.e., conditions for state breakdown and conditions for peasant revolt—into constituent ordinal variables that are present to differing degrees across cases.

Table 2 illustrates how the variable "conditions for state breakdown" is disaggregated by Skocpol into three ordinal subvariables: (1) international pressure, which promotes crises for regime actors, (2) nonautonomous state, which prevents government leaders from implementing modernizing reforms, and (3) agrarian backwardness, which hinders national responses to political crises. Likewise, Skocpol disaggregates "conditions for peasant revolt" into two ordinal variables: (1) peasant autonomy and solidarity, which facilitate spontaneous collective action by peasants, and (2) landlord vulnerability, which allows for class transformation in the countryside.⁹

As table 2 suggests, for each of these constituent variables, Skocpol in effect presents a rank ordering across the eight cases. Because the three positive cases of social revolution do not all share the same score for any of the five variables, and because at least one negative case has the same

⁸ The question of how strongly variables must covary before causality can be inferred has not been adequately addressed by analysts. For an initial discussion, see Mahoney (1998).

⁹ The variable of "landlord vulnerability" also encompasses the structure of landholding patterns (e.g., the percentage of small-holder property), which is a major but often overlooked part of Skocpol's (1979) argument.

TABLE 2
ORDINAL COMPARISON. RANKING OF CASES EMPLOYED BY SKOCPOL

	CONDITIONS FOR STATE BREAKDOWN*			CONDITIONS FOR PEASANT REVOLT†		
	International Pressure	Nonautonomous State	Agrarian Backwardness	Peasant Autonomy and Solidarity	Landlord Vulnerability	
France	2	3	3	2	3	3
Russia 1917	4	2	2	3	3	3
China	3	3	4	1	2	2
England	2	4	1	1	1	1
Russia 1905	2	2	2	3	3	3
Germany	1	2	1	1	1	1
Prussia	3	2	1	1	1	1
Japan	3	1	2	1	1	1

NOTE.—The rankings for this table are based in part on Skocpol's (1979) summary charts on pp. 155–57, however, I also relied heavily on the text in chaps. 2 and 3. Evidence for some of the variables for which it was more difficult to establish the exact ordering is taken from the following pages: (1) international pressure in England, p. 141 (which implies England faced pressures as great as France and greater than Germany); (2) nonautonomous state in Russia in 1917 and 1905: pp. 88–89 (which implies that the Russian dominant class had more leverage than the Japanese dominant class but certainly not as much as the French or Chinese); (3) agrarian backwardness in Russia 1917 and 1905: p. 128; (4) agrarian backwardness in Japan, pp. 103–4; (5) peasant autonomy and solidarity in France and Russia, p. 128, (6) landlord vulnerability in China vs other cases, pp. 151–54. Finally, the scoring for peasant autonomy and solidarity in Germany refers to areas east of the Elbe

* 1 = least, 4 = most

† 1 = least, 3 = most.

score as a positive case of revolution for each of the variables, it is clear that a deterministic eliminative logic is not at work here.¹⁰ Instead, when viewed in relation to table 1, the data from table 2 suggest that Skocpol may have used an additive procedure for assessing causality. That is, the dichotomous scoring of variables in table 1 may have been derived by adding together scores of the constituent variables in table 2. For example, by adding together scores for the three processes that make up conditions for state breakdown, we find that the four cases that score "yes" on state breakdown in table 1 (i.e., France, Russia 1917, China, and England) have a sum total of at least seven, whereas each of the four cases that score "no" on state breakdown in table 1 have a sum of less than seven.¹¹ Ordinal differences on these constituent variables are used by Skocpol to explain contrasts in the process of state breakdown among cases that share the same score on dichotomous variables. For example, even though Russia and France have the same score on all dichotomous variables, the ordinal comparison makes it clear that state breakdown in Russia was much more strongly conditioned by international pressures than in France, and this difference is highlighted in Skocpol's case analyses.

Likewise, with conditions for peasant revolt, the four cases that score "yes" in table 1 (i.e., France, Russia 1917, China, and Russia 1905) have a sum of at least three when peasant autonomy/solidarity and landlord vulnerability are added together. All cases with a "no" for peasant revolt in table 1 have the minimal sum of two. Hence, a sum of at least three appears to make peasant revolt extremely likely. Yet, there is substantial variation among the cases that experienced peasant revolt, ranging from China (a sum of three) to Russia (a sum of six). These variations come into play in Skocpol's analysis, reflecting important differences in the way in which peasant revolts occurred. Thus, China's peasant revolt initially involved only agrarian disorder, not autonomous onslaughts against landlords, and it was necessary for the Communist Party to mobilize Chinese peasants before a full-scale rebellion could take place. By contrast, in Russia (in both 1905 and 1917), conditions allowed for massive and spontaneous peasant revolts against private-landed property throughout the country. Hence, even among cases that experienced peasant revolts, differences

¹⁰ Several scholars (e.g., Nichols 1986; Burawoy 1989, and Goldstone 1997) have examined most or all of the variables in table 2 as dichotomous categories to show how Skocpol's (1979) argument does not conform to the methods of agreement and difference. These scholars fail to realize that Skocpol uses ordinal (not dichotomous) comparison in assessing these causal variables. Part of the blame for this misunderstanding rests with Skocpol, who was not explicit in her methodological statement about how she disaggregates dichotomous variables into ordinal constituent variables.

¹¹ To meaningfully add scores together, one must assume that (1) the data reflect interval differences and (2) each constituent causal variable carries the same weight.

in the degree to which peasant autonomy/solidarity and landlord vulnerability were present led to differences in the process and timing of peasant revolt.

When compared to a nominal strategy of causal assessment, ordinal appraisal has the advantage of avoiding deterministic eliminative assumptions and allowing analysts to recognize that the degree to which a given variable is present may make an important difference in the explanation of an outcome. In this sense, ordinal comparison is more consistent with the assumptions of statistical analysis. However, when used by itself and with only a small number of cases, ordinal appraisal has the disadvantage of not providing a strong and clear basis for eliminating causal factors. Unlike nominal comparison, ordinal comparison does not necessarily permit the analyst to eliminate a potential causal factor because a single case deviates from an overall pattern of covariation. In fact, because ordinal comparison cannot deterministically eliminate rival explanations, this strategy may lead analysts to find empirical support for a great number of explanatory variables. Hence, the price of assessing partial causality may well be a loss of parsimony.

Narrative Analysis

In addition to systematically comparing cases with one another, Skocpol's work has a strong "narrative" component—that is, it analyzes revolutions as the product of unique, temporally ordered, and sequentially unfolding events that occur within cases (see Griffin 1992, p. 405). Sewell (1996*b*) points out that if Skocpol's causal argument had depended solely on comparisons of variables across cases, "there would have been no need to write a long book; a brief article with a few simple tables would have sufficed" (p. 262). The question is not really whether Skocpol uses historical narrative but rather how exactly her narrative contributes to the overall causal argument.

The ways in which narrative may or may not play a role in causal analysis have been extensively examined (e.g., Abrams 1982; Abbott 1990, 1992; Aminzade 1992; Griffin 1992, 1993; Kiser 1996; Quadagno and Knapp 1992; Rueschemeyer and Stephens 1997; Sewell 1992, 1996*a*, 1996*b*; Somers 1992; Stone 1979; and Stryker 1996). From these discussions, a consensus has emerged that narrative can be a useful tool for assessing causality in situations where temporal sequencing, particular events, and path dependence must be taken into account. Unfortunately, as Abbott (1992) suggests, methodologists have not offered many concrete illustrations of narrative assessment in actual research practice (but see Gotham and Staples [1996] and Stryker [1996]). In addition, far too little

has been said about the relationship between narrative and other strategies of causal assessment.

It is not possible to present a full summary of Skocpol's narrative argument here. However, figure 1 offers a schematic representation of her narrative for one piece of the overall causal argument: conditions for state breakdown in France. In the nominal comparison discussed above, these conditions were treated at a highly aggregated level as a single variable that was simply summarized as "present" (as indicated by the "yes" in the cell for France under the column "conditions for state breakdown" in table 1). When used in ordinal assessment, this variable was disaggregated into three constituent factors that were ranked across cases (see table 2). Quite obviously, the narrative appraisal in figure 1 takes causal complexity to an entirely different level, offering a historically detailed and nuanced understanding of state breakdown in France by disaggregating it into dozens of small steps. And figure 1 does not even do full justice to Skocpol's narrative argument. For example, the figure overlooks complex feedback linkages between variables, and it fails to summarize Skocpol's observations about the weighting of particular variables and specific understandings of the causal process through which one variable leads to another.

The numerous elements contained in figure 1 include statements that could be understood as involving nominal or ordinal measurement. Yet, narrative analysis is quite distinctive in relation to these alternative approaches. Whereas nominal and ordinal approaches involve broad, highly aggregated variables, narrative analysis entails a major shift toward disaggregation, along with a highly self-conscious focus on the historical sequences in which these disaggregated elements appear. Figure 1 thus offers a visual picture of what scholars such as Sewell (1996a, 1996b) have referred to as "eventful" analysis and what Skocpol and others call "conjunctural causation." In this strategy of causal analysis, the investigator gives "analytic weight to the conjunctural, unfolding interactions of originally separately determined processes" (Skocpol 1979, p. 320). Narrative analysis is always contingent on theory, and, although Sewell advocates eventful narrative as a form of causal assessment in its own right, narratives are often structured by other strategies of causal assessment (see Skocpol 1994, pp. 332–33). For example, Skocpol's narrative does not focus on events surrounding urban worker revolts precisely because this factor was eliminated in the nominal comparison. Likewise, the narrative analysis of international pressure in France is relatively brief precisely because the ordinal comparison revealed that this variable was comparably less important in the French case.

Figure 1 specifically shows how the variables used in Skocpol's ordinal assessment of conditions for state breakdown (i.e., agrarian backward-

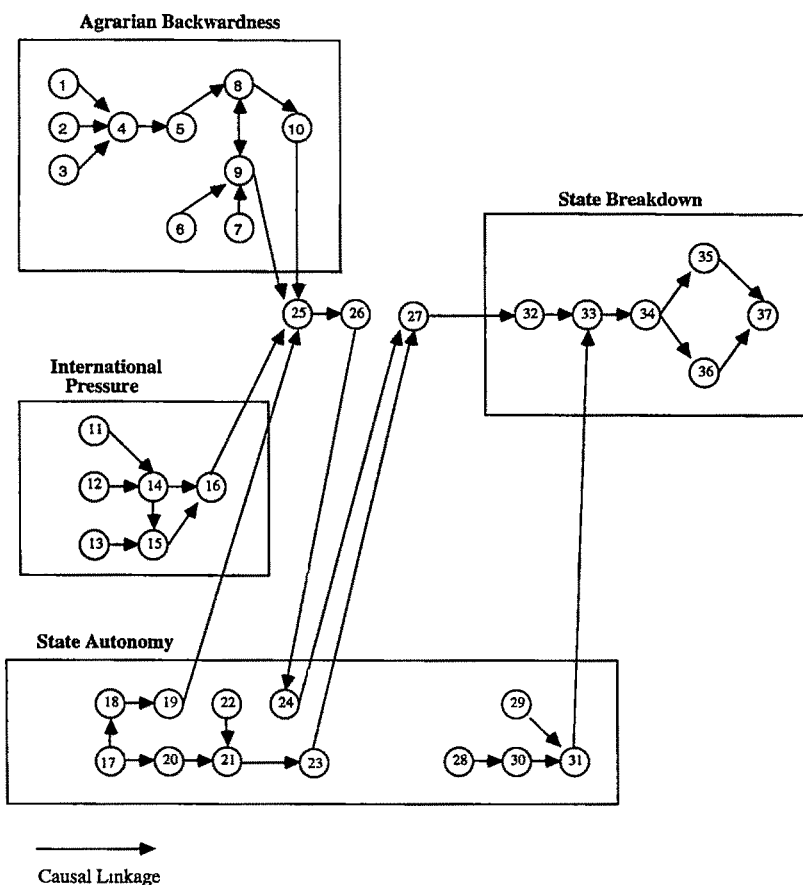


FIG 1.—Narrative analysis showing conditions for the state breakdown in France (all page numbers are from Skocpol [1979]):

1. Property relations prevent introduction of new agricultural techniques (p. 55)
2. Tax system discourages agricultural innovation (p. 55)
3. Sustained growth discourages agricultural innovation (p. 55)
4. Backwardness of French agriculture (esp. vis-à-vis England) (p. 56)
5. Weak domestic market for industrial goods (pp. 55–56)
6. Internal transportation problems (p. 56)
7. Population growth (p. 56)
8. Failure to achieve industrial breakthrough (p. 56)
9. Failure to sustain economic growth (p. 56)
10. Inability to successfully compete with England (p. 56)
11. Initial military successes under Louis XIV (p. 54)
12. Expansionist ambitions of state (p. 54)
13. French geographical location vis-à-vis England (p. 60)
14. Sustained warfare (pp. 54, 60, 63)

15. State needs to devote resources to both army and navy (p. 60)
16. Repeated defeats in war (pp. 54, 60, 61, 63)
17. Creation of absolutist monarchy; decentralized medieval institutions still persist (pp. 52–53)
18. Dominant class often exempted from taxes (pp. 60–61)
19. State faces obstacles generating loans (p. 61)
20. Socially cohesive dominant class based in proprietary wealth (pp. 56–59; 61–62)
21. Dominant class possesses legal right to delay royal legislation (p. 62)
22. Dominant class exercises firm control over offices (pp. 61–62)
23. Dominant class is capable of blocking state reforms (pp. 61–64)
24. Dominant class resists financial reforms (p. 62)
25. Major financial problems of state (p. 63)
26. State attempts tax/financial reforms (p. 64)
27. Financial reforms fail (pp. 63–65)
28. Recruitment of military officers from privileged classes (p. 65)
29. Military officers hold grievances against the crown (p. 65)
30. Military officers identify with the dominant class (p. 65)
31. Military is unwilling to repress dominant class resistance (pp. 64–65)
32. Financial crisis deepens (p. 64)
33. Pressures for creation of the Estates-General (p. 64)
34. King summons the Estates-General (p. 64)
35. Popular protests spread (p. 66)
36. Conflict among dominant class members in the Estates-General; paralysis of old regime (p. 65)
37. Municipal revolution; the old state collapses (pp. 66–67)

ness, international pressure, and state autonomy) are actually chains of separately determined, causally linked events that interact to produce state breakdown in France, an outcome that is itself made up of a series of causally linked events. The diagram illustrates the role that specific events play in Skocpol's argument, and it allows one to identify points of intersection and begin to see exactly how the three processes come together. Thus, the three causal chains meet at circle 25 to produce "major financial problems of the state," which is a key stepping stone leading to state breakdown. Of course, Skocpol's full argument concerning the causes of revolution in France is much more complex, because it includes the other macrovariable of "conditions for peasant revolt," which is made up of the constituent variables "peasant solidarity/autonomy" and "landlord vulnerability." Diagramming the points of intersection between the causal chains for both "conditions for state breakdown" and "conditions for peasant revolt" in France would be a more complicated task.¹²

¹² Indeed, the narrative analysis in fig. 1 summarizes only 17 pages (i.e., pp 51–67) of Skocpol's (1979) book.

The contribution of narrative analysis to Skocpol's causal argument does not merely involve providing an empirical basis for scoring cases in the overarching nominal and ordinal comparisons. Rather, the narrative makes an independent contribution: one criterion for judging a causal argument rests with the ability of an analyst to meaningfully assemble specific information concerning the histories of cases into coherent processes. Skocpol's causal argument is made much more convincing by the fact that she is able to order numerous idiosyncratic features of French, Russian, and Chinese history into meaningful accounts of unfolding processes that are consistent with a broader, overarching macrocausal argument. Furthermore, tracing historical processes in a given case over time can be a useful tool for eliminating potential explanations (George 1979; Rueschemeyer and Stephens 1997; Goldstone 1997). For example, narrative can be used to evaluate explanations by establishing at a more disaggregated level whether posited causal mechanisms plausibly link hypothesized explanatory variables with an outcome (Quadagno and Knapp 1992; Kiser 1996). Likewise, narrative can be used to assess rival explanations through a "pattern matching" procedure in which hypotheses are evaluated against multiple features of what was originally treated as only a single unit of observation (Campbell 1975). These procedures are used by Skocpol to eliminate potential causes of revolutions.¹³

When compared to nominal and ordinal comparison, narrative analysis has the obvious strength of allowing the analyst to show sensitivity to detail, process, conjuncture, and causal complexity. Successful narrative accounts are not subject to the criticism of "oversimplification." However, narrative appraisal carries with it key limitations. Most important, this form of assessment can obscure a study's overarching macroexplanation and thus seemingly undermine its theoretical parsimony. Indeed, narrative analysis introduces dozens of new variables that often play an underspecified role in the overall causal argument. For example, it would take hundreds of circles to diagram the full narrative argument in many works of macrocausal analysis. Should each of these circles be considered a separate variable? To what extent does the entire argument rest on the validity of each causal linkage in the narrative? Questions such as these are left unanswered in most narrative assessments, and thus some scholars have viewed narrative as an underspecified and nonrigorous form of causal investigation (see Griffin [1993] and Stryker [1996] for potential solutions to these problems).

¹³ For example, Skocpol (1979, pp. 170–71) argues that ideologically motivated vanguard movements were not an important cause of social revolution partly on the grounds that clear causal mechanisms linking these movements to revolutionary outcomes cannot be identified.

TABLE 3

STRATEGIES OF CAUSAL ASSESSMENT IN RECENT MACROCAUSAL ANALYSES

	Nominal Comparison	Ordinal Comparison	Narrative Analysis
Goldstone (1991) .. .	X		X
Wickham-Crowley (1992)	X	.	..
Ertman (1997)	X	...	X
Rueschemeyer, Stephens, and Stephens (1992)		X	X
Wuthnow (1989)	X
Clark (1995)	X
Aminzade (1993) ..		X	X
Luebbert (1991)	X	X	
Goodwin and Skocpol (1989)	X	X	..
Downing (1992) ..	X	X	X
Orloff (1993)	X	X	X
Collier and Collier (1991)	X	X	X

In sum, the Skocpol example shows that nominal, ordinal, and narrative strategies of causal appraisal can be meaningfully combined. The example also demonstrates the distinct strengths and weaknesses of each strategy. Nominal comparison contributes greatly to theoretical parsimony by providing powerful tools of elimination. However, methods associated with this technique require the analyst to adopt a deterministic understanding of causality. Ordinal comparison is a useful strategy for assessing partial causation and examining how the degree to which a variable is present influences an outcome. Yet, used by itself, ordinal comparison provides less incisive criteria for eliminating rival explanations and hence may lead to less parsimonious conclusions. Finally, narrative analysis allows scholars to remain highly sensitive to causal complexity, sequences of processes, and a more fine-grained understanding of historical detail. But, when used alone, this mode of analysis can lead to unparsimonious explanations that can be hard to generalize beyond an individual case. Hence, narrative may suffer from the problems associated with "idiographic" explanation.

CAUSAL ASSESSMENT IN RECENT WORKS OF COMPARATIVE HISTORY

Although Skocpol combines nominal, ordinal, and narrative appraisal, recent works reveal a variety of different combinations. This section systematically evaluates the strengths and weaknesses of various combinations. Table 3 lists the works considered and the primary strategies of

causal appraisal they adopt. These works are not the only (or necessarily the best-known) examples of macrocausal analysis. But they are nonetheless useful in illustrating the range of approaches to causal assessment in macrocausal analysis. These works all involve scholarship of high quality, thus facilitating an evaluation of the merits of these different methods independent of the investigators who use them. The instances in which the works do inappropriately use strategies of causal assessment reflect more general methodological issues that arise in macrocausal analysis and not just the failing of the particular researcher.¹⁴

Balancing Parsimony and Determinism

One methodological combination is found in works that use only nominal and narrative strategies. This combination has the virtue of giving scholars the freedom to employ the powerful eliminative logic of nominal comparison without having to further complicate matters by reassessing relationships in ordinal terms. The combination can thus narrow the range of hypothesized relationships down to a set of explanations that are elegant in their overall simplicity. Furthermore, the use of narrative in this combination can contribute to the persuasiveness of the explanation by allowing the analyst to show how a causal pattern appears valid even when assessed in light of great historical detail.

Yet a nominal-narrative combination is not without limitations. For one thing, the absence of ordinal comparison leaves the researcher without a powerful tool for evaluating partial and probabilistic causation. Furthermore, divorced from assessments of partial causation, historical narratives can become dry, mechanical stories in which the same causal pattern operates in case after case. Overall, then, the scholar using macrocausal analysis based on nominal and narrative appraisal faces challenges in avoiding overly deterministic explanations.

Jack Goldstone's *Revolution and Rebellion in the Early Modern World* (1991) is illustrative of this combination. Employing a variant of a most different systems design, Goldstone seeks to explain state breakdown during the English Revolution, the French Revolution, the Ottoman crisis, the Ming-Qing transition in China, and several other early modern cases by showing "how a common causal pattern lay behind all these events"

¹⁴ It should be noted that most of the works considered here examine at least two national cases. However, many recent macrocausal analyses use change over time or brief cross-national comparisons to assess causality in a single national case. For recent examples, see Bensel (1990); Skocpol (1992); Steinmetz (1993); Tilly (1995); and Sewell (1996a).

(p. 12).¹⁵ He uses nominal comparison extensively to reject potential explanations. For example, he argues (p. 19) that Skocpol's argument, which stresses the importance of military pressure for revolution, is "poorly suited" for explaining variation in early modern Europe. He points out that some revolutionary cases were relatively free from war (e.g., France in 1848), while other instances of nonrevolution were characterized by intense warfare (e.g., France in the late 17th and early 18th centuries). He thus concludes, "the incidence of war is neither a necessary nor a sufficient answer to the question of the causes of state breakdown" (p. 20). Many other explanatory factors are rejected either because they are not present across all cases of state breakdown or because they are present both in cases of state breakdown and cases of state stability.¹⁶

The explanation favored by Goldstone, the one that withstands his nominal-comparative tests, is a "demographic/structural" model that highlights how long-term population growth leads to state breakdown by affecting critical structural variables. In particular, population growth contributes to state financial crises, intra-elite and elite-state conflict, popular opposition, and transformative ideologies. Through a sometimes painstaking evaluation of the historiography, Goldstone shows how this causal pattern characterizes each case of state breakdown but not cases and periods of nonbreakdown. The overall explanation is, as he puts it, "quite beautiful in its parsimony" (p. 459), and it offers a fascinating vision of how gradual, long-term changes operating on several levels of analysis might come together to produce sudden episodes of dramatic transformation.

Goldstone is inventive in the organization of historical presentations, which helps him avoid the problem of presenting overly repetitious narrative accounts—a recurring problem in this kind of study. In the discussion of England, the first case examined, he carefully develops a quantitative model and presents a lengthy qualitative assessment. By contrast, less attention is given to the quantitative model in the discussion of France, although an extensive qualitative analysis is provided. Cleverly, Goldstone spares readers from extended historical discussion in his analysis of subsequent cases, just at the point when the narrative argument might begin to be repetitive.

¹⁵ Goldstone (1991, pp. 53–61) characterizes his methodology as an effort to identify "robust processes." However, his discussion of this method follows the logic of the most different systems design quite closely.

¹⁶ A nominal eliminative logic is used by Goldstone (1991) to reject the Whig explanation and revisionist accounts of the English breakdown (p. 67), explanations that emphasize the importance of the commercialization of agriculture to revolution (p. 146), sociological explanations of the French Revolution (pp. 171–73), and crude demographic explanations of the Meiji Restoration (pp. 405–6).

However, Goldstone's argument at times reads quite deterministically, as if explanatory variables had to produce revolution, and as if actors had no room to avert state breakdown given prevailing demographic trends and structural conditions.¹⁷ The narrative structure of the work in part reinforces this tendency toward determinism. The majority of Goldstone's narrative analysis is not devoted to analyzing contingent events and processes but rather to a sustained consideration of historical and sociological debates and to empirically demonstrating the presence or absence of particular causal variables. Precisely because a change in the scoring of a single one of his dichotomous variables might call into question the entire (nominal) argument, he devotes considerable attention to making sure that each variable is measured correctly. This effort allows Goldstone to demonstrate an extraordinary hold over historical material and often makes for engaging reading, but it does not mitigate the problem of determinism.

Two caveats should be made about this overall assessment of Goldstone. First, although his analysis is focused on a relatively small number of national cases, he supplements this small-*N* comparison with a statistical analysis based on a large number of within-case patterns. In particular, he operationalizes individual macrovariables in terms of a large number of quantitative measures and combines these measures into an overall "political stress indicator" that is evaluated statistically. However, this should not lead one to mistakenly conclude that the book is primarily a statistical analysis of revolutions. Rather, Goldstone's main goal is to use insights from brief statistical within-case analyses as supplementary evidence that supports the central nominal argument developed for a small number of national cases.

Second, Goldstone moves toward ordinal comparison in his discussions of England, France, and Germany in the 1830s and 1840s. In this sense, *Revolution and Rebellion* does contain some ordinal analysis. However, the overriding focus on validating the nominal argument seems to prevent Goldstone from fully employing the strengths of this strategy. An example can be found when, in one of the most interesting discussions of the book (pp. 311–34), Goldstone briefly moves toward ordinal comparison to suggest how political leadership might make an explanatory difference. Based on an implicit ordinal comparison, he argues that, despite substantial population pressures, revolution was less likely in both England and France in 1830 than during previous centuries because fiscal reforms had been put into place that served to control state crises and mute elite hostility.

¹⁷ Goldstone (1991) attempts to suggest that state breakdowns were not inevitable (see pp. 148–49), but this claim appears somewhat empty given that he asserts that his explanation would be falsified if his demographic and structural causes were present in an early modern case that did not experience a revolution (see pp. xxv–xxvi).

Thus, the period around 1830 is understood to be only *partially* conducive to revolution. In this context, Goldstone suggests that the reason England averted revolution in the 1830s, whereas France did not, rests with the contrast between the compromising political strategy pursued by William IV in England and the inflammatory actions of Charles X in France. Leadership therefore briefly appears as an additional variable that could fruitfully be added to the basic demographic/structural model. Yet, Goldstone ultimately avoids this conclusion, suggesting that the key difference between England and France might be due not to leadership but to the fact that key demographic/structural variables did not reach critical threshold points necessary to produce a revolution in England (pp. 322, 342). In the end, Goldstone's analysis primarily uses only nominal and narrative strategies, and his argument must stand or fall based on the strengths of this methodological combination.

The shortcomings of narrative analysis have led some scholars to abandon this strategy in favor of introducing more cases and more sophisticated forms of nominal comparison. This is true of Timothy Wickham-Crowley's *Guerrillas and Revolution in Latin America* (1992). In an effort to explain the emergence and relative success of guerrilla movements in modern Latin America, Wickham-Crowley employs Boolean algebra, which he correctly identifies as a systematic extension of Mill's methods of agreement and difference (p. 303). The use of Boolean algebra makes sense both because Wickham-Crowley seeks to assess processes of "multiple conjunctural causation" in which different combinations of explanatory factors produce the same outcome (Ragin 1987) and because after he divides countries into different periods and regions his case base is sufficiently large to meaningfully employ this method. Through the Boolean minimization procedure (see Ragin 1987, pp. 93–95), he eliminates many potential explanations and in the end is left with only a handful of combinations of variables to account for both the successes and failures of Latin American guerrilla movements.

Wickham-Crowley (1992) does not develop what Sewell (1996*b*) refers to as "eventful narratives" to support his causal explanation.¹⁸ Rather, his historical discussions focus on carefully evaluating potential hypotheses and sorting through evidence to assess whether explanatory variables are present or absent in specific cases. Unencumbered by an eventful narrative, the book seems more systematically comparative and more rigorous in its hypothesis testing than most works of comparative history. At the same time, the lack of an extended narrative gives the impression that no

¹⁸ In chaps 8 and 11, Wickham-Crowley (1992) presents somewhat more detailed narrative discussions. However, these discussions are brief and clearly play a secondary role within the overall analysis.

historical contingencies are at work, reinforcing the appearance of determinism in the nominal argument. Indeed, Wickham-Crowley makes little pretense of avoiding causal determinism. Rather, his approach is to maximize the strength of nominal comparison by employing the most sophisticated methods available, even if this means not attending to the nominal strategy's characteristic weakness.

Other scholars, such as Thomas Ertman in *Birth of the Leviathan* (1997), address the problem of determinism by attempting to reconcile nominal comparison with a focus on partial causation. Ertman carefully constructs a typology of four types of early modern regime states, and his goal is to explain why particular European cases developed specific types.¹⁹ He first rejects several competing explanations by using the eliminative logic of the methods of agreement and difference. In particular, he shows how the theories of state development advanced by Hintze (1975), Tilly (1985), Mann (1986), Anderson (1974), and Downing (1992) lead to predictions about early modern Europe that are contradicted by at least some of his cases (see Ertman 1997, pp. 12, 15, 18). Based on the inability of alternative theories to account for these "exception" cases, Ertman justifies the development of a new explanation. In an elegant presentation in chapter 1, he outlines this alternative explanation, showing how three dichotomously measured variables (i.e., administrative vs. participatory government, pre-1450 vs. post-1450 geopolitical competition, and existence vs. nonexistence of powerful representative assemblies) "can account for *most* of the variation" in early modern state building in Europe (p. 6, emphasis added).

Ertman carefully chose the word "most," for his explanation cannot account for *all* of the variation in outcomes among the 14 cases he considers. In particular, state development in Sweden and Denmark does not correspond to the pattern suggested by Ertman's explanation (see pp. 33, 266–67, 305–16). In these cases, "powerful contingent events conspired to confound expected paths of development" (p. 33). The presence of these exception cases leads Ertman to concede that his explanatory variables did not make outcomes inevitable; rather, "contingent historical circumstances" blew development processes off course (p. 320). With this concession, the author introduces the idea of partial and probabilistic causation into what is otherwise a deterministic nominal argument. Yet, in doing

¹⁹ At one point in the introduction, Ertman (1997) notes that he is interested in explaining variations within the same basic regime-state categories—e.g., why one case is more of an instance of a given regime-state outcome than another (p. 32). This suggests that he uses ordinal comparison. However, beyond a couple of sentences for France (see specifically pp. 35, 91, 110), he does not develop this line of analysis, and ordinal comparison plays virtually no role in the book.

so, he may well have introduced a double standard into the argument: how can Ertman reject other scholars' arguments on the grounds that they cannot fully explain the variation of interest, while preserving his own explanation, which also cannot fully explain the variation of interest? This issue is not satisfactorily addressed in *Birth of the Leviathan*, and thus the study does not convincingly demonstrate how nominal explanation can be used to assess patterns of partial causation.

The basic narrative structure of the book largely avoids the problem of presenting repetitious historical accounts that can plague works that employ a nominal-narrative combination. Because Ertman looks at four different outcomes, the reader remains engaged from chapter to chapter as new outcomes are explored. This is true even though Ertman's dense and contextually rich narratives remain carefully focused on the explanatory argument. In fact, with the exception of the awkward inclusion of the Swedish and Danish cases, which are tacked on at the very end of the book, Ertman's narrative is exemplary for avoiding the temptation of presenting long chronologies of events that are not focused on the central explanatory argument.

Analyzing Partial Causation in History

Other works of macrocasual analysis avoid the shortcomings associated with nominal comparison by employing only ordinal and narrative analysis. This combination allows the analyst to showcase the ordinal argument and elevate patterns of partial causality to center stage. Moreover, narrative analysis plays a major role in supplementing the ordinal assessment: rich historical investigation is used to identify the specific scoring of variables across cases. In this sense, macrocausal analysis based on ordinal and narrative causal assessment avoids causal determinism and blends narrative appraisal more smoothly into the overall argument than works that combine nominal and narrative strategies.

The distinct limitation of an ordinal-narrative combination, however, is that it cannot easily eliminate alternative explanations. When only a small number of cases are selected and a nominal strategy is not used, explanatory factors that partially covary with an outcome cannot necessarily be rejected. Thus, the practitioner of an ordinal-narrative combination may be prone to find that many variables are at least partially supported as a component of the explanation, such that an unwieldy number of factors may be identified as contributing causes. In addition, the ordinal assessment may well suggest that quite different combinations of scores on variables combine to produce the same outcome across cases, further complicating the causal argument. The result can be a high degree of causal indeterminism and a loss of theoretical parsimony.

Dietrich Rueschemeyer, Evelyne Huber Stephens, and John D. Stephens's *Capitalist Development and Democracy* (1992) is both a clear example of this combination and a good illustration of how the combination's limitations can be partially sidestepped. The primary goal of the book is to explain the origins of democracy in roughly 40 countries from 19th- and 20th-century Europe, North America, and Latin America. As is characteristic of scholars who choose not to rely heavily on nominal comparison, Rueschemeyer, Stephens, and Stephens tend not to eliminate potential causal variables outright but instead incorporate them into a highly encompassing theoretical framework.²⁰ Consequently, the authors identify a long list of variables that might contribute in partial and complex ways to democratization.

Building on quantitative cross-national work, which has repeatedly identified a positive correlation between economic development and democracy, the authors attempt to specify the variables that make up the intermediary sequences (i.e., causal mechanisms) linking development with democracy. Variables from three broad categories are considered. First, they examine several factors derived from a "relative class power model." Most importantly, Rueschemeyer, Stephens, and Stephens show that the presence of a strong working class is positively associated with democracy, while the presence of a strong landlord class is negatively associated with democracy. For example, in cases where the working class is weak and landed elites are strong, as in many Latin American countries, democracy has been infrequent and unstable (p. 270).

Second, several explanatory variables center on ordinal assessments of state power and state-society relations.²¹ For example, the authors explore how different levels of state autonomy affect the prospects for democracy. They argue that if "state autonomy" is defined as autonomy vis-à-vis non-elite groups in society, there is "an inverse relationship between state autonomy and democracy" (p. 65). By contrast, they suggest that if state autonomy is defined as autonomy vis-à-vis the dominant class, then increased state autonomy makes democracy more likely (pp. 64–66). Other state-centered variables concern ordinal differences in the organizational

²⁰ The authors reject certain deterministic hypotheses, but they do not usually eliminate specific variables from these hypotheses. For example, they reject Moore's (1966) hypothesis that the bourgeoisie is necessary for democracy, but they nonetheless still consider the bourgeoisie to be an important explanatory variable that affects a country's prospects for democracy.

²¹ Some of these variables are used in conjunction with nominal comparison—e.g., the claim that state autonomy is a necessary condition for democracy (Rueschemeyer et al. 1992, p. 64). However, the main thrust of the argument is based on ordinal comparison.

density of society and the position of coercive organizations in the state (pp. 66–68).

Third, several variables involve transnational structures of power. Especially with respect to these variables, the authors avoid stating any rigid generalizations because they believe the effect of transnational factors is strongly mediated by domestic factors. Thus, for example, they find no clear-cut relationship between extent of warfare, economic dependency, and political dependency and the emergence of democracy. The ways in which these factors contribute to democracy must be viewed in light of scores of other variables (pp. 69–73).

As this overview suggests, it is not possible to summarize Rueschemeyer, Stephens, and Stephens's argument in terms of a single causal pattern. Variables combine together in different ways to produce democracy. As a result, historical narrative becomes the tool through which the authors assess the specific causes of democracy across their cases. More precisely, historical narrative is used to establish the ordinal weighting of each variable and thus the particular combination of scores that led to democracy in a given case. Because the relative importance of specific causal factors is different for each country, the reader is offered substantially varied narratives from one case to the next.

On one level, the major limitations associated with an ordinal-narrative combination are plainly present in *Capitalist Development and Democracy*. Specifically, it is difficult to summarize the causal argument of the book because so many factors and potential causal paths are introduced. In this sense, the argument might be viewed as suffering from causal indeterminism. Yet, on another level, Rueschemeyer, Stephens, and Stephens partially overcome this problem by identifying "relative class power" as their most important set of variables (p. 5).²² When looked at in light of only these particular variables, the argument seems manageable. This privileging of one set of variables as more important than others is of course a standard technique in multivariate statistical research. But the procedure is used less often in macrocausal analysis. Rueschemeyer, Stephens, and Stephens's study suggests how analysts who do not employ numerical coefficients can nonetheless weigh the importance of variables through close qualitative appraisal.

A desire to have more eventful narratives has contributed to calls for more fully historical approaches to comparative history. In recent years, several works have appeared that employ primarily only narrative ap-

²² The authors do not simply arbitrarily single out class-power factors as the most important variables. Rather, the empirical evidence suggests that these factors best explain the robust correlation between development and democracy—a correlation that cannot be well explained by state or international power complexes

praisal to assess causality, carefully avoiding for the most part nominal and ordinal comparative techniques. This might be seen as an attractive option by scholars who seek to "free" narrative from the constraints imposed on it by logical comparative methods (e.g., Sewell 1996a, 1996b). On the other hand, narrative appraisal has been criticized on its own terms. Scholars have argued that narrative, when not supplemented by alternative strategies, is "mere description" or "story telling" (see Griffin 1992).

Robert Wuthnow's *Communities of Discourse* (1989) and Samuel Clark's *State and Status* (1995) are examples of recent studies that use primarily narrative appraisal to assess patterns of causation. Yet, it is interesting that these are also works that only partially and imperfectly correspond to macrocausal analysis. Thus, while at certain points the studies seek to make causal inferences about macro-level structures and processes, they are also centrally concerned with using narrative to offer meaningful *interpretations* of history that show sensitivity to the culturally embedded intentions of individual and group actors. This concern with interpretation moves these works close to what Skocpol and Somers (1980) call contrast-oriented comparative history. In this mode of comparative history, narratives are structured by—and carefully organized around—certain broad concepts and orienting themes that enable the analyst to offer a commentary on the distinctive features of each case.

Indeed, the books by Wuthnow and Clark suggest that the use of narrative *in the absence of* nominal or ordinal methods may be better suited for description and interpretation than for causal inference. Thus, Wuthnow's narrative analysis is most effective when using broad concepts introduced at the beginning of the book (e.g., production, selection, institutionalization) to develop an interpretive account that illuminates important meaningful processes that characterized the Protestant Reformation, the Enlightenment, and the rise of Marxist Socialism. When assessing variables that other scholars have introduced to *explain* these events, Wuthnow's narrative account is less convincing. For example, he rejects class-based variables mainly on the grounds that they do not prove illuminating when understanding the historical meaning and interpretive significance of the processes that constituted the three major cultural innovations under consideration (pp. 566–70). However, a variable need not be interpretively illuminating to be useful in causal assessment, and thus analysts expressly concerned with causal inference may believe Wuthnow's narrative account provides weak grounds for rejecting class-based variables.

Similar remarks can be made about Clark's new book on state building and aristocratic elites in Western Europe during the early modern period. Clark pursues narrative analysis through the lens of four "large processes":

commercialization, centralization, differentiation, and status. Although he claims to draw on an ordinal/correlational method (p. 23), most of his analysis involves a straightforward narrative assessment of how these large processes and their respective subprocesses played themselves out in contrasting ways across and within the British Isles and France, Savoy, and the Southern Low Countries. The difficulty Clark finds in using narrative for the purpose of causal analysis becomes clear in a section in the concluding chapter entitled, "The Causes of the Differentiation of Power." After reflecting on his narrative account, Clark writes that "While there has been a master trend toward power differentiation over the centuries, during the period covered by this book there was no master *cause* of this trend. . . . The lesson is that if sociologists want to understand the master trend of differentiation they will have to give greater consideration to the *particularities* of history" (p. 370; emphasis added). It is not surprising that Clark reaches this conclusion, for his narrative-based approach is better suited for *interpreting* the historical meaning and significance of power differentiation than for *explaining* its origins.

Finally, it must be noted that even when scholars eschew ordinal and nominal strategies in favor of narrative analysis, their causal arguments may inadvertently be drawn back to these strategies. An example is found in *Ballots and Barricades*, a fine book by Ronald Aminzade (1993). The central question Aminzade poses is why, despite many important similarities, did the French cities of Toulouse, Saint-Étienne, and Rouen have revolutionary communes marked by dramatically different balances of power between liberals, radicals, and socialists during the 1870–71 period. Explicitly seeking to follow Sewell's agenda for an event-centered historical sociology, Aminzade suggests that his argument "takes the form of analytic narratives—that is, theoretically structured stories about coherent sequences of motivated actions" (p. 27). Aminzade's explanation is indeed highly sensitive to issues of timing, path dependence, and event sequencing, but his "analytic narratives" are also clearly structured by more formal comparative methods, in particular the method of concomitant variation. Thus, potential causal factors such as degree of industrial backwardness are called into question because they do not covary with the relative strength of liberal, radical, and socialist movements in the three cities (p. 4). And the main explanatory argument offered by Aminzade—which highlights the importance of the relative timing of local economic development and class struggles and the degree of worker political autonomy vis-à-vis local Republican Party formation—is based in substantial measure on cross-case, ordinal comparison (pp. 252–56). In this sense, *Ballots and Barricades* has at least as much methodological common ground with *Capitalist Development and Democracy* as with books by Wuthnow and Clark.

Rigorous Hypothesis Testing and the Problem of Frozen History

Notwithstanding recent calls for a more eventful sociology, some macrocausal analysts employ primarily only nominal and ordinal strategies, opting not to include extensive narratives in their studies. Because narrative can have a somewhat ambiguous relationship to causal analysis, works that employ a nominal-ordinal combination may appear to offer a notably rigorous form of hypothesis assessment. Indeed, this combination allows all scholarly energy to be directed toward testing hypotheses through systematic, cross-case comparisons. And because hypothesis testing takes place using *both* nominal and ordinal comparison, the overall impression of a thorough causal assessment is reinforced.

Yet, from the perspective of many historical sociologists, the exclusion of narrative analysis can lead to overly rigid causal arguments in which eventful processes, sequencing, and timing are not given sufficient attention. As Burawoy remarks (1989, pp. 769–70), macrocomparative analysts may “freeze history” by ignoring sequences of processes and interconnections among variables. Furthermore, if indeed one important litmus test of a good comparative-historical argument is the ability of a scholar to make sense of unique and sequentially unfolding processes within cases, then studies that do not use narrative analysis simply lack a major tool of causal evaluation.

Gregory Luebbert's *Liberalism, Fascism, or Social Democracy* (1991) exemplifies a nominal-ordinal combination. In conjunction with his nominal assessment, Luebbert implicitly employs Mill's methods of agreement and difference to uncover the origins of liberal democratic, fascist, and social democratic regimes in interwar Europe. Drawing on the powerful eliminative logic of these methods, he rejects several existing explanations of interwar regimes. For example, Luebbert argues that explanations of fascism that point to the depression and high inflation “simply do not stand up to comparative analysis” (p. 307). Some cases experienced economic downturns and inflation as significant as the fascist cases yet did not develop fascist regimes. Conversely, at least one case (i.e., Spain) that developed a fascist regime “neither suffered from hyperinflation nor experienced the depression deeply” (p. 308). In this sense, the depression and inflation are neither necessary nor sufficient causes of fascism and can be rejected.

Luebbert's preferred explanation centers on political coalitions among social classes. He holds that liberal-democratic vs. non-liberal-democratic regime outcomes can be explained based on whether a “lib-lab” alliance pattern (i.e., an alliance between liberals and the labor movement) was present before World War I. In all of the liberal-democratic cases, this alliance pattern was present; in all of the non-liberal-democratic cases,

this alliance was absent. Hence, the dichotomous explanatory variable of "lib-labism" is perfectly correlated with liberal democracy. Luebbert also explains differences among nonliberal cases by exploring whether the peasantry united with middle classes, which is argued to have led to fascist regimes, or whether the peasantry united with socialists, which is argued to have led to social-democratic regimes. Here again, the author identifies invariant relations between his categorical explanatory variables and outcomes.

Luebbert's argument also explicitly draws on ordinal comparison (pp. 3–4). He (informally) reconceptualizes his dichotomous explanatory variable of lib-labism as an ordinal scale and ranks countries based on the degree to which they experienced lib-labism.²³ He also ordinally ranks countries in terms of the extent to which they developed liberal-democratic regimes. These rankings allow him to assess the covariation between the two variables. One of the interesting features of Luebbert's argument is that, in the ordinal assessment, there is substantial *but not perfect* covariation between lib-labism and liberal democracy.²⁴ Despite the less than perfect fit, Luebbert suggests that the association is strong enough to conclude the variables are causally related. Hence, when dichotomous categories are converted to ordinal variables, Luebbert adopts a probabilistic understanding of causation in order to argue that a pattern of strong but not perfect covariation reflects causation.

Liberalism, Fascism, or Social Democracy does not contain significant chronological narratives of unfolding events and processes. Rather, Luebbert's main focus is on using historical evidence to rigorously measure nominal and ordinal appraisal variables and employ them to test causal hypotheses; he leaves it to the reader to piece together the chronological histories and event sequences that surrounded the development of interwar regimes in Europe. Consequently, Luebbert's explanation is relatively static and "uneventful" when compared to narrative-based works. Although Burawoy (1989) charged Skocpol (1979) with freezing history, the accusation is actually much more appropriate for Luebbert.

Because narrative analysis can entail the presentation of lengthy chronologies, this strategy may be of necessity, precluded in article-length manuscripts that seek to develop and test major hypotheses in relatively few pages. For example, Jeff Goodwin and Theda Skocpol's "Explaining Revolutions in the Contemporary Third World" (1989) summarizes the ordinal and nominal arguments developed more fully by Goodwin (1988) in his doctoral dissertation, but—likely for reasons of space—skips the

²³ Luebbert (1991) actually uses the variable of prewar liberal hegemony, which is a proximate measure of lib-labism.

²⁴ I have discussed this elsewhere (Mahoney 1998).

analytical narratives presented in the larger work. When compared to the dissertation (now a forthcoming book [see Goodwin forthcoming]), the article illustrates how studies that use only nominal and ordinal comparison can rigorously test hypotheses through systematic comparisons but cannot show an appreciation for densely contextualized and sequentially unfolding historical processes.

Goodwin and Skocpol use nominal comparison to eliminate several potential explanations of Third World revolutions. For example, they implicitly use the method of difference to eliminate poverty and professional revolutionary organizations as causes of revolutions: these factors are present widely in both cases of revolution and nonrevolution in the Third World and thus cannot explain why some countries have revolutions while others do not (pp. 490–91; see also pp. 497–98 for other uses of the method of difference). However, they do not use nominal comparison to develop their own favored state-centered explanation. Rather, this explanation is based on variables that are conceptualized continuously and, when actually applied to specific cases, assessed as ordinal categories (see p. 504 fig. 1.). Specifically, Third World countries with low state bureaucratization, low levels of state penetration of national territory, and limited state incorporation of social groups are argued to be especially vulnerable to revolution. By contrast, high levels on each of these variables make revolution very unlikely. For cases that exhibit intermediate levels on one or more of these variables, such as Peru and Guatemala in the late 1980s, the likelihood of revolution is somewhere in the middle (p. 503). Consistent with ordinal appraisal, Goodwin and Skocpol (pp. 496–500) state their hypotheses in probabilistic terms—that is, as making outcomes “more likely” or “less likely.”

The fact that Goodwin and Skocpol employ nominal comparison (which is associated with causal determinism in small-*N* analysis) to evaluate other scholars' arguments, but use ordinal comparison (which is compatible with probabilistic causation) to develop their own explanation, might lead one to conclude they have created a double standard similar to the one that arose in relation to Ertman's book. Yet, when Goodwin and Skocpol eliminate alternative explanations using the method of difference, they show not only that a few deviant cases fail to conform to the expected causal pattern but that *most* cases do not conform to the anticipated result. For example, *most* countries with poverty and professional revolutionaries have not experienced revolution. Given that the alternative explanations rejected by Goodwin and Skocpol have no apparent relationship with Third World revolutions, it seems likely that these explanations would have been eliminated even if methods that allow for the assessment of partial causation were employed. In this sense, Goodwin

and Skocpol do not appear to hold alternative explanations to a different standard of falsification than their own explanation.²⁵

Recent Studies that Combine All Three Strategies

Finally, some recent studies have combined all three strategies, if not always in the same exact manner as Skocpol's *States and Social Revolutions* (1979). As suggested in the discussion of Skocpol, this combination has the merit of helping to balance out the specific limitations associated with each strategy. For example, the characteristic weaknesses of a nominal strategy are seemingly countered by the use of ordinal and narrative strategies, the weaknesses of narrative analysis are overcome by the strengths of nominal and ordinal comparison, and the limitations of ordinal appraisal find antidotes in narrative and nominal assessment. To at least some extent, then, a "triple combination" of strategies brings together the best of all three worlds.

However, the triple combination requires analysts to develop complex, multilayered explanations. This methodological complexity of necessity breeds some loss of parsimony. Indeed, identifying all of the main variables and understanding the overall causal argument in studies that employ a nominal-ordinal-narrative combination can be a quite challenging task. In order to mitigate confusion, scholars may emphasize primarily the nominal argument when they summarize their findings and methodology at the beginning and end of their studies (this was true with Skocpol). They may also lean heavily on a nominal strategy throughout the analysis, using ordinal comparison and narrative primarily to add nuance and credibility to the main nominal argument. Yet, the fact that all three strategies are actually being used often fuels tensions and apparent contradictions. For example, assuming that variables are not disaggregated when moving from nominal to ordinal comparison, analysts must choose whether a given narrative passage corresponds with the nominal assessment or the ordinal assessment. Confusion over whether the narrative analysis is highlighting the nominal or ordinal explanation may lead to uncertainty concerning the relevance of narrative passages to an overall argument. Likewise, if an author identifies a study's methodology as entailing only a nominal strategy, the use of ordinal and narrative analysis throughout the

²⁵ By contrast, Ertman (1997) fails to make a convincing argument why his explanation, which cannot account for two cases, should be accepted over alternative explanations, which cannot explain four cases (the theories by Hintze [1975], Tilly [1985], Mann [1986], Downing [1992], Anderson [1974] each appear to be unable to account for four of Ertman's 14 cases—see Ertman [1997], pp. 12, 15, 18).

actual text of the study may appear to readers as logically inconsistent with the author's stated methodology and analytic summary remarks.

One notable recent example of the triple combination is Brian M. Downing's *The Military Revolution and Political Change* (1992). Explicitly building on the work of Barrington Moore (1966), Downing sets out to explain the origins of liberal democracy and bureaucratic absolutism in Europe. His macroexplanation is based on nominal comparison, and it unfolds in two steps. First, Downing (1992) argues that medieval Europe developed a set of institutions and power relations (e.g., a balance between crown and nobility, decentralized military systems, and peasant property rights) that distinguished it from all other world civilizations and predisposed the region to democracy (pp. 18–55). In effect, he uses the method of agreement to show that this "medieval constitutionalism" was necessary but not sufficient for democracy. Second, drawing on the method of difference, he explains variation in political regime outcomes among European countries (i.e., democracy vs. absolutism) by reference to the "military revolution" of the 16th and 17th centuries. Specifically, Downing argues that countries that experienced a high level of warfare *and* mobilized extensive domestic resources to finance modern armies undermined their predisposition to democracy and built nondemocratic, absolutist political systems. By contrast, if either or both of these two processes were absent, then the medieval constitutional heritage was preserved and liberal-democratic systems were eventually developed.

Downing also assesses causal patterns using ordinal comparison, though, unlike Skocpol, he does not disaggregate nominal causal variables into constituent processes. Instead, he reassesses nominal explanatory variables as ordinal variables at the same level of aggregation (see p. 242). For example, in his discussion of the origins of bureaucratic absolutism in 17th-century Prussia and France, Downing seeks not only to understand why these countries developed the same (dichotomously measured) regime outcome. Rather, one of his major concerns is to understand why France developed a less extensive form of absolutism, which in turn allowed for "a second chance for liberal democracy" (p. 127). He explains this ordinal difference (i.e., a very high level of absolutism in Prussia versus a moderate level in France) in terms of ordinal differences on his key explanatory variable of domestic mobilization of resources to finance army building. Specifically, he suggests the French economy was wealthier and the French dominant class stronger than its Prussian counterparts, which meant that the French state mobilized fewer domestic resources and thus preserved more of its medieval constitution heritage (pp. 127–32).

Yet, this is not the full story in explaining the difference between France and Prussia. Downing also offers a rich narrative discussion of how causally connected events unfolded in contrasting ways in the period following

the military revolution. For instance, because French political institutions providing for an independent nobility were not completely wiped out (as they were in Prussia), the French aristocracy remained powerful enough to effectively oppose the monarchy. Likewise, peasant communities with a high capacity for revolt were not overwhelmed by totalizing bureaucracies as in Prussia. These processes, uncovered and developed through historical narrative, help explain why France, and not Prussia, was able to get another chance for liberal democracy (pp. 132–38).

The France-Prussia example is merely one illustration of how Downing combines nominal, ordinal, and narrative appraisal; others are not hard to find.²⁶ In one sense, because *The Military Revolution* uses all three strategies, its methodological strengths and weaknesses parallel those of *States and Social Revolutions*. However, a major difference merits emphasis. Downing's shift from nominal to ordinal appraisal does not involve a radical reconceptualization and disaggregation of variables. Consequently, when compared to Skocpol's explanation, the logic of Downing's argument may seem easier to follow and provoke less confusion. At the same time, however, Downing's causal argument may appear to lack the methodological sophistication of Skocpol's, precisely because his argument does not operate on multiple levels of conceptual aggregation.

Like Skocpol and in contrast to Downing, Ann Shola Orloff in *The Politics of Pensions* (1993) does disaggregate variables when shifting from nominal to ordinal assessment. Orloff's primary objective is to explain variations in the timing and character of social programs for the aged in Britain, Canada, and the United States. Specifically, she asks (a) why did Britain adopt old-age coverage before World War I, whereas the United States and Canada did not, (b) why in the 1920s did Canada adopt a federal pension law, whereas the United States did not, and (c) why in the 1930s did the United States adopt a nationwide program for the elderly, whereas Canada failed to adopt contributory insurance? Orloff uses nominal comparison (principally the method of difference) to generate the following answers:²⁷ (a) the British state was capable of initiating and

²⁶ An excellent example is Downing's (1992, chap. 7) discussion of England. The nominal comparison shows how early modern England lacked the warfare and domestic mobilization of resources necessary to undermine its medieval predisposition to democracy. However, the ordinal comparison demonstrates that varying levels of warfare and domestic resource mobilization led to differing degrees of constitutionalism across three different time periods (i.e., pre-1642, 1648–82, and 1682–1713). These three time periods are analyzed through a fascinating narrative that nicely blends together both the nominal and ordinal arguments.

²⁷ Orloff (1993) presents a detailed methodological discussion that highlights the ways in which she combines Millian methods, a most similar nations design, and elements

managing complex social programs, whereas the U.S. and Canadian states were not, (b) the U.S. state was extremely fragmented, whereas the Canadian state was not, and (c) the U.S. state had enhanced capacity after 1930, whereas the Canadian state was hampered by constitutional roadblocks.

Anyone who has read *The Politics of Pensions* will immediately realize that Orloff's causal argument is much more sophisticated than this summary suggests. The three nominal explanatory factors listed above are merely aggregated summaries of constituent factors that are often specified in ordinal terms. For example, the explanatory factor of "state capability" used to account for pre-World War I differences between Britain, on the one hand, and the United States and Canada, on the other, is actually composed of several subvariables, including degree of patronage, extent to which state officials can negotiate policy reforms, and willingness of political elites to adopt social reform measures (e.g., pp. 302-3). When evaluated through ordinal comparison, the United States and Canada do not share the same score on several of these constituent explanatory variables, even though they experienced the same outcome (i.e., failure to adopt old-age coverage). For example, political elites in the United States were more strongly opposed to social spending than Canadian elites, and state officials in Canada had less capacity to negotiate policy reforms than U.S. state officials (pp. 253-58, 303). Like Skocpol, Orloff appears to have implicitly followed an additive strategy in which ordinal subvariables are summed together to generate a "total score" that underpins the dichotomous scoring of state capacity used in the nominal assessment. Hence, when all subvariables for state capacity are (informally) added together, the United States and Canada have similar total scores.

Narrative analysis is also a major component of the book's causal argument: Orloff further disaggregates subvariables into multiple events and processes that intersect to produce pension outcomes. For instance, the ordinal variable of "willingness of elites to adopt social reform" mentioned above is actually composed of several causally connected events. For the Canadian case, the causal chain would read something like this: late state development → limited national supervision of local poor relief efforts → limited awareness of need for social welfare reform → political elites lack interest in public social spending → little elite backing of spending measures (pp. 253-58). With the addition of narrative analysis, Orloff's argument becomes extremely complex. Indeed, because the book seeks to explain multiple outcomes (only Orloff's three most important outcomes were listed above), the reader is pushed to the limit to hold together the overall argument as Orloff moves not only from nominal to ordinal to narrative

of Boolean algebra (see pp. 23-38). Unfortunately, this entire discussion focuses on the nominal comparison, overlooking ordinal and narrative analysis altogether.

assessment but also from outcome to outcome.²⁸ For some readers, the extensive investment of time and energy needed to hold together the full argument may prove too taxing.

Ruth Berins Collier and David Collier's 877 page *Shaping the Political Arena* (1991) also demands much time and energy from the reader if its causal argument is to be seriously analyzed in the manner it warrants. When compared to most other macrocausal works, this book is distinctive because it first uses *ordinal* comparison to assess highly aggregated causal variables and then disaggregates these macrovariables into constituent variables that are assessed using *nominal* comparison. These constituent nominal variables are then further evaluated using narrative analysis. Hence, Collier and Collier move from ordinal to nominal to narrative appraisal.²⁹

Collier and Collier's main argument shows how different labor incorporation periods in eight Latin American countries set in motion a chain of political events that led to major contrasts in the character of national party systems. The authors identify four types of labor incorporation periods that reflect ordinal contrasts in the "mode and scope" of incorporation: state incorporation, electoral mobilization by a traditional party, labor populism, and radical populism. Although these four types of incorporation periods explicitly reflect a rank ordering, each type is formally defined in terms of constituent dimensions that are measured dichotomously (see pp. 166–67).³⁰ These constituent dimensions are used in the analysis as *variables* to help explain contrasts among the cases. In this way, when incorporation periods are disaggregated into subvariables, Collier and Collier in effect move from ordinal to nominal appraisal.³¹

This distinctive approach has interesting implications for the question of probabilistic versus deterministic causation. First, because Collier and Collier's most general argument employs ordinal comparison, the authors are well positioned to treat causation as partial and probabilistic at the

²⁸ Orloff (1993) also uses the method of agreement to explain similarities between the three cases (e.g., she attempts to locate the "necessary and sufficient conditions" for the adoption of welfare programs in the three countries [p. 36; see also pp. 299–301]). Furthermore, another entire set of comparisons for the United States and Canada involve longitudinal assessments of change over time (p. 35).

²⁹ A similar methodological approach is used (implicitly) in Sohrabi (1995).

³⁰ Collier and Collier are quick to point out (1991, p. 163), however, that the dichotomous categories do not fit every case perfectly.

³¹ Collier and Collier's analysis is actually even more complex than this suggests: nominal constituent variables are sometimes also assessed using ordinal comparison. Thus, Collier and Collier (1991) not only move from ordinal comparison to nominal comparison by disaggregating macrovariables but also return to ordinal comparison in their assessment of disaggregated variables.

macrolevel. Indeed, Collier and Collier explicitly state that incorporation periods are "explanatory factors that must be looked at in conjunction with other explanations and as important explanations that make certain outcomes more likely, but not inevitable" (p. 20; see also pp. 38–39, 511). For the Colliers, "the relationships under analysis are probabilistic and partial" (p. 20). Second, ordinal assessment at the macrolevel leads the authors to have more difficulty eliminating alternative explanations than do scholars who employ nominal comparison at the macrolevel. Collier and Collier must acknowledge that many other variables, including variables not analyzed in their work, probably played an important role in producing the outcomes under investigation.

Third, at lower levels of conceptual aggregation, including in the narrative assessment, the argument often reads more deterministically. Nominal comparison lurks just beneath the macroordinal argument, and this type of comparison contributes to a sense that many development processes were structurally determined by the type of incorporation period a country experienced. For example, important contrasts on dichotomously measured outcomes between cases of labor populism and cases of incorporation by a traditional party can be traced back to a single dichotomous dimension that distinguishes the two types of incorporation periods.³² Hence, in contrast to Skocpol, Orloff, and Downing, Collier and Collier's book often reads more deterministically when macrovariables are disaggregated into constituent variables.

CONCLUSION

Macrocausal analysis has often been characterized as following a single strategy of causal inference (e.g., Skocpol and Somers 1980; Skocpol 1984; Lieberman 1991). Yet, in fact, at least three different strategies of causal appraisal—nominal comparison, ordinal comparison, and narrative analysis—are used. In some cases, a study may employ only one of the strategies; more commonly, it will draw on two, or sometimes all three. Since the alternative strategies have been combined in diverse ways, a spectrum of different approaches characterizes contemporary macrocausal analyses. By way of conclusion, it is appropriate to summarize the varying merits and shortcomings of these different approaches.

³² The dichotomous outcomes considered are: whether the party that oversaw labor incorporation was subordinate in the new regime established following the incorporation period, whether the union movement was linked to the center, and whether centrist parties had a majority in the electoral arena. The dichotomous variable that ultimately explains these differences is whether the political party that led the incorporation period established a strong organizational link to the union movement.

When used by themselves, nominal, ordinal, and narrative strategies have their own strengths and weaknesses (see table 4). The three strategies might be viewed along a continuum: nominal comparison tends to yield parsimonious and deterministic explanations, ordinal comparison tends to yield less parsimonious and less deterministic explanations, and narrative analysis tends to yield idiographic and contingent explanations. Because of such differences, the strategies are partly in tension with one another. And when two or more of them are combined, these tensions may rise to the surface. At the same time, however, the combination of strategies can serve to balance out the respective biases and blind spots of each. Combining strategies therefore leads to a complex set of trade-offs.

Perhaps the most widely employed combination involves the simultaneous use of nominal and narrative appraisal. This combination is appealing because it enables the investigator to develop an argument that is parsimonious yet shows great sensitivity to historical detail. In this sense, practitioners of a nominal-narrative combination can claim to bring together the virtues of both sociology and history. However, they do so by resolving the tension between nominal and narrative strategies in favor of the former: narratives are structured around the nominal argument and thus tend to read deterministically. Nevertheless, to the degree one believes macrocausal analysts should think about causation in terms of necessary and sufficient conditions, this combination may hold the most promise for research.³³

The approach of combining ordinal and narrative analysis has been adopted less frequently by scholars, which is unfortunate given that it offers a powerful means of assessing causality. Ordinal comparison is an important tool for evaluating *partial* causation, and this strategy blends well with narrative analysis: narrative becomes the means through which the analyst identifies the specific scores on variables that combine to produce the outcome of interest. The shortcoming of an ordinal-narrative combination is that it lacks nominal eliminative tools, and thus it becomes difficult to develop a parsimonious explanation. Yet, for scholars who reject the deterministic assumptions of nominal comparison, an ordinal-narrative combination offers an attractive alternative.

Although narrative has recently received much attention in the field of historical sociology, some recent works adopt a nominal-ordinal combination. This combination enables the researcher to direct full attention to testing hypotheses through logical, cross-case comparisons without allowing the argument to become "bogged down" in the minutia of narrative detail. When done well, this strategy can lead to arguments that are

³³ Ragin and Zaret (1983), Ragin (1987), and Skocpol (1984) advocate this understanding of causation.

TABLE 4
APPROACHES TO MACROCAUSAL ANALYSIS: STRENGTHS AND WEAKNESSES

	Strength	Weakness
Nominal strategy	Contributes to explanatory parsimony	Entails a deterministic view of causation
Ordinal strategy	Allows for the assessment of partial causation	Cannot easily eliminate rival explanations
Narrative strategy	Permits processual analysis of temporally sequenced events	Can produce idiographic analyses
Nominal-narrative combination	Develops parsimonious explanations grounded in a sensitivity to historical detail	Explanation and narrative account tend toward determinism
Ordinal-narrative combination	Blends probabilistic causal assessment with meaningful narrative account	Explanation may appear to be causally indeterminate
Nominal-ordinal combination	Offers a thorough and logically compelling causal assessment	Explanation is subject to the criticism of "freezing history"
Nominal-ordinal-narrative combination	Serves to partially balance out characteristic weakness of each strategy	Complexity of explanation may contribute to a loss of parsimony

especially rigorous in their comparative-causal logic. However, as advocates of narrative strategies of causal inference would be quick to point out, much is lost with the exclusion of narrative analysis. Indeed, the nominal-ordinal combination is highly vulnerable to charges of freezing history and ignoring processes of sequencing and historical contingency.

Finally, scholars have combined and will continue to combine all three strategies. Works that adopt this triple combination embody the characteristic weaknesses of each of the three strategies, and, as we saw with the Skocpol example, it is possible to criticize such works from a variety of perspectives. However, these works also embody the characteristic strengths of each strategy. Provided readers recognize that all three strategies are being used (something which has not been true with Skocpol), this approach can serve to balance out the respective biases of each strategy. In this sense, there is a strong argument to be made in favor of combining all three strategies. The drawback, however, is that the combination inevitably leads to explanations that encompass several layers of methodological complexity, and thus the overall argument may be difficult to grasp. For some scholars, the gains offered by this approach may not be worth the costs imposed by added methodological complexity.

Since each methodological strategy and combination carries its own strengths and limitations, no one approach is inherently better than the rest. This raises the issue of the procedure through which an analyst selects a given approach. Individual scholarly tastes and preferences no doubt play a major role in this selection process. For example, analysts who strongly value parsimony and are less concerned with avoiding causal determinism will be attracted to the nominal strategy. Investigators who reject deterministic methods, by contrast, will likely find virtues in ordinal and narrative strategies. In addition, analysts may employ the approach that is most congruent with their distinctive skills and talents, allowing them to showcase their academic strengths. For instance, the scholar who is highly skilled at writing detailed, eventful chronologies may well be drawn to the narrative strategy. In this regard, it is worth emphasizing that any of these approaches can be poorly applied; methodology is no substitute for investigator aptitude.

Yet, the decision to employ a particular approach is not simply a matter of scholarly tastes and skills; it is also structured by broader methodological considerations. For example, data limitations may not permit the use of ordinal comparison, especially if only two cases are selected for analysis (at least three observations on a variable are needed for ordinal analysis). Likewise, narrative analysis is quite difficult to carry out in relatively few pages and thus may be avoided in article-length publications. Furthermore, the nature of the research question posed by the investigator may influence which methodological approach is adopted. For instance, if an

analyst inquires about the causes of ordinal differences on some outcome variable, this investigator may well be drawn to an ordinal strategy in the overall causal argument. Likewise, for a particular research question, a given investigator may feel that it is impossible to capture the complexity of a causal pattern without a narrative assessment. Yet, for a different research question, this same investigator may feel that narrative analysis provides an unnecessarily complex approach to the assessment of a relatively simple causal pattern.

Finally, the adoption of a given approach may well be conditioned by ongoing research cycles within the field of macrocausal analysis. In their seminal discussion of "Uses of Comparative History in Macrosocial Inquiry," Skocpol and Somers (1980) suggested that alternative logics of comparative history might together form a research cycle among a community of scholars. There is a sense in which the different strategies of causal inference discussed here—though all fall within only one of Skocpol and Somers's logics (i.e., macrocausal analysis)—can also be seen in light of a research cycle. For example, macrocausal analysis might be seen as following a cycle in which the respective strengths and weaknesses of nominal, ordinal, and narrative strategies shape the overall evolution of research. The three-way debate between Sewell (1996*b*, pp. 254–62), Skocpol (1994, pp. 326–34), and Katznelson (1997, pp. 98–99) can be seen in this light. Sewell calls for scholars to adopt primarily narrative-based arguments to avoid the pitfalls of more formal comparative methods, Skocpol in turn defends nominal comparison as the most powerful strategy, and Katznelson encourages scholars to employ ordinal comparison as an alternative approach. This debate reflects important divisions within the macrocausal tradition, and there can be little doubt that broader research among macrocausal analysts is influenced by the playing out of such debates and by the strategy (or combination of strategies) that happens to be in ascendancy. Yet, when viewed from a larger perspective, the Sewell-Skocpol-Katznelson controversy can be seen in more complimentary terms: by advocating different approaches, scholars serve to check and balance the characteristic weaknesses and limitations of one another. In this sense, debates about method among macrocausal analysts help prevent any single approach from gaining unwarranted dominance and may serve to place each method within a larger complimentary research cycle.

Of course, the ways in which macrocausal analysis may or may not follow a general research cycle cannot be fully understood until analysts become more explicit and self-conscious about how they use nominal comparison, ordinal comparison, and narrative analysis. The failure to be methodologically self-conscious has contributed to charges that macrocausal analysts lack a sophisticated approach to causal analysis (e.g., Lieberman 1991; Kiser and Hechter 1991). In addition, the absence of

methodological explicitness has made it difficult for many readers to fully understand and appreciate the arguments of macrocausal researchers. This article has attempted to provide a foundation for greater methodological reflection among macrocausal analysts. As an accompaniment to a continuing large output of high-quality research, greater methodological awareness may be an antidote against abandoning the unfinished but promising agenda of macrocausal analysis.

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Book Reviews

Creating Country Music: Fabricating Authenticity. By Richard A. Peterson. Chicago: University of Chicago Press, 1997. Pp. xiv+306. \$24.95.

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Richard Peterson's name has long been synonymous with studies addressing the production of popular culture. Over the past 25 years, Peterson has penned myriad articles that explore the complex interaction of popular music makers, the individuals and organizations that promote them, and the fans that consume and embrace their works. Within this tradition, *Creating Country Music: Fabricating Authenticity* arguably represents his best offering to date. This book provides an interesting social history of American country music. Readers begin their excursion in June of 1923, the year in which country music formally burst onto the recording scene, and they journey to the present where they share in Peterson's speculations regarding the role of country music in the 21st century.

Three specific issues drive Peterson's analysis of the country music industry. First, the author argues for a certain uniqueness to the institutionalization of country music, and he carefully traces for readers the distinctive ways in which the genre grew, solidified, and flourished. Peterson explores, for example, the prejudices of entertainment impresarios, noting that such industry players found themselves in the uncomfortable position of packaging and promoting a genre that was antithetical to their own aesthetic sensibilities. The author examines the ways in which these biases slowed country music's march toward commercialization. Peterson also charts the creation of the distinctive country music images and icons. We learn the story behind images such as the "old-timers," the "hillbilly," and the "cowboy," and we gain entrée to the plans of those involved in their construction—Henry Ford, George Hay, and Nat Levine, among others. Finally, Peterson explores the technological milieu surrounding country music's institutionalization. He charts the ways in which three new media—radio, records, and the growth of hard-surface roads—made possible the commercialization of the genre. Peterson crafts this story with very readable and inviting prose. Students of history and popular music buffs, as well as sociologists of culture and organizations, will feel equally at home in traveling this historical terrain.

As a second task, Peterson takes up the issue of authenticity. The author argues that authenticity represents the hallmark of country music. As such, he explores the meaning of the word and the politics inherent in attributing authenticity to a cultural product. In this all too brief excursion, Peterson notes that sincerity and credibility dominate the meaning of authenticity in the country music world, and he reviews some of the

self-conscious ways in which artists and industry figures construct these "natural" characteristics. This section represents the freshest dimension of Peterson's book. One cannot help but wish he had pursued this discussion in greater depth and detail. Peterson's treatment of the social construction of authenticity provides readers with some interesting categories for thought: authenticity work, signifiers of authenticity, the process of authenticity renewal. His discussion also engages (albeit much too hastily) an exciting literature addressing the mechanics by which value, in its various forms, is constructed. And Peterson's conclusions regarding the nature of authenticity in country music are provocative and important. He argues that authenticity does not refer to some clear standard or essential element from the past. Rather, authenticity is a reconstruction of various aspects of the past, elements continually selected and crafted to meet the needs of the present. Readers will see the exciting potential of these arguments, and many will wish that Peterson had unpacked them in a more elaborate way.

If the author's conceptualization of authenticity is correct, then one must view country music as a vital and dynamic form. It is on this note that the author concludes his work. Specifically, Peterson briefly speculates on the particular structural conditions (i.e., changes in the production system, generational cycles, changing audience tastes, and so on) that could maintain and foster country music's authenticity in the 21st century. In the author's words, "So long as country music remains the shared property of performers, the industry, and fans, the sense of authenticity will be preserved as it is changed" (p. 233).

Creating Country Music promises wide appeal. Researchers sympathetic to the neoinstitutional approach will find the book a fascinating case study of an American industry's development. Similarly, those dedicated to the process of social construction will appreciate the book's historical detail and the analytic frame used to trace the institutionalization of this genre. On a pedagogical note, the book can make a valuable contribution to graduate and undergraduate seminars addressing the sociology of art, the sociology of culture, or organizations and society. And beyond sociology, the book could easily find a home in courses addressing modern American culture or music of the 20th century.

The Sociology of Taste. By Jukka Gronow. London: Routledge, 1997. Pp. xiv+199. \$60.00 (cloth); \$18.99 (paper).

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This book runs counter to the general trajectory of most discussions of aesthetics, taste, and fashion now found in the sociology of culture and cultural studies. In an intellectual climate generally abhorrent of totaliz-

ing forms of analysis and theory, Jukka Gronow has produced a "treatise on taste" (p. ix).

Drawing on a variety of works, Gronow develops parallels "between the aesthetic of taste and gastronomy, on the one hand, and between the need discourse and modern nutrition science, on the other" (p. 2). Questions of need, surplus, and pleasure are common to these fields and discourses. That which is over and above "need" becomes surplus, and surplus easily translates into "luxury" and "hedonism" when discussed in pecuniary contexts. Whether the field is food consumption or economic consumption, however, Gronow sees pleasure and beauty as overriding tropes in a vocabulary of action.

The analogy between taste and gastronomy and needs and nutrition reveals a central problem in the sociology of taste—that is, the antinomy between the individual and the social or, to put it another way, the singular and the universal. Gronow shows how this antinomy is central to the project of Kant—who, along with Simmel, is his main philosophical forbearer. For Kant, "the feeling of beauty requires that it be shared universally, but how could something that was exclusively based on the subjective feeling of pleasure be universally valid, too?" (p. 11). The sociology of taste thus addresses the paradox of the coexistence of the sensuous experience of the individual and aesthetic judgments of the collective (e.g., what is beauty, what is art). "Good taste," he argues, has served as an "ideal means" for making social distinction precisely because it adjudicates between the personal and the social.

Gronow favors expressive value over the instrumental, particularly when it comes to deciphering human motivation (individual or collective). In this way, he is antirationalist as well as anti-rational choice. It is not surprising, therefore, that he does not deal with perspectives that invoke dominant class ideology as a determinant of taste, for that would be an instrumental explanation. Drawing selectively upon Bourdieu, Gronow rightly argues that not all status competition is hierarchically ordered class competition (pp. 22–30). Kitsch, for example, is a cheap mass-produced copy of something once considered elegant. In the 19th century, it gained social import from appearing to have adopted the status symbols of the elite, only to trivialize those symbols by turning them into articles of daily use (e.g., "a provincial coat-of-arms reproduced on the handle of a spoon" [p. 43]). Kitsch pokes fun at but does not try to imitate elite styles and values; it is a kind of a game. (The case of the Soviet Union is particularly interesting [pp. 49–70].)

The current fashion system (kitsch was intermediary between elite-class fashion of the 18th century and the mass fashion of the 20th) is analyzed by way of Simmel, Kant, and Blumer. It provides opportunity for both individual expression and shared taste. Fashion presents a kind of objectified commonality, a way to make judgments about ourselves vis-à-vis others and about others vis-à-vis ourselves. Making much of Blumer's "collective selection" thesis, Gronow all but claims that the

modern fashion system is an expression of zeitgeist (pp. 101–5), which registers collective good taste.

He concludes by attempting to integrate Simmel's and Kant's views on play and art with Simmel's formal sociology. "Because pure association . . . is necessarily reciprocal, it and only it can be called beautiful without any reservations. . . . With every other kind of social interaction—even if they are not 'pure'—an aesthetic element is associated, too" (p. 152). All social interaction thus has both ludic and aesthetic qualities.

This aspect of his sociology of taste resembles more an act of art appreciation than an invitation to disciplined interpretation. That is, Gronow is preoccupied with examining the object of observation and less mindful of the act of observation. Amid some insightful analyses and discussions of a number of contemporary and classic works on taste and consumption, Gronow draws attention away from his arguments in the act of presenting them. In short, his is a problem of voice.

His voice is often authoritative, offering no avenue to examine the basis of its authority. At times, it seems like he believes that truth is located somewhere in the integration of perspectives—taking something from Kant, a little from Simmel, a bit more from Blumer, he finds it legitimate to posit *the* fashion system. His voice is univocal. There is scant accounting for the place of race, ethnicity, gender, sexuality, and age—major sociological dimensions of identity—and thus little discussion of various forms of power beyond social class. These comments notwithstanding, Gronow urges us to take seriously the aesthetic and ludic dimensions of social life, both of which remain neglected in some of the most "cultural" of cultural studies.

Possessed by the Past: The Heritage Crusade and the Spoils of History.
By David Lowenthal. New York: The Free Press, 1996. Pp. xiii+338.
\$25.00

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Historian David Lowenthal, author of *The Past is a Foreign Country* (Cambridge University Press, 1985), continues in the realm he has made very much his own: the questioning of history and heritage and the analysis of appropriations and uses of the past. If, in his previous work, he has explored how in the past "they do things differently," this latest work is very present-minded. It is not a history per se, but historiography, a tightly argued, auspiciously organized, and elegantly written dissection of contemporary historical thinking. The accomplishment of the work lies in how it manages to replicate the diversity of its subject, moving from Cicero to Disney's *Pocohantas*, from TV talk shows to Chinese emperors, from Maori and Caribbean ethnic struggles to Elvis and the Elgin mar-

bles, with a strong sense of critical aplomb and a talent to make everything seem to belong.

The argument follows the book's title. It is very much about "possession," and the author proposes a formidable array of genealogies of the term by inquiring how control of the past is related to questions of authenticity, to blood lines, to patrimony (in the old sense of transference of property), legacies, and allegiances—personal, communal, ethnic, "national," and imaginary. "Possession" thus suggests both the frenzy of commemorative ritual that attends assertions of cultural identity, as well as the hand of the marketplace where the very real buying and selling of the past takes place—with all of its attendant pitfalls: commodification as vulgarization, representation as sanitization, educational value as measured by the box office. Lowenthal does a fine meditation on advocates of reconstructing the past the way it should have been, the problems of "authenticity," and no small accounting of politicians, publicists, entrepreneurs, and just plain folks who see that history is far too important to be left to "historians."

Subtitled the work "the heritage crusade and the spoils of history" is equally evocative, since the book is about crusading in its most vainglorious and righteous incarnations and the notion of "spoils" invites a critical look at both the foibles of "winners" who get to write history and the revenge of the dispossessed in claiming voice. The thesis is organized around a dialectical interplay between definitions, uses, and distinctions between "heritage" and "history." Basically, history is proposed as a critical discipline that strives, however forlornly, for judgments of the past that one might call (in Popper's terms) "falsifiable." Heritage is quite the opposite; it is a series of strategies for representing the past in a way so as not only to make it unverifiable but to make its truth or falsity beside the point. Heritage operates on the principle of "it could have happened," "it captures the spirit," or increasingly, "*based on a true story*," even if, especially in popular reconstructions, words are put in the mouths of those who never spoke them, unrelated figures are made to interact, and entire periods of history are collapsed into synecdochic models that simply "stand for" a given period.

Lowenthal shows how both history and heritage work in contemporary culture and ends by demonstrating, despite many distinctions, their general inseparability. He is master of the anecdote and brings his critical spirit and humor to such wonderfully overdetermined examples as a video game ("Play with history—in the ancient hills of Judea . . . pursued by a Philistine who is trying to kill you!"), which thoughtfully reassures potential players, "No prior knowledge of the Bible is required."

Critical readers will note that Lowenthal covers a laundry list of academic issues: the question of the end of history; the role of ethics and interpretation in writing; the problem of "objectivity"; the roles of race, class, and gender in social analysis; the "linguistic turn"; the meanings of narrative; the politics of representing the past. Presenting "heritage" in such thematic forums as "being innate" or "being first" allows the author

to investigate how hybridity, racial mixing, and status work in the Caribbean, the sort of research one would expect in an ethnological monograph, not this broad treatise. Woven into a rich fabric of examples and anecdotes, one encounters the lexicon of contemporary critical historiography: identity, imperialism, hybridity, discourse, questions of memory and nation building, postmodernity.

One wonders what Lowenthal would make of extending his critical skills in directions taken by other historiographers who have suggested the role of ethical categories to give historians some equilibrium in the storm-tossed present of history and its reconstructions. Lowenthal is observant of all sorts of moralities and their ambiguous uses in *Possessed*, so perhaps new paradigms are not his desiderata; to be possessed by the past is certainly something he knows, and we are fortunate to have his vision in the present.

Sinners, Lovers, and Heroes: An Essay on Memorializing in Three American Cultures. By Richard Morris. Albany: State University of New York Press, 1997. Pp. xxi+237. \$18.95.

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Richard Morris explores the process of memorializing the dead in 19th-century America by studying the rhetoric used by various groups in society to remember Abraham Lincoln. He supplements this analysis with a highly original and insightful scrutiny of words and images in American cemeteries of the time. His purpose is to disclose how the process of memorializing the dead was inscribed by a much more extensive cultural war between competing worldviews, each vying for the minds of men and women in the past. As such, his work contributes significantly to the growing scholarship on the construction of American collective memory over time and to the emerging understanding of how the technologies of culture—words, images, symbols—can affect the way we see the past, present, and future.

Morris feels that an actual event—Lincoln's death—created a condition for intensified cultural debate between powerful worldviews in mid-19th-century America. As many citizens looked to victory and the end of four years of hostility, the assassination of the president came as an immense shock, a tragedy that begged for explanation. The process of imagining Lincoln was extensive but dominated, in the author's view, by three powerful traditions or ways of seeing the world: religionist, romanticist, and heroist. Taking advantage of the cultural "chaos" created by Lincoln's death, all three viewpoints sought to "lay claim to the nation's public memory through their value-laden portrayals" (p. 42) of the president.

The three American cultures described here dominated the rhetorical

battle over how to remember Lincoln and all who died. The religionists, whom Morris felt "held sway over social life" and "public memory" (p. 45), attempted to persuade all who would hear them that experience was largely conditioned by the will and laws of God. Individuals mattered only in that they were instruments of God's will; Lincoln himself was only a gift from God that served the nation well. But power was ultimately divine. Consequently, their burial practices and grave markers were modest, minimizing the importance of the individual and of life on earth. Glory was to be found in the afterlife, not in this one.

The ethos of the romanticists moved away from heavenly goals and envisioned a harmonious community on earth. The basis of their moral view of life and of their veneration of Lincoln was to be found in their dream of a unity between all men and between man and nature. They celebrated Lincoln as one who drew wisdom from his early attachment to the land in Illinois. And they imagined a society where all men, despite class and race, would be seen as brothers. Thus, their gravescapes moved away from the minimalist words and headstones of the religionist toward the design of places from which the living could derive emotional satisfaction in the here and now. Often they built beautiful cemeteries with picturesque settings and objects of art.

The final contender in the public debate over memorializing were the heroists. This was essentially a tradition that celebrated the expression of individualism over the idea of submission to God or nature. Morris argues that, in their eyes, Lincoln earned his immortality because he was a "meritist." He pulled himself up from humble beginnings to a high position, heroically triumphed over adversity, and, consequently, stood as an inspiration to all citizens that the realization of their lives could be found on earth through their own efforts. Heroists tended to memorialize the dead with statues of soldiers who were celebrated for their individual action on battlefields or with massive tributes to powerful individuals like the Washington Monument, begun in 1848.

No doubt the cultural perspectives described here were important, but it is difficult to see how they could be studied without reference to the cultural (and political) position of American nationalism as they are in this book. Nationalism would come to dominate the way in which Americans framed the past over the course of much of their history. A substantial literature already suggests that this way of thinking collectively was gaining substantial ground before the Civil War. Any discourse over Lincoln could not have remained insulated from the views of the nationalists, which may have been expansive enough to imagine a past, present, and future that included religionists, romanticists, and heroists. Perhaps all the traditions discussed in this book were forced to confront the image of Lincoln because they could not flee from the growing cultural power of the nation, with its imagined links to the will of God, to the promise of social cohesion, and to the idea of opportunity for all individuals.

Rebellious Laughter: People's Humor in American Culture. By Joseph Boskin. Syracuse, N.Y.: Syracuse University Press, 1997. Pp. xii+245. \$39.95 (cloth); \$17.95 (paper).

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"Deciphering the relation between communal laughter and social vicissitudes that generates a particular comedic climate is tricky and rests on juxtaposing empirical data with historical inferences," Joseph Boskin states (p. 201). Yet, Boskin accomplishes just such an analysis. In 13 enlightening and entertaining chapters, Boskin justifies why Americans told and laughed at particular types of jokes during certain periods of time. In his introduction, he acknowledges that his thesis rests on an old argument, previously made by other authors such as Constance Rourke in *American Humor: A Study of National Character* (Harcourt Brace, 1931). Like many before him, Boskin asserts that all humor expresses rather than represses contemporary conflicts and anxieties. Therefore, when people want to stifle dissent, they will label certain types of jokes as sick or racist, as unworthy of being told or heard. Thus, political correctness spurs rebellious humor or laughter.

Boskin's original contribution lies in his ability to connect humor and history. He correlates the joke cycles that came and went across the United States from the 1940s until the 1990s with the social, political, and economic changes that occurred during those five decades. Boskin insists that what makes American humor unique is its ironic commentary on "The American Dream." For example, the one-liner, "Whoever dies with the most toys wins," was most popular during the materialistic 1980s. In fact, the rise of one-liner jokes ("Take my wife, please") mirrors the lifestyle changes of Americans as they moved from slow-paced rural towns to fast-paced metropolitan cities. Stand-up comedians and one-liners, rather than yarn-spinners and tall tales, serve humor to people too busy to sit still. Similarly, an explosion of jokes about race and gender told by women and men of all colors (including whites) coincided with Civil Rights and feminist movements in the 1960s. Jokes about success and failure, minorities and majorities, and cities and rural areas are American staples.

Next, Boskin examines the most popular American jokes in the last half of the 20th century: child jokes, giant jokes, sick jokes, lightbulb jokes, politically incorrect or correct jokes, and joke wars ("Whoever dies with the most toys, still dies.") All of these joke cycles challenged an American ideal. Dead baby jokes ridiculed the sanctity of the American family. Elephant jokes questioned superpower imperialism: "How do you stop an elephant from charging? Take away his credit card" (p. 66). Popular in the 1970s, lightbulb jokes expressed concerns about impotence in sexual relations (with their emphasis on screwing), bright ideas (which lightbulbs symbolize), the OPEC oil crisis (lightbulbs require energy), and

simple accomplishments (replacing a lightbulb). Politically correct or incorrect jokes abounded as gender and race relations changed. For example, "Why is a husband better than a cucumber? Because he can take out the garbage" (p. 169) and "What is the difference between a nigger and a black? A nigger is a black who has left the room." Inevitably, jokes that make fun of gender, race, or controversial issues offend some people who take gender, race, and other controversial subjects seriously. Equally inevitably, some offended people try to impose their seriousness with regard to these issues on the public at large. In short, social crises and changes inspire jokes that then prompt prohibitions against telling those jokes. During these prohibitions against jokes, critics say that Americans have lost their sense of humor. Boskin excels whenever he details the recurrent repression of controversial humor and humorists.

Rebellious Laughter is an incisive social history of American humor, but it is also flawed. Boskin demonstrates convincingly that temporal correlations existed between historical events and the popularity of certain jokes, but he never really clarifies the causal connections. Personally, as a thirty-something black male raised in a predominantly white, middle-class college town, I remember as a child sharing many of the jokes Boskin lists, especially the elephant jokes. Therefore, I wonder how telling and listening to elephant jokes was caused by my worries about American imperialism? Moreover, what does the popularity of situation comedies like *Seinfeld* say about 1990s America? Boskin gives us no independent means of answering these questions. His logic has a certain intuitive circularity. He argues that people's experiences shape their jokes then shows readers how people's jokes fit their experiences. What I want to know is How can we use one milieu to predict the other? Histories look backward, but they should also give readers clues as to how they can look forward.

Spectacular Nature: Corporate Culture and the Sea World Experience. By Susan Davis. Berkeley and Los Angeles: University of California Press, 1997. Pp. xii+313. \$50.00 (cloth); \$18.95 (paper).

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Susan Davis's new book, *Spectacular Nature*, is an important addition to the now well-established field of studies on theme parks. In minute ethnographic detail, she describes the many ways in which ocean life is produced and packaged for an audience of adults and children by the Californian marine park, Sea World.

The general intent of the book is to understand how the myriad of displays and performances, as well as the spatial layout constructed by the park, concur to create an intricate system of meanings representing ocean wildlife, nature, and animals. This system of representation is in

turn geared toward profit making and enhancing the public image of the Anheuser-Busch corporation, the owner of the park. This already indicates that Davis's study is at odds with previous approaches that viewed theme parks as festive cultural niches celebrating pleasure and semiotic inventiveness. Instead, Davis makes a convincing case for the importance of examining theme parks in general and Sea World in particular as corporate economic institutions interlocked with the structure and strategies of late capitalist market. In contrast to studies of popular culture that view mass-produced meanings in terms of the desires and pleasures they tap into and empower consumers with, Davis brings the study of commercial entertainment back to the firm ground of social and economic critique. Toward this end, the book follows two threads: one accounts for the commodification of public space; the other concerns the problem of the representation of nature and the environment in the mass media. Sea World, Davis claims, ties remarkably well the corporate and the natural and mobilizes a battery of sophisticated symbolic devices to blend one into the other. Thus when visitors enter Sea World, they are not simply spectators of some remote reality; they are immersed into an *experience* of nature, exoticism, and wilderness, all cast in the authoritative aura of science. While experiencing such meanings, the consumer is subtly manipulated so that she can be at once relaxed, entertained, educated, and transported to the exotic landscapes of wildlife.

Through a carefully crafted methodology, Davis shows that the multiplicity of meanings that are artfully orchestrated in the park are the result of well-thought-out marketing research and strategies. Through in-depth interviews with Sea World personnel, repeated visits to Sea World spanning from 1987 to 1995, and an analysis of photographs of the park, Davis shows that in Sea World, nothing is more carefully planned than surprises, spontaneity, and wilderness. In minute and fascinating detail she exposes the ways in which the "Sea World experience" is manufactured through marketing research, architectural engineering, careful scheduling of the shows, location of the restaurants, strict control over the personnel's performance, and the conversion of the visitor's gaze into a tourist's and even (Western) colonizer's gaze.

The analysis is not only "internal" but "external" as well, aiming to identify the reasons why Sea World resonates with American culture. The response provided by the book is many-stranded, but a few central themes can be readily identified: Davis undercuts Sea World's self-image by judiciously locating the park in the tradition of circus and animal training; Sea World taps into a long-standing Western cultural tradition of nature appreciation and animal displays, both of which are organically connected to the Western project of economic control and cultural rationality. Thus the packaging of nature and animals or the gaze and consumption still functions as a powerful metaphor for political, intellectual, and geographic control. Furthermore, by packaging itself as an "educational experience" for the family, Sea World resonates with middle-class ideals of education and learning.

One could argue that Davis's analysis is hampered by the absence of voices representing the millions of middle- and upper-middle-class white Americans who attend and (presumably) enjoy Sea World. She offers two convincing arguments to offset this criticism: (1) because the decisions made by Sea World staff are based on and fed by ongoing market research, the consumers' cultural habits, modes of perception, and desires are in fact "written in the text" of Sea World (2) the spatial, temporal, human, and animal design of the park directs and constrains so much of the consumers' experience that an analysis of those constraints yields an essential aspect of that experience.

As can be expected from an analysis that is predicated on the tasks of social criticism, the conclusions of the book are not optimistic. Sea World and its like point to a worrisome social reality. Public space is increasingly commodified as our contact with nature is increasingly mediated by corporate institutions. This money-based and close-to-home tourism alters our relation to nature and our ability to grasp the devastating effects of human action on the environment. What Davis finally unravels is the gentle but ever more powerful face of corporate strength at the century's end. By displaying a philanthropic concern for the environment, which takes the form of wildlife preservation, corporations, exemplified here by Anheuser-Busch, can have their cake and eat it; they can comfortably sustain social relations of exploitation and the economic system that gives rise to pollution and yet save their public image by claiming to be nature lovers. Thus the recycled image of nature and wildlife, so meticulously constructed by Sea World, ends up reaffirming the authority of science, corporate power, and economic exploitation.

This book offers a well-documented example of the ways in which entertainment, education, leisure, and such sacred meanings as nature and family interlock with the strategies of marketing research. To the student of culture, it offers a model of the ways in which ethnography can illuminate the complex interlocking of meaning and economy in the culture of late capitalist societies. But despite the clarity of the argument, the elegance of the writing, and the seriousness of the research, the book is disappointing in that it seems oblivious to much of recent research on culture, organizations, and capitalism.

As Davis is undoubtedly aware, a great deal has been written on the production of culture by corporate organizations, on the construction of scientific authority in the media, on commodification, and on the interlocking of culture and economy in late capitalism. Unfortunately, with the exception of a few evocative lines in the introductory chapter, no trace of these vibrant debates can be found in the book. Second, although I have a great deal of sympathy with Davis's return to a political and economic analysis of contemporary commercial culture, and although Davis takes great pains to present the Sea World text as an ideological compromise between consumers and producers, one is left with yet another reduction of culture to ideology and economic interests. In the end, Sea World is presented as nothing but a machine that lures us in and

prevents us from grasping our real environmental interests, a position difficult to sustain after so many studies have shown that "commodities" and "consumption" work at the interface between corporate interests and everyday life and have cunning ways of undercutting the cultural logic that produced them. Finally, Davis's analysis suggests that corporate representations of nature, such as that of Sea World, displace, replace, and ultimately corrupt a more direct relationship with nature, an idea that is as plausible as its opposite, namely that the more capitalism destroys nature, the more sacralized the image of nature becomes, thus paradoxically reinforcing rather than undermining our relation with it. Like many studies that undertake the difficult task of understanding the effects of capitalism, Davis's work is predicated on too simple an idea of what money, commodities, and rationality do to people and culture. As the works of Alan Silver, Paul Willis, Viviana Zelizer, Michael Schudson, Thomas Haskell, Nathan Sznaider, and others have suggested, such concepts as "market," "commodities," or "money" are sociological "black boxes" that need to be opened if we want to make sense of the eerie contradictions that make up the culture of late capitalism.

Trapped in the Net: The Unanticipated Consequences of Computerization.
By Gene I. Rochlin. Princeton, N.J.: Princeton University Press, 1997.
Pp. xvi+293. \$29.95

Rob Kling
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Gene Rochlin wants to alert the readers of his ambitious book to the significant, but often invisible, social risks of intensive computerization through networked systems. Aside from the introductory and concluding chapters, the book is divided into two major parts. In the first part (composed of three chapters), Rochlin briefly recounts the history of computerization in North America, with special attention to the Internet's precursors. He also examines how managerial ideologies, such as scientific management, have helped shape the computerization of work and have often reduced workers' discretion and skill. The book's second part (seven chapters) is a set of vividly drawn cases that examine the computerization of trading in financial markets and the computerization of battlefield command and control systems. These chapters form the empirical core of the book and the most detailed evidence on which Rochlin anchors his arguments.

Rochlin's analysis of the ways in which computerized trading has altered the structure of traders' work and of financial markets is circumstantial, but worrisome. Computer systems have been critical for creating new financial instruments, such as derivatives, that have been at the heart of several multihundred million dollar losses and bankrupted some local governments. They were also at the heart of the multibillion dollar

loss through which an individual trader in Singapore bankrupted Barings P.L.C., a 200-year-old British firm, in 1995. Rochlin argues that traders were subject to substantially more organizational control before computer systems became commonplace in trading. In these cases, the managers who were charged with financial oversight misunderstood the complexity of the derivatives.

In addition to trading complex "artificial" (or highly symbolic) goods, traders can work around the clock in a variety of world markets. The ways that the recent rapid declines of the Hong Kong and Indonesian currency markets in the fall of 1997 shook stock markets worldwide (and led to declines in the value of pension funds in the United States, for example) indicates that these markets are much more integrated than most analysts heretofore realized. Thus the book's subtitle, *The Unanticipated Consequences of Computerization*, is especially apt. The international currency and stock markets, however, are coupled in ways that go well beyond market-trading operations. For example, the stock of MNCs, wherever they are headquartered, can rise or fall with business prospects in their major markets, such as Asia. Unfortunately, Rochlin does not examine the ways that the business practices of MNCs links markets around the world, and he may be overemphasizing the importance of computerized trading on international financial instability.

Rochlin's most compelling analysis of electronic battlefields is based on a detailed account of the way that a cruiser with an unusually sophisticated command and control systems, the Aegis-equipped USS Vincennes, shot down a commercial Iranian airliner and killed the 290 people on board in 1988. Rochlin uses official reports to portray the cognitive framing of the ships officers, who were simultaneously in a fire fight with small boats, and the flow of clues about the airliner's movements, and their misinterpretations of its character and its pilots' intentions. He reports how Aegis miscued the ship's officers during a stressful episode. The airliner was ascending, but the lack of altitude information on the main screen allowed the operator to believe it was descending. The operator had earlier locked onto an Iranian F-14 fighter still on the runway, and believed the commercial flight to be that fighter. The official Navy reports treated the episode as an accident caused by human error. Rochlin uses this case and others to draw a more sociological conclusion, "that a highly automated, rapid-response battle system that depends for its function on real-time interactions between complex computerized systems and human operators may have an inherently high probability of error in any crisis situation that has not been anticipated, planned, and rehearsed" (p. 168).

Financial and airline mishaps, such as those discussed by Rochlin, have been taken very seriously by computer scientists who study "high-risk" systems and who develop normative practices from observations like his (see, e.g., Peter G. Neumann, *Computer-Related Risks* [Addison-Wesley, 1995]). Unfortunately, Rochlin does not examine this literature or assess the extent to which the design of newer high-risk systems are being influ-

enced by this kind of professional "best practice." These professional reforms may be organizationally inadequate or have little influence, but Rochlin ignores the debates and their importance for his overall arguments.

Sociologists can find this book to be a helpful introduction to the organizational character of some high-risk computer systems. The book offers a provocative series of cautionary tales. The underlying argument about the social risks of complex highly coupled sociotechnical systems builds on Charles Perrow's study of routinize catastrophes (*Normal Accidents; Living with High-Risk Technologies* [Basic Books, 1984]). Unfortunately, Perrow's analysis is merely mentioned in a footnote in a late chapter. Rochlin effectively identifies some of the problems that come as a by-product of managers and professionals enthusiastically embracing new "technological imperatives," but he does not theorize their computerization movements and the structure of the debates inside or outside these organizations (see, e.g., my own "Systems Safety, Normal Accidents, and Social Vulnerability" and Suzanne Iacono and Rob Kling, "Computerization Movements and Tales of Technological Utopianism" *Computerization and Controversy: Value Conflicts and Social Choices* 2d ed. [Academic Press, 1996]). Rochlin's book is an intriguing invitation for theoretically inclined sociologists to theorize seriously these kinds of events. Princeton University Press has posted the book's complete contents at on the Internet (<http://pup.princeton.edu/books>) for those who would like to preview it before purchase.

The Terror of the Machine: Technology, Work, Gender, and Ecology on the U.S.-Mexican Border. By Devon G. Peña. Austin: University of Texas Press, 1997. Pp. xi+460. \$45.00 (cloth); \$19.95 (paper).

Melissa W. Wright
University of Georgia

In his book, *The Terror of the Machine*, Devon Peña sets out to tell the human story of the *maquiladoras*. "The point," he writes, "is to understand the dynamic and intersecting contradictions in the evolution of capitalist domination and workers' struggles" (p. 28) without "(reducing) human beings to mere labor power." And so he promises to tell the story with some Marxian insights into capital without committing metanarrative and overdeterminist errors.

Peña correctly identifies the neglect of human stories in the much studied *maquiladora* industry. With only a few exceptions over the last two decades, accounts of human rights abuses along with industrial transformation within the *maquilas* have tended to cast the still largely female and poorly educated workforce as either agentless victims or as homogeneous units of production. Years of public attention and scores of academic texts have molded a profile of the "typical" *maquila* worker who

lacks individual style, ambitions, or otherwise strong opinions about the industrialization exploding around her.

Peña promises not to mimic such offenses. His interview-rich text details poignant illustrations of worker, largely female, views on the pressures they face in the *maquilas* and how they confront a dehumanizing environment. "I have emphasized the workers' own voices simply because the workers have direct and, in my opinion, more accurate knowledge of the factory" (p. 17). Yet as Peña progresses through an engaging argument, which begins with Henry Ford and ends with NAFTA, he unfortunately, and rather surprisingly, commits those very errors he criticizes. He recreates archetypal characters who do not change through time and denies the diversity integral to a vast labor force with varied personal histories.

First signs of a universalizing rendition of workers occurs early on when Peña declares, "I share the *maquila* workers' commitment to social change, workplace democracy, and economic justice. I share their concern for the earth and the rampant ecological destruction that capitalism produces . . ." (p. 17). Who are these earth-loving people? Are we really to believe that all *maquila* workers share a common vision for social justice?

Such universal depictions are inevitable consequences, I believe, of Peña's decision to make contemporary claims based on data more than a decade old. As I read, I kept wishing he had offered a historical account of the early 1980s. His fascinating description of the worker organization, The Center for the Orientation of Women Workers (COMO), which is central to his argument, reveals a vibrant and inspiring endeavor worthy of historical attention. Yet he insists upon making claims that his data simply do not support.

In its heyday, COMO defied all expectations by scholars, such as the renown New International Division of Labor crowd, who claimed that all third world women workers were damned to live in the dismal conditions of a docile proletariat. The women who formed COMO demonstrated that they were anything but docile. While many were not highly educated, they articulated their politics with unwavering clarity and startled a *maquila* elite who thought they had stumbled upon an inexhaustible mine of passive females. Why Peña does not develop the strengths of his research on COMO in the early 1980 period is mystifying and, in the end, crippling to his objective. COMO is now defunct as a political force in Ciudad Juárez, and since Peña insists upon linking this organization to contemporary events, his failure to explain the demise of this group is glaring.

Peña reveals his distance from the contemporary site of his research in other areas. Long before his 1997 publication date, a number of independent worker groups had made their presence felt along the border. In 1994, one of them, the Frente Auténtico de Trabajo (FAT), had almost succeeded in organizing an independent union (a union not affiliated with a political party) in a General Electric-owned facility located in Ciudad

Juárez. Last year, workers in the Han Young factory in Tijuana, with the assistance of the FAT and some U.S. groups, did elect the *maquiladora's* first independent union. Peña does not mention the FAT. One gets the feeling that he simply lost touch with current forms of resistance among *maquiladora* workers.

As Peña readily admits, he has lost track of some of the principle characters in his story. "What happened to Juana Ortega?" he asks in the beginning of a chapter entitled, "Mexico in the Fast Lane." He wonders if she is "somewhere in the United States." Last I heard, she was still in Ciudad Juárez and not too difficult to find. My point here is to stress that with some recent research, this and other questions or assertions Peña makes would have changed.

Maquiladora workers are women and men from many walks of life. Some are migrants, as most were in the early 1980s, but many are now the children of those workers Peña interviewed. They represent another generation of the *maquiladora* labor force, and their views, ambitions, and work experiences should not be collapsed with those of an earlier time. Peña's interviews and observations had the potential of supporting a fascinating and necessary historical account of worker responses in the incipient decades of *maquiladora* expansion, if only he had written that history.

After Lean Production: Evolving Employment Practices in the World Auto Industry. Edited by Thomas A. Kochan, Russell D. Lansbury, and John Paul MacDuffie. Ithaca, N.Y.: ILR-Cornell University Press, 1997. Pp. x+349. \$47.50 (cloth); \$19.95 (paper).

James Rinehart
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Does lean production (LP) provide, indeed require, challenging jobs performed by multiskilled workers in a high-trust, participatory environment? Or is LP driven by the goal of work intensification and staffed by workers whose jobs require minimal skill and whose participation is restricted to suggesting productivity improvements? To what extent is LP being diffused, and how are unions responding? These articles address such questions by examining employment practices of auto companies in 13 countries. The editors are associated with MIT's International Motor Vehicle Program (IMVP), which is funded by the auto industry.

IMVP maintains lean production (just-in-time practices, small buffers and lot sizes, and so on) gets best results when accompanied by "high involvement" work practices and human resource policies. In recent global surveys, MacDuffie and Pil give high scores on "involvement" to automakers that have teams, extensive job rotation, problem solving groups, suggestion programs, inspection by production workers, nontra-

ditional hiring criteria, much training, contingent compensation, and few status differentials. The data show convergence toward "high involvement" measures but with considerable variation.

Tight labor markets and worker discontent have led the undisputed champion of LP, Toyota in Japan, to relax some key aspects of lean production. Ishida questions whether LP requires a high-involvement environment. At Toyota, improvement activity (*kaizen*) is done mainly by managers, engineers, and team leaders. Job design is Taylorist, production is planned and managed from the top, and all employees experience "relentless" pressure.

Most Big Three plants in the United States, including NUMMI, are LP-mass production hybrids but with large differences across companies and plants owned by the same company. Fear of plant closure is an important determinant of union locals' agreement to elements of LP. Saturn is considered a post-lean company due to its extensive joint governance structures.

Big Three plants in Canada rank high on productivity and quality without many "high-involvement" practices. Their absence is due partly to union resistance (which Kumar and Holmes view as formidable). Nevertheless, companies have implemented just-in-time practices, eliminated subassemblies, outsourced jobs, and reduced non-value-added labor—all elements of lean production. These changes "invariably" increased the pace and intensity of work.

The German autoworkers union, which regards LP as a cost- and job-cutting system, agreed to elements of LP in exchange for employment security guarantees. Roth maintains the leanest plant in Germany, General Motors (GM) Opel, has the most narrowly defined jobs and allows workers the least discretion. Automakers in Great Britain have introduced LP employment practices. Fiat, Renault, and Seat have eliminated several layers of management and introduced teams, but worker discretion remains minimal due to control by first-line supervisors and systematic monitoring.

A Volvo plant has bucked the trend to standardized work by increasing job cycle times from two to between 10 and 28 minutes and allowing work groups considerable autonomy. At a Saab (GM-owned) factory, teams also have broad responsibilities, but they are expected to *reduce* cycle time.

In Australia, automakers have introduced some lean work practices, but teamwork is retarded by the union, which views it as manipulative with no autonomy. Most plants remain "neo-Fordist."

Wildcat strikes convinced automakers in South Africa to shelve some "Japanese-inspired" initiatives. The union has proposed changes beneficial to companies and workers (e.g., literacy and skills training), but automakers continue to rely on traditional production and management methods.

In Brazil, traditions of parent companies have led to different ap-

proaches to restructuring. GM's top-down strategy emphasizes production and uses NUMMI as a model. Mercedes-Benz negotiates change and stresses labor-management relations.

South Korean automakers must contend with the growing power of unions, strike waves, and high labor turnover. Assemblers and parts producers exhibit a wide range of practices but with little movement toward leanness.

The editors conclude there has been considerable diffusion of LP employment practices in auto plants in the 1990s. However, substantial variations arise from differences in company strategies, culture, government policies, and union strength. LP generally is impeded by powerful unions and spurred by competition, overcapacity, and declining profits. The book's title is misleading. Most automakers are far from having adopted the complete lean package, let alone having gone beyond it (whatever that might entail).

Some IMVP involvement criteria give no indication of involvement. For example, plants get high scores solely on the basis of having a large percentage of their workforce organized in teams. Teams in most auto plants, including the leanest, have only a fraction of the discretion enjoyed by workers in some Scandinavian factories. The same criticism applies to training and job rotation, which is mistakenly equated with multiskilling. IMVP does not measure workloads, work intensity, job complexity, and health and safety problems, especially repetitive strain injuries. "High-involvement" practices do not preclude a stressful working environment, nor do they ensure optimal efficiency.

This is an informative book that provides an excellent overview of developments in the global auto industry. The LP debate continues, as both proponents and critics will find in these articles support for their positions.

Why So Slow? The Advancement of Women. By Virginia Valian. Cambridge, Mass.: MIT Press, 1998. Pp. xvii+401. \$30.00.

Carol Nagy Jacklin
University of Southern California

Virginia Valian asks an important question. How can it be that most women and men truly believe that they should not and do not discriminate by sex, while progress in the employment and particularly in the promotion of women in the professions has been so slow. Worse, discriminatory promotion decisions are often made in the belief that the decision maker is doing his or her egalitarian best.

The answer, according to Valian, is that small discriminatory decisions accumulate. These small decisions are to be found in many areas but can be found most prominently in evaluation processes. The accumulation of

these very small differences cause large differences in the number of women and men found in prestigious positions in all areas of the workplace.

Good summaries of research literature are presented in many areas of gender schema. The summaries are made memorial by apt anecdotes, many of them from Valian's own experience. She also presents mathematical models demonstrating convincingly how a few small differences early in one's career can translate into big differences in professional outcomes.

Research is summarized for various relevant aspects of gender issues including gender schema socialization; biological explanations of gender differences, particularly biological explanations of cognition; gender schema theory and how it affects the workplace, the evaluation processes, and our sense of self.

The real strength of this book is not in its research summaries, good as they are, but in its new framing of old questions. Without preaching, Valian shows just how deeply ingrained sexism is by showing how we frame both research questions and the general understanding of our social world. We cannot understand how questions are framed without understanding the sexism that pervades them. For example, neither female nor male academics have asked how girl's superior conversational verbal skills get in the way of their math word problem test scores. Few female or male researchers have asked why male hormone cycles and consequences do not get into popular culture as have the simplistic view of women's hormonal cycles. Few female or male researchers have asked why men are not given an equal chance at the important human experience of parenthood. Valian shows how fruitful such new question formulation can be. Of particular virtuosity is Valian's analysis of the cost to a woman in the workplace of being perceived as feminine or of being perceived as masculine.

The last section of the book is devoted to suggesting how to speed the advancement of women. Valian believes that if individuals understand that biases in the evaluation process are an outcome of gender schemas, the individuals will change their evaluation behaviors. Several specific suggestions and the Johns Hopkins case study are given to show how this change can be instantiated. Although I agree that this remedy will somewhat speed the advancement of women, I am less optimistic that it will radically change the workplace.

I wish I had Valian's faith that when people understand their discriminatory behaviors they will change their ways. My cynicism is not with her analysis and suggested remedies. The accumulation of small discriminatory decisions in the workplace no doubt occurs. Her remedies no doubt redress that bias. However, I would add a larger dollop than Valian would of the variable "power" and the rewards to some men and some women of keeping power and privilege away from whomever does not have it. My cynicism has to do with just how many people of egalitarian goodwill there really are in the workplace.

Our different views may be generational. Valian suggests that women enter fields, such as psychology, where there are already many women. I am only a few months into emeritus status, but that advice would have been impossible for me to follow only a few decades ago, even in the field of psychology. I hope I am wrong and she is right in believing the number of egalitarian thinking women and men out there is very large. Even if my cynical view is correct, Valian's analysis and suggestions of how women can "take" power are very good and illustrate much about the nature of power.

Why So Slow will make an admirable text in social psychology, sociology, and gender studies classes with empirical orientations. It will make an excellent topics class text in sociology or psychology or an excellent optional text for introductory classes.

But I wish for this book a larger audience than the college classroom. Because of its solid research summaries, its formulation of the accumulation of advantage concept, and especially because of its taking old questions framed by the culture's pervasive sexism and reformulating them into new ways of viewing our world, *Why So Slow* should be read by all thinking women and men.

Under the Cover of Kindness: The Invention of Social Work. By Leslie Margolin. Charlottesville: University Press of Virginia, 1997. Pp. xiv+216. \$29.95.

Jerry Floersch
University of Chicago

Leslie Margolin reverses the idea that social workers help, a method borrowed from Michel Foucault, and marshals data from case records and social work texts to effectively and lucidly criticize the function and practices of 19th- and 20th-century social work. Professional social workers control the poor through their unique, ubiquitous, and largely unconscious discourse on helping and kindness. I find the book's strength in its exemplary reversal of the idea of a benevolent helper. I find its central weakness in its overreliance on a Foucauldian analysis.

Margolin presents social work's internal critique but fails to place his research alongside many important external criticisms: Roy Lubove (*Professional Altruist* [Harvard University Press, 1965] points out the contradictions between altruistic helping and professional bureaucracy; Joel Handler (*The Coercive Social Worker* [Rand McNally College Publishing, 1973]) shows that caseworkers misuse power; Michael Lipsky (*Street-Level Bureaucracy* [Russell Sage Foundation, 1980]) demonstrates that social workers leverage power over clients by manipulating social welfare regulations and work environments; Andrew Polsky (*The Rise of the Therapeutic State* [Princeton University Press, 1991]) argues that social workers define and control "juvenile delinquents"; Regina Kunzel (*Fallen*

Women, Problem Girls [Yale University Press, 1993]) looks at social workers who dominate unwed, pregnant mothers; Mary Odem (*Delinquent Daughters* [University of North Carolina Press, 1995]) shows how social workers manage female delinquency; Elizabeth Lunbeck (*The Psychiatric Persuasion* [Princeton University Press, 1994]) argues that they manipulate, among others, hypersexualized females; and finally, Linda Gordon's social workers (*Pitied but Not Entitled* [Free Press, 1994]) confine poor women to the home. These scholars, unlike Margolin, do not rely solely on Foucauldian analysis. Thus, in this review of *Under the Cover of Kindness* I evaluate the limits and potential of the Foucauldian analysis of social work.

Margolin examines the birth of social work (pt. 1), its "aggressive" phase (pt. 2), and recent "new excuses" (pt. 3) for its perpetuation. The first four chapters, "Basic Social Work," are especially good at illustrating a "social work gaze." Through close readings of texts and case records Margolin argues that social work coincides with the moral, political, and social need to investigate 19th-century urban poverty. Because investigation can be an onerous task, social work learned to use "sympathy and friendliness . . . to gain entry into private places" (p. 23). Home visits (surveillance) became "social work's totem technique, corresponding to the psychometric test of the psychologist or the physician's prescription" (p. 26). The use of "emotional support" to access information became a source of Orwellian doublethink: social workers consciously used friendliness to investigate unconventional and suspicious behavior while at the same time "forgetting" that such manipulation was itself suspicious. The invisible and secret were transformed into "facts" through biographical social histories recorded in case records. Moreover, the social work ruse is hidden from social workers by self-mystification (p. 60).

In part 2, Margolin argues that social work continued to "penetrate" the "hard-to-reach" even after its first, self-reflective, post-World War II phase. For example, the profession blamed the poor for societal failures and assisted psychiatrists in carrying out thousands of lobotomies, all in the name of helping. In the 1960s, social workers discover "empowerment" (pt. 3), a "brilliant strategy" (p. 120) to ensnare new clients. But social workers were never fully conscious of their discursive strategies. They became unwitting victims: "Whatever system of rules and obligations is operating here originates neither in the social worker nor in the client but in the discourse itself" (p. 134).

I find this book wrought with all the potential and limits of a strict Foucauldian reading of social work. I agree: horrific actions can be justified in the many languages of social workers, teachers, and scholars. And language is fraught with ambiguity and infinite meanings; yet, social life is dependent upon speech and writing. Are we to assume that all of the appalling acts of humanity, including those that take place under the watchful eye of social workers, are beyond our knowledge and exist merely in the relativizing discourses of professionals? If I know artifice, then I can reasonably identify the authentic. For Margolin, social work

is reduced to artifice alone. Moreover, had social workers been so clever at hiding their intentions, why were they not equally capable at disguising case records and texts? For Margolin to identify judiciously the artifice in social work discourse, he limits the analysis to social work texts (excluding reference to actual events), and he must read social work actions as a mirror of everyday speech acts. Margolin needs a method for discerning artifice from authenticity, but his Foucauldian analysis is limited by the lack of any epistemological criteria to evaluate knowledge claims. Margolin's central error is, then, the epistemic fallacy: reducing *what* we know to *how* we know it.

Discourse is reality and reality is discourse. If one follows this logic, Margolin's analysis is flawless. On the other hand, if there is more to knowledge than mere language events, this work may only be a starting point for researching the complex relationship between power and language in social work action.

Franchise Law Firms and the Transformation of Personal Legal Services. By Jerry Van Hoy. Westport, Conn.: Quorum Books, 1997. Pp. xii+156. \$59.95.

Jean E. Wallace
University of Calgary

Studies on the legal profession often focus on law firm practice, and many are restricted even further to large law firms. Consequently, the less prestigious and less powerful segments of the legal profession tend to be overlooked. Van Hoy offers an exception in *Franchise Law Firms and the Transformation of Personal Legal Services*, which provides an in-depth, qualitative analysis of the work experiences of lawyers in this relatively new segment of the legal profession. His analysis is based on in-office observations and interviews that he conducted over a year in the local branches of two national franchise law firms.

Van Hoy begins with an overview of the rise of franchise law firms in the personal legal services market. Franchise law firms successfully penetrated a market that was historically managed by sole and small-firm practitioners largely by exploiting both the cheap supply of legal labor and an untapped pool of middle-class consumers. Franchise firms offer a very limited menu of routine personal legal services that are mass marketed and mass produced.

Franchise law firms refer to chains of local law offices located in strip malls, shopping malls, or other retail business districts. A typical branch office includes a managing attorney, one or two staff attorneys, and a number of secretaries. Between each branch office and each firm's founding partner are several levels of management that may be organized along district, regional, and sometimes national lines.

Van Hoy provides a rich, informative description of the division of

labor in franchise law firms. Managing attorneys are responsible for the day-to-day operations of their branch office; they must ensure productivity and profitability. Staff attorneys are hired to consult with clients and sell services. Their work appears more focused around clerical and sales duties than researching and solving legal problems. Secretaries are instrumental to the success of the office and have the broadest range of tasks and responsibilities. They are acknowledged by all members of franchise firms as essential and we see that in many ways secretaries are more important to the functioning of these firms than are the staff attorneys.

Van Hoy paints a bleak picture of the work experiences of franchise firm's staff attorneys. Consultation with clients lasts 15 minutes or less, and clients are "herded through a production system that maximizes productivity and efficiency" (p. 2). Prepackaged law in the form of computer boilerplates is the key element in the mass production of routine legal services and for maintaining office productivity. The extreme standardization not only results in restrictive, repetitive, and boring work for franchise attorneys, but also significantly limits their autonomy, their influence on clients decisions, and the scope of legal skills they exercise in their daily work. Staff attorneys are poorly paid, work long hours, and have limited potential for promotion opportunities. "Staff attorneys are neither essential nor considered legal experts. They are extra help to facilitate selling services to clients" (p. 41). The standardized production technique grants significant legal decision-making responsibilities to secretaries who write letters, draft documents, and manage case files. The secretaries are more satisfied with their work, often appear to have more decision-making authority than attorneys, and their compensation is comparable to and sometimes more than the attorneys they work for.

Van Hoy's attention to this curious work situation is interesting and highlights his attentiveness to the complexities of franchise law firms and the workers involved, even workers who are not members of legal profession. He also examines lawyers in sole and small-firm practice who provide similar legal services but who do so in very different contexts and in a much more personal way. He conducted interviews with a sample of these lawyers so he could systematically compare their work settings and work experiences with franchise lawyers.

Van Hoy examines the alienating conditions of franchise law firms and discusses why efforts to unionize have been unsuccessful. He explains that lawyers take these jobs largely because of the overcrowded labor market. Inexperienced law school graduates turn to franchise firms as a last alternative, and most leave within two years. He closes by discussing the role of computer technology, the degradation of legal skills, and the future for lawyers providing personal legal services.

One of the major strengths of *Franchise Law Firms* is that Van Hoy's analysis is clearly and directly tied to the classical and contemporary literature on professional occupations in general and the legal profession in particular. He skillfully locates his work in the broader sociological literature thereby challenging us to rethink our traditional notions about law-

yers and their work experiences. *Franchise Law Firms* will be valuable to students of work, occupations, and professions and especially those interested in the legal profession or professional control. Van Hoy presents rich, vivid quotes throughout that detail the occupational context and work experiences of franchise lawyers. In doing so, he demonstrates the need for conducting in-depth field research in order to disentangle the complexity of factors related to occupational change and reorganization and the workplace implications such changes have for members of the occupations involved.

Party or Patient? Discursive Practices Relating to Coercion in Psychiatric and Legal Settings. By Stefan Sjöström. Smedjebacken: Boréa Bokförlag, 1997. Pp. 354.

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If the interface between traditional institutions is increasingly the site at which social categories are created, defined, and reproduced, then Stefan Sjöström's recently published study of the psychiatric and legal processes associated with compulsory care in Sweden is a paradigmatic study of power in contemporary society. *Party or Patient* will quickly find its way to the bookshelves and course reading lists in a number of disciplines. Sociologists will read it for its intelligent use of ethnomethodology, which enables Sjöström to build a fine-grained picture of ordinary language, its shifting contexts, and its relation to the minutiae of day-to-day practice. Medical anthropologists will read it for its discerning analysis of the way mental illness is defined in a hybrid psychiatric/judicial culture; lawyers, because it is an important study of a legal setting that has become psychologized. Mental health professionals will be sobered by its penetrating account of the way stock-in-trade, practical categories (such as "dangerousness," "insight," "boundaries") are actually produced and reproduced at ground level—sometimes substantiated empirically, sometimes not—by the staff as they go about their day-to-day clinical work. The social processes that Sjöström dissects so sedulously would be recognized by forensic psychiatrists throughout the English speaking world, which makes this a relevant and significant contribution to the international literature. At the same time, the book contains a wealth of material on Scandinavian mental health legislation, of more specific value to forensic psychiatrists working in that region.

One might picture an ideal-typical contrast between psychiatry (with its caring orientation, its open-ended diagnostic logic, and its tendency to include cases in categories rather than exclude them) and the law (with its adversarial orientation, its formal, rule-governed logic, and its criteria of relevance narrowly defined). Yet in the two ethnographic settings of this study, the Kronosund General Hospital Psychiatric Clinic and the

County Administrative Court, the distinctions are blurred. Sjöström's shows us a psychiatry that can become quite adversarial, at least from the patient's perspective, and a form of law in which attorneys do not always take an adversarial stance on behalf of their patients. Rules of evidence become elastic and categories are applied loosely. Psychiatric and legal personnel—chief psychiatrists, judges, public defenders, court appointed psychiatrists—are united by the shared orientations of a professional class, by universalizing language that can have the effect of decontextualizing the patient, by an easy-going companionship (the latter three groups of people lunch together between hearings), and by their tacitly agreed, commonsense notions of what it is to be mentally ill, irrational, potentially violent, or not responsible for one's own behavior. Though professionals all, these are people who speak about patients as "nuts" or "crazy." Formally speaking, the patient is a party (appellant or defendant) at such hearings; yet at an informal level, he or she tends to remain, in the eyes of these doctors and lawyers, a patient all along, and usually a crazy one, at that.

Sjöström's command over his technique, both as ethnographer and writer, brings the various actors in these clinical and legal settings to life, notably the patients themselves, and none more so than one doughty, resilient soul who the author has called Åke Sjögren. Åke reappears, in chapter upon chapter, as we follow his path through hospital admission unit, to the compulsory care unit, to his day in court. If the ethnographic description of these sites appears too detailed at times, then the author can be forgiven, because it is through such passages that he takes you into the inner workings—the conversations, the conferences, the talking, the writing—of these busy and powerful workplaces. And the extended transcripts provided in this book are invaluable in that they allow one to fully appreciate the deftness of the microresistance achieved by Åke in the arena of language. As he fights out his losing battle with one doctor then the next, he nevertheless manages to twist their questions around, confuse and outwit them, and at least win more than half of the skirmishes by means of his irony, his wordplay, and his gently teasing mockery, as in the following exchange, which took place on the 12th of March, between Dr. Boström and Åke: "Do you know what day it is?" "Well in my world it is the 12th of March" (p. 107). The reader develops an admiration for Åke as underdog, as I suspect the author did, but in the process the microanatomy of power is displayed in the full richness of which good ethnography is capable.

Sjöström's method of displaying the practical knowledge and discretionary power of institutions, while at the same time demonstrating the vicissitudes of an individual who traverses these institutions, is exemplary. Well crafted, theoretically penetrating, with a methodology that is made transparent, and a valuable discussion of the ethics of fieldwork, *Party or Patient* is a model ethnographic study of what is, perhaps, one of the most intrusive exercises of state power.

Disrupted Lives: How People Create Meaning in a Chaotic World. By Gay Becker. Berkeley and Los Angeles: University of California Press, 1997. Pp. xi+264. \$27.50.

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In many respects, *Disrupted Lives* represents medical anthropology at its best. It is firmly grounded in empirical research involving qualitative analysis of victims of various disruptions and disorders. At the same time, it addresses a carefully formulated theoretical problem, which concerns the relationship between the body, metaphor, and personal identity. Biographically, Gay Becker, whom we discover was herself the victim of childhood illness that included asthma, is closely woven into her own research topic. The volume represents an interesting and instructive dialogue between author and her informants, which, as a result, has an important ethical dimension. The notion of "disruption" leads us toward a reflexive uncovering of the frailty of our lives and the precarious character of the institutions that underpin them. This study is also marked by the characteristic shortcoming of cultural anthropology, namely a lack of attention to the political framework within which body and metaphor are connected.

The book is composed of five separate but interconnected studies. These are (1) an infertility study that examined how couples cope with the cultural disruption of childlessness and how men and women differ in their responses to the condition, (2) a midlife study of people between the ages of 35 and 65 years, which examined identity and disruption, (3) a stroke study, which examined how people's lives were radically changed by an abrupt, chronic, disabling illness, (4) a late-life transition study, which explored the transition from independence to dependence among people over 80 years, and finally (5) a study of ethnic minorities, which examined cultural responses to chronic illness in 240 people over 50 years. These five studies shared a common methodology because they were based on in-depth interviews, they were longitudinal, and the data were analyzed qualitatively by means of one process.

We can locate this study in the context of a profound and systematic criticism of the legacy of Descartes, specifically the mind/body dichotomy. Both sociology and anthropology have demonstrated that identity is fundamentally embodied. Both subjective and objective identity cannot be easily separated from embodiment. It follows that "self" is not an enduring or stable fact but changes with aging and the life-course. Hence, radical disruptions to self occur as a result of traumatic illness, which often breaks our relationship with significant others, reorganizes our life-world, and threatens to destroy the comfortable relationship between self, body, and others. In North America, where there is an important emphasis on youthfulness, activism, and independence, disruptions to everyday life from sickness and aging represent, as Talcott Parsons argued in the

sick role, a profound challenge to the sense of self identity. Because disruptions from sickness can transform the body-image, others have significant problems of routine interaction in responding to representational ambiguities and disfigurement.

Becker argues that metaphors of illness play an important part in helping people make sense of these unwanted discontinuities. Metaphors help us to understand, but they also have therapeutic qualities. These narratives of disruption are moral accounts of people's lives. Metaphors are the vehicles that express the values that make life meaningful and coherent. Narratives of healing are part of the process of healing. Given the importance of activism and individualism in American culture, healing narratives are typically structured around themes of disruption and the assumption of personal responsibility. One obvious weakness of this study, however, is the neglect of power relationships in the negotiation of these narratives. There is only one passage in the book where Becker briefly enters into a discussion of how the interaction between therapist and patient involves power (p. 176), but in general terms, the hierarchical structure of power in relation to medical knowledge is fundamental to such questions as: What metaphors are available to patients and how are they legitimized? Are there deviant narratives of illness that subvert medical power? In short, meanings and metaphors are negotiated in medical settings, where resources are unequal. These issues have been constitutive of contemporary medical sociology, where, in the last decade, the work of Michel Foucault has been influential.

This absence fortunately does not vitiate the quality of this study of illness narratives and the self. In fact, there is a submerged theme in her study that requires special attention and suggests a radical alternative to the conventional perspective on the body/self couple. The stability of everyday life requires the presumption of a continuous and reliable self, and hence we assume that disruptions are exceptional interventions within this normality. Toward the end of her study, Becker came to the conclusion that "continuity is an illusion. Disruption to life is a constant human experience" (p. 190). Only the body itself has some continuity, but it is also vulnerable. Hence, the everyday world involves a constant struggle to sustain the illusions of order and continuity against a backdrop of persistent and ineluctable disorder. Metaphors, which mediate between the self and chaos, provide the building blocks of cultural meaning.

Real Heat: Gender and Race in the Urban Fire Service. By Carol Chetkovich. New Brunswick, N.J.: Rutgers University Press, 1997. Pp. xiv+241. \$50.00 (cloth); \$18.95 (paper).

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It is the case that even committed feminists who seek equality in all jobs historically labeled as male draw the line at women firefighters. The image of a burning building and a fireman carrying a victim down a ladder to safety just does not permit appreciation of a woman doing that job. Thus it is not surprising that resistance to women's entry into the occupation has been strong, although a substantial number of women have joined the firefighting corps in many urban communities and have proven themselves able and enthusiastic members having gained entry through litigation and breaking down barriers against them.

This vivid and engaging account of the entry of women and minority firefighters into a formerly white male bastion tells us not only something of the nature of prejudice and the mechanisms that enforce isolation of "undesirable" members but also about the culture of the firehouse and the socialization of new recruits. Based on fieldwork in the Oakland Fire Department during the period of academy training and 18-month probation period, this is a sensitive and multidimensioned analysis and description of an occupational community. Furthermore, it shows how much new recruits rely on informal structure to learn the ropes of their trade and how dependent they are on the goodwill and cooperation of old-timers on the job. Not merely a matter of good performance in the formal training that precedes work on the job, competence is as dependent on "belonging" as it is on the acquisition of skills. Similar to situations in professional life (such as the law, which I have studied), women find that coworkers and officers can facilitate or inhibit opportunities to prove oneself by the possibility to be in or out of the action at fire scenes. The lack of opportunities to prove oneself—whether the result of being in a slow station or being purposely kept out of the action—means a delay in acceptance by coworkers.

Furthermore, to those scholars who suggest that individuals' sense of confidence or lack of confidence is a function of their upbringing (or race or gender) alone, Chetkovich shows how situationally determined confidence can be. Both women and members of minorities in firehouses either gain opportunities to demonstrate skills and thus build confidence, or they find that a lack of opportunity to perform well leads to self-doubts.

It is no easy matter for anyone to join the firefighting ranks. New recruits of majority groups go through hazing much like military academies and fraternities—often cruel and painful and always a "testing" procedure to determine how much a person can "take it." Unrelated to the work, practical jokes and casting the newcomer into a position of servitude create a climate of uncertainty to any rookie, but they bear down

especially harshly to white and minority women and minority men to whom many of the practices can only be interpreted as racist and sexist. Trying to balance their responses between good humor and the maintenance of dignity means balancing a tightrope requiring strong interpersonal skills and at least a little help from sympathetic insiders.

Chetkovitch's work is rich in illustration of the rituals and rites of passage all firefighters go through. Charting the social practices, she then shows the differences between the ability of women and minority men to fit into hostile environments and the ways in which the majority themselves learn to cope with "aliens" in their midst or to continue to reject them.

Chetkovitch's analysis will be helpful to scholars of occupations and professions and to policy makers, to say nothing of firefighting administrators who may well reflect on how their "culture" impedes or facilitates the integration of its nontraditional members. It is an important contribution also to the debates on affirmative action, showing how real inclusion is necessary for creating competence in the new recruits and that programs that do not seem successful may have much less to do with the excellence of the outsiders who are brought inside and more to do with the ways in which they are permitted to learn the tools of their trade.

Global Diasporas: An Introduction. By Robin Cohen. Seattle: University of Washington Press. Pp. xii+228. \$50.00 (cloth); \$19.95 (paper).

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The strength of Cohen's book lies in his attempts to give specific meaning to the term "Diaspora." He does so by constructing a typology that pivots the definition within the historical experiences of specific ethnic groups. The main weakness of this book lies precisely in this attempt to correlate each diasporic category with a specific ethnic group. Throughout, there is a tendency to neatly reduce a wide range of experiences to these ascribed categories. Given the intense and vibrant debates on the hybridity and malleability of identities, the endeavor to concretize this very fluid concept is problematic.

Cohen proposes that the typology of diasporic communities as victim, imperial, labor, trade and cultural, respectively, correlates with Africans and Armenians, Britons, Indians, Chinese and Lebanese, and Caribbeans. Jews are analyzed in terms of representing the general diasporic condition, and Cohen provides an eloquent and informative narrative undermining the assumption that the Jewish experience and the term Diaspora are synonymous, or that their experiences determine the meaning of the term. He stipulates six conditions that are necessary for applying the term Diaspora to any community. These include having a

single traumatic event occur that coincides with dispersal, the creation of imagined homelands, voluntary and involuntary movement of people, collective memories, and common identity with coethnic members in other countries.

Cohen's analysis of other diasporic groups are enlightening but not always convincing. For example, according to his typology, Africans and Armenians are "victim" diasporas and hence have abiding homeland myths. The country, symbol, and idea of Ethiopia provides, according to Cohen, a powerful source of collective memory and imaginings for Africans in the Caribbean, Mexico, and Brazil. The pogroms of 1915–16, when over one million were killed by the Turks, serves as the most abiding common memory of community and identity. What is the purpose of comparing these two groups? That as "victims," they construct identities and create diasporas in specific ways. Cohen concludes that both groups exhibit similarities, in that they show solidarity with their homelands, have a strong ethnic group consciousness, experience a troubled relationship with their host countries, and have a sense of empathy with other coethnic members. This being true of many "diasporic" communities, how exactly does Cohen's typology enhance our understanding of the term itself and diasporas in general?

Analyses about diaspora tend to be awkwardly situated between a broadly defined macrolevel analysis and an intricate microlevel study. Broad trends and overarching features oftentimes underplay detailed ethnographic data. What then are the strengths of macrolevel diasporic studies? Broadly defined group identities are recognized and highlighted. A partial explanation for in-groupness, coherence, and resilience of such groups is provided. Relevance is given to overarching historical narratives associated with shared ancestry and with common social and political experiences. These studies also alert us to the possibilities of evaluating whether specific diasporic groups are capable of becoming active in political moves for secession, federalism, repatriation, and critical support for partisan homeland politics. Diasporic studies channel us toward clearer definitions, especially when the categories of ethnic and religious groups remain amorphous and imprecise. These studies enable analytical distinctions between diasporas and other ethnic groups, be they indigenous, immigrants, political refugees, migrants, or others.

Cohen has tried to straddle the delicate divide between a generalized analysis and intricate empirical studies. The chapters in this book are analytically uneven with the first chapter discussing the Jewish diaspora being the strongest. The rest of the book displays moments of eloquence and insight, punctuated by glib statements that sometimes trivialize the narrative. To his credit, Cohen includes postmodern approaches, in his effort to illustrate the "cultural" diasporic disposition of Caribbean peoples. Although he successfully focuses on some of the more compelling elements of these approaches, particularly those concepts about the constructed character of identities, Cohen only uses them to analyze one ty-

pology and people. This has in part contributed toward a more rigid, less fluid and less nuanced analysis of other diasporic communities. If he had, for example, chosen to analyze the Chinese, Indian, African, and Armenian communities from the perspective of his typologies, and also as fluid, malleable, hybrid identities on the frontlines of difference, he would have written a more compelling account. Cohen's desire to provide a series of categories for more precise definitions undermines elements of difference within these categories. By taking on board the notion of identities as constructed and hybrid, Cohen would have been able to reflect more accurately the everyday struggles of diasporic communities living in the borderlands between "homeland" and home.

Different histories, experiences, and differences in time and space make the task of comparing diasporas exceedingly difficult. Cohen has managed to accomplish this with a great deal of dignity, contributing to an ongoing and vibrant debate on identity and diasporas.

The Racial Contract. By Charles W. Mills. Ithaca, N.Y.: Cornell University Press, 1997. Pp. xii+171. \$19.95.

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The Racial Contract is a work of political philosophy that offers a bold conceptualization of the racial order and a critique of the way it has been (mis)represented within the domain of scholarship. How does a society founded on principles of democracy and equality countenance slavery and Jim Crow? And how do scholars who provide the intellectual framework for liberal democracy reconcile their ideational system with the reality of a racism so embedded in the nation's political structures that it vitiates all claims to "democracy?" Many tomes have been and continue to be written that seek to reconcile the irreconcilable. But the elemental truths can be stated concisely, and, in this slim, well-crafted volume, Mills cuts through the shibboleths and the mystifications that pervade both popular and academic discourse on race.

Mills's argument is that alongside the social contract that is embedded in the Constitution and that is the framework for liberal democracy is "a racial contract" that excludes whole groups from the rights of citizenship and relegates them to a twilight zone defined by violence, exploitation, debasing poverty, and, in the personal sphere, denial of one's personhood. In a rebuke to the Myrdalian orthodoxy still cherished by sociologists, Mills writes: "There is obviously all the difference in the world between saying the system is basically sound despite some unfortunate racist deviations, and saying that the polity is racially structured, the state white-supremacist, and races themselves significant existents that an adequate political ontology needs to accommodate" (pp. 123-24). For Mills, racism is not merely an ignominious exception to the social con-

tract, but like overseas colonization, it yielded important material benefits to whites and even provided the basis, ironically, for their vaunted freedoms.

The Racial Contract offers a theoretical framework that ought to serve as the starting point for any serious study of race in American society. Mills criticizes political philosophers for their glaring failure to include the inconvenient facts of white supremacy and racial subjugation in their theoretical disquisitions. In sociology, of course, there has been no dearth of studies of race and racism. However, Mills has a message that is equally searing. The thrust of scholarship has been "to evade and to elide and to skim over," thereby providing moral justification for racial oppression or denying its existence altogether. Indeed, according to Mills, there has been "an inverted epistemology, an epistemology of ignorance . . . producing the ironic outcome that whites will in general be unable to understand the world they themselves have made" (p. 18).

Mills's critique of race scholarship pertains not just to those who provide intellectual sustenance for the racial contract, but even to liberals and Marxists who elide race within their universalistic scheme of values. As he writes, "The preoccupation of nonwhite moral and political thought with issues of *race*, puzzling alike to a white liberalism predicated on colorless atomic individuals and a white Marxism predicated on colorless classes in struggle, thus becomes readily explicable once the reality of the racial contract has been conceded" (pp. 110–11). Implicitly, Mills offers a rejoinder to recent writers who contend that it is time to get "beyond race," who envision "a postethnic society," or who dismiss racial mobilization as a gratuitous expression of "identity politics."

Mills also offers a counterargument to those who think that the passage of landmark civil rights legislation amounts to an abrogation of the racial contract. He scoffs at "the pretence that formal, juridical equality is sufficient to remedy inequities created on a foundation of several hundred years of racial privilege" (p. 73). I wish that Mills had done more to expose and dissect the new incarnations of the racial contract. Nevertheless, at a time when "the epistemology of ignorance" is ascendent, we can be grateful for a book that speaks the unpalatable truth.

Angels' Town: Chero Ways, Gang Life, and Rhetorics of the Everyday.
By Ralph Cintron. Boston: Beacon Press, 1997. Pp. xiii+264. \$26.00.

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Angels' Town is an ethnographic study that explores the poetics of everyday life as experienced by urban Mexican Americans. Author Ralph Cintron's aim is not so much to describe actions and institutions as to capture the ethos of the fieldwork site. This mission involves a detailed examination of the rhetorics, symbolisms, and performatives that constitute the

public life of the community. For this reason, the text blends the concerns of critical ethnography with those of contemporary cultural studies and rhetorical theory.

Angels' Town also bears the imprint of the poststructural critique of the traditional ethnographic enterprise. There is an abundance of self-reflexivity, with Cintron questioning his own *latinidad*, researcher status, and the writing process. Such themes are, of course, common in postmodern ethnography, and often they lead to a concern with texts and autobiography rather than the field site itself. A strength of *Angels' Town* is its ability to discuss these issues with sensitivity and yet avoid the retreat into a defeatist, solipsistic relativism.

These same poststructural influences make summarizing the substantive content of the book rather difficult. Cintron, perhaps rightly, is hesitant about making sweeping claims for his research findings. Instead, each chapter has its own concerns and uncovers ethos by teeing off from specific research sites. The result is a text characterized by a series of more or less discrete points of tentative illumination. The first, and most important of these, is Don Angel, an older man who works as a gardener and janitor. Cintron's discussion of Don Angel centers on his facility with *chero* folkways. He is a *curandero* and is skilled in *viejito* discourses such as those associated with divination and the bawdy wordplay of *albures*. According to Cintron, Don Angel uses this traditional knowledge as a way of momentarily affirming his dignity and status despite his objective social location: "In finding talented parts of himself in these actions, then, he found moments that allowed him to individualize himself within conditions of apparent hegemonic oppression, or to create respect under conditions of little or no respect" (p. 92).

This search for respect and identity is a leitmotif that Cintron uses in other chapters to explain phenomena as diverse as the posters on a boy's bedroom wall, gangs with their graffiti and codes of violence, low-rider cars called "thumpers" and "Too Low Flow," and the blacktopping of a parking space by an ordinary family. A second theme running throughout the text is the search for order and its role in the intersection of life-world and system-world. Here Cintron explores the collision of the rhetorics of town plans, visas, and social security numbers with the everyday orderings of Mexican Americans. The central aim here is to demonstrate the adaptive and creative qualities of the textualities (e.g., forged documents, graffiti) through which Mexican Americans negotiate and order both public spaces and the labyrinthine world of bureaucracy with its "discourses of measurement."

All things considered, *Angels' Town* is a significant and beautifully written contribution to the fast-growing field of Mexican American ethnography. It succeeds admirably in the difficult task of documenting the voices of ordinary people and at the same time contributing generalizable knowledge to its field. As with any book, there are some things that might have been done differently—and perhaps better. At times, Cintron draws a long bow in moving from observable behavior to speculation on the

motivations and interior lives of his subjects. The result are occasional moments of interpretation that are perhaps too deeply rooted in humanist psychology and that sit rather uncomfortably with the poststructural theories of texts and discourse deployed elsewhere in the book.

A second set of concerns surrounds the treatment of gender issues. Cintron acknowledges that his informants were predominantly male. Yet masculinity hardly figures in explanations of their behaviors and beliefs. Are automobiles, graffiti, militaristic imagery, gangs, guns, and sexually explicit joking behavior most fruitfully viewed as attempts to generate a sense of control and respect or as specifically *gendered* ways to accomplish this aim? A substantial and well-known literature on masculinity supports this alternate position. A final issue is ethical. *Angels' Town* appears to be littered with clues that could be used to identify the research site and, possibly, the persons involved in the study. Finding a way of providing contextual information for the reader while protecting informant confidentiality is a difficult task. If too little detail is provided, the text will be characterized by abstraction, sterility, and hermeneutic inadequacy. Yet if too much information is given away, the research site will be compromised. Red herrings offer a colorful solution but can lead to ersatz ethnography that lacks conviction and intellectual integrity. It is not clear that *Angels' Town* has located a happy equilibrium between these contending evils.

A Generation at Risk: Growing Up in an Era of Family Upheaval. By Paul A. Amato and Alan Booth. Cambridge, Mass.: Harvard University Press, 1997. Pp. x+319. \$35.00.

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What are the long-term effects on children of the great changes in the family that have occurred over the past several decades? We are just beginning to assemble the answers to this question. Most social scientists think in terms of shorter time horizons, in large part because that is the way funding agencies and promotion committees think. In addition, the changes that occurred in the 1960s and 1970s (and have in some cases continued through the 1980s and 1990s) have only now gone on long enough so that we can study the first generation of adults who experienced the transformed family as children. Paul Amato and Alan Booth's impressive study is one of the first to provide us with long-term data on this generation.

It is not adequate to merely question today's twenty- and thirty-somethings about their families of origin and then to correlate what they say about their childhoods with how they are doing as adults. For one thing, it is hard to know how much their recollections of the past are colored by their current state of mind. In addition, there are some aspects of their

childhood family lives that they cannot be expected to report accurately on. What is needed, therefore, are long-term, longitudinal studies that provide detailed baseline information on families and then follow their children for at least a decade. This is the kind of study Amato and Booth present, and their finding are enlightening.

Their data come from a study that began as telephone interviews with a nationally representative sample of married couples in 1980. Crucially, the investigators asked their respondents not only standard sociodemographic questions but also multiple questions on marital quality, including marital happiness, marital interaction (e.g., "How often do you eat your main meal together?"), marital conflict (e.g., "How many serious quarrels have you had in the past two months?"), and divorce proneness (e.g., "Has the thought of getting a divorce or separation crossed your mind in the last three years?"). The parents were interviewed again in 1983, 1988, and 1992. And in 1992, the researchers interviewed all of the children who had lived with their parents in 1980 and who were now age 19 or older. The offspring were contacted again in 1995. This book examines the lives of the adult children, age 19 and over (median age of 23), in the context of their parents' marriages in 1980.

The authors find that marital quality in 1980 is broadly associated with offspring's characteristics in 1992. In fact, its associations are stronger and more consistent than are the associations with parents' gender role nontraditionalism (which generally has modest effects) or even parental socioeconomic characteristics. "Marital quality is the key variable," the authors write, "for it is bound up with virtually every dimension of offspring well-being" (p. 221).

Of particular interest is the authors' analysis of the long-term effects of parental divorce. Theirs is one of the few longitudinal surveys to measure marital quality and then to follow offspring for a long period, during which some of the parents divorce. Therefore, they can address the issue of whether it is marital distress or divorce itself that disadvantages some offspring. Their answer is that both appear to have independent, long-term effects on children's lives.

They go further, however, by comparing divorces that occurred in marriages that had low conflict in 1980 to divorces that occurred in high-conflict marriages. They report that offspring who experienced high marital conflict in 1980 were doing *better* in 1992 if their parents had divorced after 1980 than if they had stayed together; conversely, offspring from low-conflict families were doing worse if their parents had divorced. This finding confirms the oft-stated but rarely substantiated belief that if family conflict is severe, children may benefit from a divorce. Does this mean that we should not be concerned about the rise in divorce? No, say the authors. They note that only a minority of the divorces that occurred were in high-conflict marriages. The majority occurred in marriages where children would not be expected to benefit from the split.

The seeming effects of marital quality are so pervasive that one wonders if the authors' measures reflect more than just how well the spouses

are getting along. For example, parents and children in some families might both be susceptible to major depression for partly genetic reasons; if so, one would expect this shared trait to contribute both to lowering the quality of the parents' marriages and reducing the quality of their children's adult lives. The safest conclusion to draw from this book is that marital quality and unmeasured characteristics of the parents and children associated with marital quality are strong indicators of how well the children's lives will turn out.

There are some limitations to the study. Since only married couples were selected in 1980, the survey provides little information about the consequences of growing up in a single-parent family. The 1992 sample of offspring is 94% white, a consequence of the white, middle-class bias often encountered in telephone surveys (and worsened by differential attrition by race between 1980 and 1992). Overall, though, *A Generation at Risk* is an important addition to the literature on the long-term effects of families on their children.

From Cashbox to Classroom: The Struggle for Fiscal Reform and Educational Change in New Jersey. By William A. Firestone, Margaret E. Goertz, and Gary Natriello. New York: Teachers College Press, 1997. Pp. ix+182. \$24.95.

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This book is small but tackles a big job: the possibilities and limitations of school finance reform. As the authors say, the distribution of resources to schools is the most contentious and thorny issue facing educators and policy makers since desegregation. Do schools for children in financially poor districts deserve the same (or more) support as those in rich districts? Even if the answer is yes, is it possible to appropriate and use financial resources to redress present-day inequities?

The authors of this book, well-qualified as they are to consider these questions, chose to pursue a mixture of qualitative and quantitative strategies. They compiled detailed longitudinal case histories for 12 (out of 500) school districts in New Jersey and also analyzed a statewide survey. They first examined inequities in community, fiscal, and administrative contexts in which the New Jersey schools functioned. They then tried to assess whether the Quality Education Act (QEA), a statute designed to bring educational services into parity across districts, actually did achieve that goal and by which means that goal was approached. Put somewhat differently, the main issue around which the book is organized is whether the QEA could counteract gross inequities in the educational opportunities offered students in New Jersey.

They found that urban districts did improve the services they provided poor students after the QEA was enacted, but some districts progressed

more rapidly toward parity than others. Some districts took better advantage of the new funding than others did because of enrollment changes, but local political constraints and community cohesion played a role too. Even among districts that made the most headway, a large gap remained between rich and poor districts, however.

Analysis of fiscal changes revealed that poor urban districts did use most of their new money directly for education. Despite roughly comparable teacher-student and administrator-student ratios in rich and poor districts, professionals in wealthy districts continued to earn more than their colleagues with equivalent education and experience in poor districts. In addition, teachers in poor urban districts generally had less experience and less formal education than those in wealthy districts.

A separate issue is that "special needs" districts—those generally the very poorest—used some of the additional resources provided them by QEA to deal with growing social problems in their school populations. They offered their clients full-day kindergarten, extended-day programs, and school breakfast programs, as well as more counselors and social services. Some even added preschool programs, health services, and programs for parents. The distressed physical conditions of schools in the special needs districts, which had developed over a long period of time, could not be corrected by the local tax relief made possible by the QEA. The QEA funding allowed districts to undertake badly needed capital improvements, but the gap between rich and poor districts in school facilities remained huge.

The amount and character of the data assembled in this small book are impressive, as is the authors' courage in tackling almost intractable issues such as whether appropriated funds actually go where they are supposed to go. What comes through clearly is that more money can help poor districts provide better services, but parity across schools remains an elusive (unattainable) goal. A point not so explicitly made is that the "services" furnished by many schools go far beyond student instruction and even serve an expanded recipient pool (preschoolers and parents). The tasks that some poor districts take on are overwhelming. They are feeding as well as instructing students and helping parents take care of preschoolers as well as school-age youngsters.

As this book attests, effects of school finance reform are not easy to assess. The methods used here, however, go far toward giving a fuller picture, for example, by noting community differences in the implementation of reforms or by cataloging the many ways that poorer schools must stretch their funds. All in all, these authors have written clearly, carefully, and understandably about vexing problems that concern educators and laypersons alike.

Community Service and Higher Learning: Explorations of the Caring Self. By Robert A. Rhoads. Albany: State University of New York Press, 1997. Pp. vii+254. \$19.95.

Joseph Kahne
University of Illinois, Chicago

In *Community Service and Higher Learning: Explorations of the Caring Self*, Richard Rhoads conceptualizes and illustrates ways college students' community service experiences may help develop students' capacity for caring and civic participation. Specifically, he believes "understanding the social processes associated with community service can shed light on how higher learning might be restructured as we struggle to build democratic communities within the tensions and strains of a postmodern world" (p. 2).

Rhoads' first three chapters provide an extensive and thoughtful synthesis of social and educational theory. Drawing on George Herbert Mead, Carol Gilligan, Nel Noddings, Erik Erikson, Paulo Freire, John Dewey, Henry Giroux, and others, Rhoads considers how and when community service experiences interact with college students' "evolving sense of self" to promote a more caring, just, respectful, and democratic society. Given the underconceptualized nature of much writing on community service, this synthesis struck me as the book's most substantial contribution.

Having set the stage, chapters 4–6 report data from Rhoads interviews and observations of students who worked in soup kitchens, helped to restore homes, and implemented an afterschool program for children. Some of these experiences occurred during spring or winter breaks and some occurred during the semester. These brief service experiences were not integrated into students' academic course work.

The author's evidence that these experiences helped students transcend otherness while fostering a sense of mutuality and community, factors Rhoads relates to an ethic of care, comes mainly in the form of quotations. The college students said, for example, "working with the people of the streets has transformed 'those people' into real faces, real lives, and real friends" (p. 109). "You can't blame the poor for being poor, at least not in most cases" (p. 146). Students consistently reported that their experiences personalized the pain of homelessness and poverty; they expressed sympathy for those in need; and they described their service experiences as rewarding, empowering, and supporting their own growth.

Unfortunately, the subtle theoretical analysis Rhoads employs in the first section of the book is not maintained when he interprets this data. Most often, assertions made by students that they found their service experience profoundly moving, that they built "real" relationships, and that they now cared for and were more sympathetic toward those they served was taken as evidence of success. We are not told about the ways their caring relationships with these people are similar and different from car-

ing relationships they have with individuals more like themselves. How often, for example, do students stay in touch with those they now consider friends? Although reflection is trumpeted as crucial, we do not learn about any reflective sessions that took place so that we can analyze the ways such sessions might help shape beliefs and understandings.

We also get little help unpacking saccharin statements from students about the "life-changing" consequences of brief experiences providing useful forms of charity work. "I learned that children are the light of the world. They are perfect examples of a love that knows no boundaries of color" (p. 166). In this and most other cases, Rhoads does not do enough to help readers interpret the significance of such statements. Consequently, it is hard to feel confident about when or how these experiences might influence students' behaviors in the future or the place of these experiences in students' life narratives.

Are some of these students becoming sympathetic missionaries? What tradeoffs are embodied in emphasizing service rather than solidarity or structural change? Do service providers develop an inflated view of their own importance? Many of these issues are ably dealt with theoretically and conceptually (especially in the last two chapters that examine the importance of reflection and of "critical community service"), but the qualitative data Rhoads provides does not do enough to portray the prevalence or nature of less desirable outcomes.

Unfortunately, the author waits until page 200 to write "For the majority of the students in this study . . . connecting their participation in community service to larger social issues was not a primary concern." Most, Rhoads says, simply wanted to help others during spring or winter break. "Why are there so many homeless people in this country? . . . [I'm] not sure I want to know the answers because it would probably mean I'd have to consider a lot of serious issues and where I stand on them" (p. 200). By limiting his portrait of such sentiments to a few paragraphs, the author missed an opportunity to use his data to consider how, when, and why community service may not foster development related to care and justice. Attending to this data could also help clarify the meanings students bring to care and justice and the ways they balance conflicting priorities such as personal consumption, individualism, community, and kindness.

In terms of charting a theory-based vision of how community service might enrich higher learning, the book succeeds. The empirical analysis demonstrates that brief and intensive service experiences are often viewed positively by participants. In addition, these experiences often lead participants to articulate generous stances toward those in need and to question stereotypes. This portrait tells readers more about community service's potential than about its limits.

Political Activists in America: The Identity Construction Model of Political Participation. By Nathan Teske. New York: Cambridge University Press, 1997. Pp. ix+180. \$39.95.

Richard L. Wood
University of New Mexico

This book joins an abundant harvest of recent and soon-to-be-published works on the political culture of contemporary American society, most produced by scholars trained in sociology or political science at Berkeley or Harvard, now spread out as junior faculty around the country. While often highly theoretically-informed, this emerging genre typically keeps high theorizing to a minimum in order to focus on illuminating the real social world through ethnographic and interview-based research.

Political Activists in America displays many of the strengths of this body of work. First, the book comes to life for the reader. It opens with a vivid account of a student journalist "crossing the line" into civil disobedience at an antinuclear demonstration and his subsequent reflections on the symbolic line that he crossed within himself that day. Throughout, Teske intersperses such accounts of the experiences and reflections of political activists with his own sociological argument and interpretation. Second, Teske starts with a smart question: Why do some people become heavily engaged in political activism, rather than free-riding on the activism of others? Of course, this question has motivated a great deal of research in the 30 years since Mancur Olson provided its classic formulation. This extensive literature might have made the current book superfluous, but Teske makes a convincing case that our understanding of what motivates activists remains impoverished.

Third, Teske's research design allows for strong comparative cases to inform his analysis. *Political Activists* draws on 80 open-ended interviews with leaders of four kinds of political associations: environmental organizations, social justice advocacy groups, pro-life organizations, and business interest associations. The first three "activist" groups help control for patterns specific to a particular kind of issue focus or political orientation, and the latter "lobbyist" group provides a strong contrast case of "insider politics."

This combination of accessible writing, strong theoretical framing of a smart research question, comparative research design, and rich empirical data mined insightfully makes for a strong sociological argument and an engaging read. Teske argues that the conceptual categories underlying both rational actor and antirational actor approaches to political participation undermine any real understanding of activism by drawing a false dichotomy between self-interested and altruistic motives. Drawing on activists' reflections on their work, he shows how they articulate—and through their activism enact—a moral world in which political participation transcends such categories by serving oneself, others *and* the wider society. The main argument proposes an "identity construction model,"

which rejects both rational actor approaches (with their reduction of persons to economic actors calculating utility functions) and altruistic approaches (with their invocation of a heroic, non-self-interested ethic) by questioning the model of the “self” underlying both. In their place, this model posits an understanding of the self as a moral but not heroic actor; activists engage in politics because through politics broadly understood they “develop certain identities for themselves” and “instantiate” certain qualities they value. In evoking the worldview and self-understanding of his interviewees—in ways deeply respectful of the divergent political positions they hold—Teske enriches our understanding of why people become activists.

Political Activists in America also displays some of the weaknesses of this emerging corpus on American political culture. First, the intensive data gathering necessary for such work often leads researchers, lacking time or funding for extensive travel, to focus their empirical work close to home. In this case, Teske’s political activists all reside in northern California, where the identities and shared culture among political activists may be atypical.

Second, an argument based on probing the worldviews of research subjects must be balanced by a critical stance that sees beyond those subjects’ taken-for-granted understandings of the world. Teske often achieves this balance but falls short in his presentation of political identity construction as a highly self-directed endeavor that occurs exclusively within the process of political activism. By accepting activists’ sense of identity construction as occurring solely in the midst of their political work, this study begs the question of how these activists came to be available for political identity construction in the first place.

For many of the activists portrayed here, religious formation—whether still embraced or now shed—appears to have been central to identity construction earlier in life, particularly in leading them to seek moral meaning in the world and ethical integrity in their own lives. Teske discusses how religious faith often shapes present activism but not how earlier religious or secular ethical formation prepared the ground for it. By drawing on interviewee’s reflections, Teske illuminates the self-conscious formation of the political self, but the formation of what might be called the “proto-political self” remains opaque. Accounting for this crucial aspect of political identity construction would require attention to the cultural currents flowing in the wider society, beyond the confines of the activist organizations studied here.

Teske has written a fine book appropriate for advanced undergraduates and graduate students of political sociology, political culture, and social movements. And we specialists—of positivist and reflexive persuasions alike—will do better work for having read it closely.

Charismatic Christianity: Sociological Perspectives. Edited by Stephen Hunt, Malcolm Hamilton, and Tony Walter. New York: St. Martin's Press, 1997. Pp. viii+236. \$65.00.

Bryan Wilson
University of Oxford

The 10 papers collected in this volume address a variety of issues affecting the recent development of the Charismatic Renewal movement, with some concentration on its prospects in the third millennium. That focus is not induced by any suggestion that latter-day neo-Pentecostalism is committed to any premillennialist agenda: Indeed, the argument of those contributors who take up that concern is specifically to make the point that, for many contemporary Charismatics, the debate between pre- and postmillennial theories of the advent is distinctly passé. The reference point for several authors is rather the effect in Britain of the ecstatic outbursts in Toronto in 1994, which, as "the Toronto blessing," has galvanized some church congregations and revived, at least for the time being, some perhaps otherwise fading charismatic fellowships. Whether the blessing has actually expanded these churches' market share is open to doubt, as Philip Richter makes plain in a supply-side analysis of what Charismatic Christianity now has to offer. However, the book is by no means exclusively concerned with the effect of the Toronto phenomenon, and some papers deal with the somewhat longer history of the movement in its various ramifications, particularly in Britain.

What emerges is a picture of the bewilderingly fissile and inchoate tangle of communities, fellowships, personalities, schisms, and theologies that have been thrown up by charismatic enthusiasm, largely to the cost of the older institutionalized denominations that have sometimes, albeit not without misgivings, played host to these new iconoclastic fashions in styles of worship. Andrew Walker, who has sought in earlier work to sort out the strands constituting Restorationism and to distinguish among the various "House Church" bodies, contributes the key paper to the collection in which he debates whether the new phenomenon is to be properly recognized as postmodern religion. He seeks to rebut the idea that the so-called classical Pentecostalism was antimodern, pointing to its ready adoption of a variety of essentially modern procedures and techniques of advertising, recruitment, organized revivalism, and ministerial training. The issue of finding the appropriate label for contemporary Charismatic phenomena fails, however, to infuse the discussion by most other contributors and may ultimately be of no abiding significance. What is apparent is that neo-Pentecostalism attracts a different public from the sectarian Pentecostalism of the first 50 years of the present century: they are better-to-do, better educated, albeit not less anti-intellectual. Both Walker and Martyn Percy independently trace the theological origins of this style of Christianity back to Schleiermacher.

These essays are primarily descriptive, and sociological analysis is lim-

ited to particular cases. Thus Paul Chambers brings a formidable apparatus of sociological theory to the analysis of a double schism within a congregation in Wales that began as a run-of-the-mill classical Pentecostal church, acquired an innovating pastor, and was joined, but subsequently abandoned, by a radical clientele of quasi-hippy, communitarian Charismatics. More strictly descriptive is Keith Newell's account (perhaps an ex-insider's account) of another particular communitarian development of Charismatic Christianity in the Jesus Fellowship based in a British Midlands village. Martyn Percy, in a theologically informed sociological discussion, describes neo-Pentecostalism as "a major shareholder in the totality of Christian expression." (He estimates that Pentecostals—of all kinds—may be as many as 400 million people worldwide, while Douglas McBain suggests over 600 million.) Yet, quite persuasively, he questions the durability of a pattern of Christian worship that is rooted in emotional experience and that he sees as having fostered schism, induced sectarianism and syncretism, experienced failure, and which, for some, has led to disaster. He sees this fragmentation, the distrust of coherence and metanarratives, and the playfulness and the pleasure principle that are so conspicuous in charismatic worship, and excessively so in the Toronto blessing, as echoing the prescriptions of postmodernism.

The themes addressed in these papers are primarily of interest to sociologists of religion, but it may be remarked that at least four of the 10 authors are clerics and that the theological input is considerable. Perhaps the paper most directly linked to wider sociological issues, at least to the sociology of morals and culture, is William Thompson's densely written discussion of Charismatic politics. Thompson examines the role of Charismatics in the formation in Britain of moral pressure groups on such concerns as pornography, abortion law, Sunday trading, broadcasting standards, and general resistance to the influence of the post-1960s wave of moral permissiveness. His canvass is of course one that encompasses a wider spectrum of fundamentalism than is embraced by the charismatic movement per se, the specific influence and importance of which is perhaps not easy to determine.

On the Boundaries of American Evangelicalism: The Postwar Evangelical Coalition. By Jon R. Stone. New York: St. Martin's Press, 1997. Pp. x+229. \$45.00.

Mark A. Noll
Wheaton College

This book is most helpful when it documents the persistent efforts by self-described protestant "Evangelicals" to define the identity of their movement after its emergence from self-described "fundamentalists" in the 1940s. Jon R. Stone, who teaches interdisciplinary studies at the University of California, Berkeley, argues that participants in this movement

have employed a series of metaphors to describe their own unity-within-diversity. Of those metaphors, Stone feels that historian Timothy L. Smith's evocations of a "mosaic" or a "kaleidoscope" are most interesting. Yet Stone argues that all such metaphors lack analytical rigor. Drawing upon insights from Mary Douglas, Robert Wuthnow, and anthropologist Anthony Cohen, Stone proposes that the postwar evangelical movement is more fruitfully described as a shifting "coalition" preoccupied with the task of securing its boundaries against both a religious right and a religious left. In his sequencing, Stone sees these Evangelicals working hard in the 1940s and early 1950s to differentiate themselves from fundamentalists. He suggests that during the 1950s and 1960s, they achieved modest success in creating a theological esprit and at exerting meaningful social influence, but only because they were able to maintain a conceptual border against fundamentalists while still discriminating themselves from mainstream Protestantism. In the 1970s and 1980s, Stone thinks that evangelical boundary maintenance broke down; the evangelical leaders who had succeeded in setting themselves apart from fundamentalists were not able to hold at bay the often subtle influences of liberalism within their own ranks. Neither did they reach their goal of meaningful social influence, but rather they were forced to observe reenergized fundamentalists (Jerry Falwell, Pat Robertson, the New Christian Right) succeed where they had failed. In making his case, Stone relies heavily on the writing of a pastor and founding president of Fuller Theological Seminary, Harold John Ockenga; the theologian and founding editor of *Christianity Today*, Carl F. H. Henry; the theologian and later president of Fuller Seminary, E. J. Carnell; and the evangelist Billy Graham.

As a study of persistent concern for identity formation among these important religious figures, the book is reasonably successful. For broader purposes, there are problems. First and most important are equivocations over definitions. The leaders featured in Stone's analysis never made up the whole of even self-described "Evangelicalism." They constitute an even smaller segment of the various protestant movements that scholars routinely label "evangelical." Thus, Stone acknowledges insights by the historian Timothy Smith, but the Nazarene, Wesleyan, and Holiness constituencies whose perspectives Smith reflected are never mentioned. This is a serious matter since it means that Stone is unable to show where many of the currently visible evangelical leaders come from, for example, the Nazarene radio psychologist Dr. James Dobson, host of *Focus on the Family*, who has been the politically most influential "Evangelical" in the United States of the last decade. The same may be said about Stone's failure to incorporate into his account major groups like the Southern Baptists and Pentecostals in denominations like the Assemblies of God, whom almost everyone regards as Evangelicals.

Second, Stone may well overstate the boundary-making fixations of even his target group of self-described evangelical leaders. He writes, for example, that "Evangelicalism has been captivated by the issue of defining its boundaries" (p. 179) and that "this boundary dynamic has been

of central concern to Evangelicals" (p. 179). But these are claims requiring more comprehensive study, since the evangelical leaders themselves all would have said they were much more concerned about "winning people to Christ" or encouraging the faithful to lead holy lives than they were about defining evangelical identity.

Third, it is not clear that theories about "coalition" yield better results for this religious movement than does intensive research in data from either field research or the primary written sources generated by the people under consideration. Historians like Timothy Smith may not solve all boundary-marking questions, but their research is often broader, deeper, and hence more satisfying than the quick move made by Stone from a relatively restricted body of sources to grand social generalizations. Despite the contributions of this book, therefore, those who are interested in the boundaries of "Evangelicalism" would do better to consult the empirical social scientists who are working hard to define "evangelical" operationally, including (among others) Roger Finke, John Green, James Guth, Dean Hoge, Ted Jelen, Lyman Kellstedt, and Corwin Smidt. Or they can be referred with profit to a long-standing tradition of debate on such questions by historians like George Marsden (e.g., *Reforming Fundamentalism: Fuller Seminary and the New Evangelicalism* [1987]), Donald Dayton (e.g., "The Search for the Historical Evangelicalism: George Marsden's History of Fuller Seminary as a Case Study," *Christian Scholar's Review* 23 [September 1993]: 12-33), Joel Carpenter (*Revive Us Again: The Reawakening of American Fundamentalism* [1997]), or Douglas Sweeney ("The Essential Evangelicalism Dialectic: The Heterogeneity of the Early Neo-Evangelical Movement," *Church History* 60 [March 1991]: 70-84).

A Treatise on Social Theory. Vol. 3, *Applied Social Theory*. By W. G. Runciman. Cambridge: Cambridge University Press, 1997. xvii+329. \$69.95 (cloth); \$27.95 (paper).

Arthur L. Stinchcombe
Northwestern University

Runciman is trying here to use his theory of evolution of social practices to give a theoretical slant to a narrative of major social changes and major continuities in England from about 1880 to 1990. His most frequent citations are to the first two volumes, and he seems to have read no evolutionary theory since starting the first. The work of John Holland, Stephen J. Gould, Edward O. Wilson, David Hull, Donald Campbell, or the newer subdisciplines of organizational ecology, molecular biology, ecological archeology, neural network theory, coevolution, parasitism and symbiosis, and animal ethology have all passed him by. People who ignore most recent evolutionary theory cannot be taken seriously as theorists in the field.

Nevertheless, Runciman's idea that the units of social evolution are social practices of conflict and cooperation is fundamentally right, I think. It is because organizations are now the main carriers and creators of such practices that organizational ecology has taught us (if not Runciman) so much. This, in turn, means that keeping an eye on what social practices pay off at a given time and place for a given social category, and what causes these payoffs to rise or decline, is a good start for social history.

If we turn from the theory to the substance, recent social changes in England (and occasionally Wales), the relevant comparisons are synthetic accounts of social structures of national societies: for the United States, Robin Williams and Alexis de Tocqueville; for Norway, the book edited by Natalie Rogoff Ramsøy; or for England, earlier Hobsbawm's big book on the industrial and political revolutions.

Compared to those models, Runciman is ponderous and tedious but solid. Part of the tedium is his excessive care to explain why the words he uses are justified. Runciman has a Linnean bent, distinguishing varieties within species (e.g., capitalism with and without strong collective bargaining), species within genres (e.g., socialist, right wing authoritarian, and liberal government of economies), and repeating differentiating features each time the words are used.

Runciman seems to be speaking to an array of imagined critics of his center-of-the-road analysis. He reminds one of C. Wright Mills's description of the ideological audiences that American trade union leaders played to, from Trotskyist to utopian capitalist. But C. Wright Mills regarded these as determinants of the repertoire from which social practices were selected; Runciman mostly sees them as audiences that will not like his book. And Mills, like Robin Williams, de Tocqueville, and Rogoff Ramsøy, could write about the world without reminding one that he had remarked on a given point two volumes ago, in chapter 5, section 2.

Perhaps the most original substantive problem Runciman tackles is the decline of interpersonal deference, such as status differences in clothing (white collar, robes, wigs, hunting costumes, cloth caps), forms of upward address (vocative use of titles and surnames), body language deference (touching the forelock, salutes in the military, curtsying), and the expectation that higher status people in meetings will speak longer, introduce new issues, and close previous ones. A related phenomenon on the continent, documented in Roger Brown's old social psychology text (1965; it is absent in the new edition), is the near disappearance of the more formal second person pronouns (e.g., *vous*, *Sie*, *Usted*) and the rise of the informal ones (*tu*, *du*, *tu*).

Runciman's general idea is that authority is less surrounded with rituals of interpersonal deference, a less permanent feature of the superiors and inferiors and more attuned to rules and diagnoses of situations than to hierarchies of persons. This is a change in the socially organized "mode of persuasion" in a more democratic as well as more impersonal direction Runciman says. Authority no longer persuades by personal inequality of deference and honor but by arguments about law and public policy. The

explanation turns out to be hard to identify in Runciman's treatment. The explanation is not given in Brown, either. One has to say that at least evolutionary theory is no worse than social psychological theory.

This is then a book of medium value as a description of the recent history of the English part of Great Britain. But it fails to improve evolutionary theory, it tangles the tale with much talk about using the rights words, and the rate of self-citation to previous volumes is something of a scandal—too much vanity too publicly displayed. We need someone more like Hobsbawm or de Tocqueville to write this book for us.

Max Weber's Methodology: The Unification of the Cultural and Social Sciences. By Fritz Ringer. Cambridge, Mass.: Harvard University Press, 1997. Pp. ix+188. \$35.00.

Guenther Roth
Columbia University

Thirty years ago, the historian Fritz Ringer established his reputation with *The Decline of the German Mandarins, 1890–1933* (1969). A companion piece, *Fields of Knowledge: French Academic Culture in Historical Perspective, 1890–1920*, followed in 1992. Toward the end of his career, he has turned to a big biographical project on Weber, of which this essay is said to be an advance. A relative newcomer to Weberian studies, he shows fresh enthusiasm, claiming “that Weber’s methodology remains remarkably relevant and fruitful even today” (p. 171).

As a native speaker and modern German historian, Ringer has enviable qualifications that almost all American sociologists cannot help lacking. But disappointingly, he uses his skills for an uncritical, even celebratory, restatement of Weber’s so-called methodological writings along a well-traveled path. The progression is indicated by the chapter headings (and subheadings): the German historical tradition (humanism contra positivism); Weber’s adaptation of Heinrich Rickert; singular causal analysis (objective probability and adequate causation); interpretation and explanation (interpretive sociology and ideal type); objectivity and value neutrality. A last chapter moves from “theory to practice” and ends with five pages on the “practice” of the Protestant ethic study. Only the conclusion, barely six pages, addresses the theme indicated in the book’s subtitle, *The Unification of the Cultural and Social Sciences*, as an intended critical response to the “intra-academic cultural wars” (p. 174). In Ringer’s eyes, what makes Weber so crucial today is the defense of causal analysis against a congeries of subjectivist, deconstructionist, and other literary fashions. At the same time, Ringer even slays the ghost from the opposite extreme to the current fashions, Carl Hempel’s neopositivism, now more than half a century old.

Rather than engaging the recent threats head-on, Ringer opts for an immanent interpretation. His central effort is the treatment of Weber the

"causalist" (p. 62), not to be mistaken for a positivist. This involves a dense summary of objective probability and adequate causation, ranging from Weber's precursor Johannes von Kries to Hempel's alternative neopositivist scheme. The latter is treated critically in a sort of excursus based on a previous publication on causality (pp. 81–91), a segment tenuously related to Weber with some rhetorical devices ("needless to say," "clearly"). For Ringer, the notion of "singular causal analysis" (apparently meaning historically specific explanation) also becomes a promise to bridge the gap between interpretation and explanation.

For the most part, however, Ringer limits himself, at one extreme, to running commentaries on a large number of secondary authors and, on the other, to conventional textbook expositions—some parts are for the cognoscenti, others for the untutored. The reader understands with whom he agrees or disagrees but not sufficiently the grounds of his judgments. Thus, impatient with Rickert's "philosophical obscurities" (p. 169), he finds it "impossible to accept the view that Weber was essentially a follower of Rickert, a view proposed by Thomas Burger and fully developed by Guy Oakes" (p. 51), but their acute philosophical reasoning is not addressed. Ringer does not face the daunting problems posed by the fragmentary, elliptical, tortuous—and self-torturing—nature of the "methodological" writings, apart from conceding "Weber's occasionally difficult arguments" (p. 90). There are severe limitations to treating these writings in isolation rather than reconstructing Weber's research strategy and program on a broader basis, as Wolfgang Schluchter and Stephen Kalberg have done in their divergent ways. After all, Weber's international fame rests not on any advocacy of "singular causal analysis" but on the (non-causal) "structural phenomenology of world history" (Johannes Winckelmann) in *Economy and Society* and *The Economic Ethics of the World Religions*.

If nothing else, the format of such an extended essay—too long for an article, too short for a book—works here against making a real contribution to the vast literature on Weber that has accumulated over the decades. From the present exposition, neither friend nor foe, historian nor sociologist, is likely to get new insights for confronting the perceived current crisis. I doubt that such a narrow methodological celebration of Weber will win any battles. If one wants to join the fray on Weber's side, one might start with the "politically incorrect" passage from "Objective Possibility and Adequate Causation": "The fact that [the battle of Marathon] decided between these two possibilities [the rise of western civilization and theocratic domination under the Persians] is obviously . . . the only reason why we are historically interested in it. . . . Without an appraisal of those possibilities and of the irreplaceable cultural values which . . . depend on that decision, a statement regarding its significance would be impossible. . . . There would in truth be no reason why we should not rate that decisive contest equally with a scuffle between two tribes of Kaffirs or Indians. . . . The notion of a sort of social justice which would—finally, finally!—take the disdainfully neglected Kaffir and In-

dian tribes at least as seriously as the Athenians . . . is merely childish" (*The Methodology of the Social Sciences* [Free Press, 1949], p. 172). That would make Weber's methodology "remarkably relevant," with a vengeance.

The Problem of Trust. By Adam B. Seligman. Princeton, N.J.: Princeton University Press, 1997. Pp. vii+231. \$29.95.

Margaret Levi
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Adam B. Seligman offers the strong claim that the problem of trust, that is the widespread lament for its decline, is an effect of the increased dependence upon trust just at the moment when the conditions for trust are being undermined by structural change. He grounds his account of trust in role segmentation, role conflicts, system limits, and institutionalization, and he draws heavily on history, philosophy, religious theory, and political theory to examine and support his position. For Seligman, both trust and its opposite, mistrust, emerge only with individualism and only when agents can and must negotiate over role expectations. Trust is a modern phenomenon that mediates relationships among individuals; it involves risk of a kind that can only occur when individual agents find themselves having to interact with strangers or with those whose responses they cannot confidently predict.

Seligman has a multitude of purposes in his book. He constructs a sociological, rather than a psychological, concept by giving trust a structural basis. He differentiates trust from other terms in a general family of concepts that include reliance, confidence, faith, *fides*, familiarity. He considers and evaluates a set of related literatures. He aims to explain why declining trust has become a subject of concern.

Seligman makes considerable progress toward clarifying the sociological dimensions of trust. Contrasting his position with Émile Durkheim's and drawing on a host of authorities, most notably Robert K. Merton, Niklas Luhmann, Charles Taylor, S. N. Eisenstadt, and Ralph Turner, he identifies interpersonal trust as a means to mediate certain kinds of role conflict that evolve with the large-scale political, social, and particularly economic changes that produce modernity. Seligman nicely links trust to the "social conditions of risk" (p. 170) and to the development of individuals who are capable of forming and sharing strong evaluations.

In the process of providing his sociological and historical analysis, Seligman distinguishes trust from related concepts. His distinctions are as good as any. He is convincing that there are numerous phenomena, with quite different sources, that often bear the same name. In clearing the brush, he clarifies what is central to trust, namely, a social interaction among individuals who find themselves in social situations "that can no

longer be adequately encompassed within the matrix of normatively defined role expectations" (p. 63).

Seligman then turns to a diverse set of literatures to develop and apply his concept. He is an extremely learned scholar, but his erudition often produces too many citations per sentence and makes these chapters turgid and hard going. More importantly, he offers no major insights in his lengthy discussions of civil society, civil virtue, social capital, the public, and the private. Though there are interesting observations (and there are several), they seem less the result of the analytic usefulness of Seligman's conception of trust than of the application of his mind to the questions at hand.

Sometimes, Seligman's discussions muddy the issues, rather than clarify, as in the chapter on generalized exchange. He appears to be offering a critique of the way in which Robert Putnam, Francis Fukuyama, Kenneth Arrow, James Coleman, and others use terms such as trust and social capital. While he notes the variety of principles of generalized exchange, he argues that trust "emerges and is predicated on only one particular way of providing the unconditional bases of system confidence. It is . . . one that is undoubtedly connected with the 'rise of the West'" (p. 85). At this point, I begin to find myself lost, and by the end of this chapter, I am as unclear as I was at the beginning about Seligman's position on social capital or on Arrow's argument for trust as a lubricant of exchange.

My dissatisfaction with the argument continues through the end. Seligman is not playing to his strengths when he attempts to generate hypotheses, make predictions, or submit strong empirical claims based on his conception of trust. His contrast of the United States and Europe does not stand up to careful scrutiny, nor is it consistent with his assertion that the same sociological consequences are being experienced on both continents. His conclusion that the decline of trust is taking place as a consequence of the destruction of individualism and too great a complexity of role differentiation rests on several problematic assumptions about the nature of structural change in the contemporary era and about the effects of increased role differentiation.

The fact that Seligman is more compelling in explaining the past than in predicting the future is hardly damning. His account of the structural conditions for the emergence of trust is largely convincing, and with it he both complements and adds to a considerable—and growing—literature.

Moral Order and Social Disorder: The American Search for Civil Society.
By Frank Hearn. New York: Aldine De Gruyter, 1997. Pp. xv+206.

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Prophetic sociology risks dramatic oversimplification as it presents its truths about the social world, and it is prone to fulminant predictions of

doom unless its advice is heeded. This fine book largely avoids these pitfalls. Although its version of truth is rooted in the sociological tradition of Durkheim and Mead, it considers and gives a fair hearing to a variety of remedies for the contemporary social breakdown. This book is not yet another jeremiad; Hearn's stance is one of earnest hope, not self-righteousness, as he attempts to revive this sociological tradition as a way of approaching contemporary problems.

Contemporary American society is in crisis, Hearn argues, as social institutions such as the family weaken and communitarian interdependencies are eroded. The crisis is the bitter fruit of the enduring conflict between the individualism fostered by liberal modernity and the sentiments and practices of community on which social and moral order depend. Individualism is championed both by the capitalist market economy, with its rhetoric of individual choice, and by the liberal democratic (and, lately, welfare) state, with its rhetoric of individual rights. These forms of individualism have expanded at the expense of sociability and social solidarity, for they "detract from the social settings most congenial to the growth of trust, gratitude, mutuality, and social and personal responsibility" (p. xii). Although both right-wing advocates of the market economy and left-wing advocates of the welfare state promise to solve the problems of family collapse, neighborhood decay, crime, and social alienation, their proposals exacerbate the conditions they are intended to improve. The way out of the contemporary crisis, Hearn argues, lies in an updated version of Durkheim's moral individualism; it is sociology's task to illuminate the social conditions under which moral individuals flourish and to promulgate a sociological language in which problems of social disorder may be conceived.

Moral individuals feel tied to others. They are grateful for the gifts they have received from them and are affectively bound to duties to them rather than merely rationally committed to personal goals. They trust others and feel generally that others can be trusted. They feel responsible not only to secure their own ends, but to help others secure theirs. They value the worth and dignity of the individual and understand that these ends are best promoted (and the skills necessary to securing them best learned) in an active, vigorous social life. Human beings have a natural sociability that inclines them to seek out others, to develop relationships with them, and to seek not only the approval and respect of others but self-respect as well. And moral individuals in the modern world must learn to extend their moral sentiments not only to those in whose midst they have learned them, but to a wider community of human beings.

In chapters 1 and 2, Hearn applies his approach to contemporary problems of "bad parents" and "bad neighbors," explaining how both the market economy and the liberal welfare state undermine social institutions, social control, and communitarian interdependencies. Chapter 3 develops the sociological critique of liberal modernity and links it to the anomie of contemporary life. Chapter 4 explicates Durkheim's theory of morality in illuminating detail. Chapter 5 is in many ways the best chapter in the

book because of its incisive critique of the idea of social capital and recasting of it in terms of gifts and the sacred (and also because of its delightful and effective use of the Frank Capra film, "It's a Wonderful Life," to develop its argument). Chapter 6 presents another particularly insightful analysis of the manifest "attention deficit disorders" of contemporary life and argues for sociology's role in promoting a language of solidarity to contend with the insistent languages of individual choices and rights.

This book does not so much break new ground as it cultivates and fertilizes the sociological acreage in interesting, enlightening, and productive ways. It will be of particular use in the advanced undergraduate and beginning graduate classroom—in courses in classical and contemporary sociological theory as well as in courses in social problems and community sociology—where it will both inform the student and serve as a vivid model of sociological analysis and insight. Seasoned professionals will also find much of value, for the author seeks not only to teach his students but also to reawaken the sociological imagination of his colleagues. His book has a perhaps understandable tendency to hope for more influence by such reawakened sociologists than they may ever have. And, like others in its genre, it gives too little credit to the capacity of those contemporary men and women with whose lives the author is deeply concerned both to recognize the origins of their own problems and to take matters into their own hands. Communitarian sentiments and practices are more widespread than most sociological prophets imagine, and we must learn to recognize them when we see them. Nonetheless, the book succeeds in its goals, and it deserves to be widely read.

The People's Lobby: Organizational Innovation and the Rise of Interest Group Politics in the United States, 1890–1925. By Elisabeth S. Clemens. Chicago: University of Chicago Press, 1997. Pp. xi+459. \$58.00 (cloth); \$19.95 (paper).

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It has now been nearly half a century since political scientist David Truman penned his classic statement of the "interest group politics" paradigm, *The Governmental Process* (Alfred Knopf, 1951). Variants on Truman's original approach—reflecting inherent assumptions that political behavior can be understood as rational action by self-interested participants, and that "lobby" or "pressure" groups emerge within political systems in order to aggregate those interests in demanding particularistic responses from government—have long since become the taken-for-granted premises informing a great deal of modern scholarship on the political sociology of groups.

But, as Elisabeth Clemens aptly illustrates in *The People's Lobby*, "in-

terest group politics entails more than (just) the aggregated interests of self-interested individuals" (p. 6). In a refreshing attempt to get beyond the explanatory limitations of earlier accounts of how group politics arose in the American context, Clemens skillfully integrates the best of recent organizational and cultural approaches to historical sociology, weaving a rich analysis of the "social origins" of interest group politics during the Progressive Era. The central premise of this book, and the related theoretical debates it invites, will be of particular interest to political scientists, historians, sociologists, and students of the dynamics of social change. Clemens contends that during the period between 1890 and 1925 an American political order, which up to that point had been largely dominated by party organizations and elections, was rather dramatically transformed by an emergent "people's lobby"—manifesting new processes of "organizational innovation" in which citizens/voters began to learn how to hold their elected representatives accountable at the polls, how to monitor legislative processes, and how to effectively institutionalize their ongoing participation in shaping administrative and legislative policies. These changing forms of civic engagement during the Progressive Era, invented by citizens/voters as strategies to circumvent the parties, are examined within the politics of three exemplary states—California, Washington, and Wisconsin—using organized labor, farmers, and "the organizational accomplishments of disenfranchised women" as examples of mobilized interests. What emerges from this analysis is a considerably more complex and multifaceted account of the nature of American interest group politics and its social roots than is to be found in previous accounts of this period—particularly with respect to processes of organizational and institutional change.

What weaknesses there are in this volume—and they are relatively minor—stem from a reluctance to fully relinquish some of the old "group politics" premises it initially sets out to remedy. Though the book's prologue ambitiously critiques the old Arthur Bentley-David Truman interest group politics paradigm as outmoded, the alternative analysis that follows in fact retains some of that paradigm's explanatory limitations. First, the historical account continues to rely on motivational assumptions that imply that participants' attempts, in 1890–1925, to revise the existing American sociopolitical order were essentially a matter of self-interested rational choice. As political scientist Murray Edelman discovered years ago in his analysis of "tired groups" in *Symbolic Uses of Politics* (University of Illinois Press, 1964), rational choice models can explain at best only part of the variance in human political behavior, and institutional and organizational factors tend to be of limited utility in explaining the rest of it. Most political behavior—then as now—is not strictly rational but is usually also symbolic, involving the social creation of meaning and arbitrary definitions of "political reality." Second, the book's primary focus on interest organizations, as distinct entities that articulated circumscribed group interests, tends to obscure the convergence during that era of more profound societal forces—both progressive and reactionary—

thus missing the forest for the trees. Viewing Progressive-Era populism directly within the broader perspective of ongoing social movements and class relations would help bypass some of these myopic limitations of the interest group pluralist model, focusing attention instead on some of the ways in which citizens during this era attempted to form coalitions that went significantly beyond particularistic interests, in response to larger perceived societal needs. Finally, the volume's focus on interest organizations tends to obscure the pivotal importance of the class-skewed "critical realignment" of 1896—which fundamentally reshaped the political system along class lines in a more conservative, pro-business direction, and precipitated the pre-New Deal electoral demobilization of lower strata voters that rendered the "rise of interest group politics" necessary for sociopolitical survival. (See Walter Dean Burnham's *Critical Elections and the Mainsprings of American Politics* [W. W. Norton, 1970]; and E. E. Schattschneider's *The Semisovereign People* [Holt, 1960].)

These minor reservations aside, Elisabeth Clemens has produced a valuable scholarly contribution to our understanding of the social roots of interest group politics in the United States in the 1890–1925 period, which deserves careful scrutiny by students of 20th-century U.S. history, organizational change processes, and the political sociology of group behavior.

Plutocracy and Politics in New York City. By Gabriel A. Almond. Boulder, Colo.: Westview Press, 1998. Pp. xxvii+260. \$27.00.

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This book represents the belated publication of a 1938 dissertation by political scientist Gabriel Almond, who provides an introductory "remembrance of things past" notable for its comments on famous University of Chicago professors of the 1930s and its critique of those who think democratic market societies can function satisfactorily without a "redistributive welfare net" (p. xxv). The book begins with a six-page foreword by urban political theorist Clarence Stone, who sees Almond's findings as an anticipation of his "regime" theory.

The book has many interesting insights on the role of glittery social events in creating a sense of in-group cohesion in the "plutocracy," Almond's term for both rich people as a social stratum and a polity in which rich people "enjoy a substantially unequal share of political power" (p. xxii). The main empirical findings of the book, however, concern the social backgrounds and connections of government officials, political figures, and directors of business associations in New York from the 1770s to the 1930s. Generally, Almond reports a decline in "high society's" involvement in business groups and a decline in direct political involvement by both high-society and business leaders, although he finds the

decline is much less for the most important political offices. Moreover, he is quick to say that the control of New York politics by the plutocracy did continue by other means, especially campaign finance, lobbying, and philanthropy. Further, he argues that the "various strata of the plutocracy," which differ in religion, national origins, and the age of their wealth, can unite and return to politics directly when they need to, such as times of government corruption, wars, or depressions (p. 239).

Almond attributes the decline of the plutocracy's direct involvement in New York politics to several factors: the Revolutionary War, which legitimated lower-class involvement in politics; the stratification of the plutocracy into "old" families and "parvenues"; and the need for full-time involvement in politics in order to succeed in a society where everyone can vote. The end result in times of social tranquility is a relatively isolated plutocracy that indulges in social extravaganzas and a "class of professional politicians," mostly lawyers, "who engage in politics as a business enterprise, profit oriented, and hence safe as far as the plutocracy was concerned, since profits were to be gained by working along with the wealthy business interests" (p. 237).

In his foreword, Stone claims Almond's dissertation is an anticipation of the idea that local government officials join with business interests because organized business usually provides the most stable and reliable coalition partner, thus creating the typical urban power structure, which he calls a "regime." He says that "Almond shows the shift away from domination by individuals of wealth and prestige, in the early days of the nation, to a more diverse set of officeholders as democratization takes hold" (p. xiii). In Stone's version, "holders of wealth" merely have to be "reckoned with to a special degree" by government officials (p. xiv). But it seems more likely that Almond shows the shift from one form of class domination to another.

Both Almond and Stone ignore the fact that the "good government" movement of the Progressive Era reduced the need for direct business involvement in local government by institutionalizing a variety of procedures, commissions, and governmental structures that minimized the impact of average voters and elected officials. They also ignore another likely factor in the overall decline of direct plutocratic involvement in city politics, the differentiation of business interests into local growth coalitions, based in the intensification of land use, that involve themselves in urban politics, and a nationwide capitalist class that makes profits through the sales of goods and services and usually has little direct involvement in local politics (John Logan and Harvey Molotch, *Urban Fortunes* [University of California Press, 1987]). That is, as nationwide markets developed and the federal government became more important to capitalists operating in those markets, it should not be expected that the plutocracy as a whole would be involved in city politics.

Almond and Stone lament the "deterioration" of the inner city. Almond attributes this "crisis" to "the resistance of the economically advantaged strata to maintain, to say nothing of increasing, the level of social expen-

diture" (p. xxvi). Stone asks why slum clearance and the building of convention centers and sports arenas has not "prevented an escalating crime problem and deteriorating schools" and answers that it is because "cities have been enabled to do only those things that were directly profitable and appealing to business interests" (p. xvi). But neither stresses that growth coalitions directly create many of these problems by expanding the downtown and urban universities into neighborhoods, as Stone shows for urban renewal in Atlanta (*Economic Growth and Neighborhood Discontent* [University of North Carolina Press, 1976]). Almond's book is worth pondering, but neither Almond nor Stone provides the best starting point for understanding urban politics.

Passages to Power: Legislative Recruitment in Advanced Democracies. Edited by Pippa Norris. New York: Cambridge University Press, 1997. Pp. xvi+259. \$59.95 (cloth); \$22.95 (paper).

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Dartmouth College

After languishing during the 1980s, legislative recruitment has once again become a vital area of research. Driven partly by the desire to explore the obstacles women and minority groups encounter in seeking public office and partly by the need to understand how widespread changes in national party systems affect the nomination and selection of candidates, studies have proliferated on both sides of the Atlantic and, to a lesser extent, in Asia and Latin America. Meaningful comparisons across countries, however, have suffered from the lack of a common conceptual scheme that enables scholars to assess the relative impact of institutions, party systems, and personal calculations in the attainment of legislative office. This volume, *Passages to Power*, edited by Pippa Norris, is an important step in bringing order to an unruly subfield.

The core question of the book is how individual actors—potential candidates and gatekeepers—"interact within different institutional settings" (p. 1). Nine countries, plus the European Parliament, provide the institutional variation, ranging from the fragmented multiparty systems of Finland and the Netherlands, to the moderate multiparty systems of Australia, Canada, Germany, and New Zealand (after 1993), to the two-party systems of the United Kingdom and United States, and finally, to the predominantly one-party but transitional system of Japan. Each of these countries differs, as well, in its use of single-member districts, proportional representation, or combined methods for allocating votes. The common thread amidst this welter of institutional arrangements is the authors' focus on the supply of candidates and the demands of the "selectorate" that designates nominees. By supply-side factors, the authors typically mean the personal experiences and attributes that motivate candidates to seek office. By demand-side factors, they focus on the judgments

of party officials and activists regarding the capacity of individual candidates to represent the public and win elections.

Each of the chapters stands on its own in providing a comprehensive analysis of the recruitment process in the selected countries. In this respect, the book will be of interest to country and area specialists, to students of political parties and electoral institutions, and to those interested in the social and political stratification of advanced industrial societies. But the contributors to *Passages to Power* have also shown a commendable self-discipline in adhering to their common framework of analysis. By including careful descriptions of the opportunity structures found in each country and combining them with surveys of the relevant elites engaged in legislative recruitment (with the exception of Fukui and Herrnson who wrote on Japan and the United States, respectively), they have embedded their findings about the attitudes of candidates and party activists in a broader context. An examination of recruitment from both a macro- and microlevel perspective is a research strategy that several scholars, including this writer, have advocated for some time, and so it is intriguing to see it attempted in so many different national contexts.

Upon closer inspection, however, the supply and demand schema is not as tidy as it appears, because some traits, such as gender or prior officeholding and campaign experience, affect both the potential candidates' level of political ambition and the selectors' assessments about their fitness for office. Moreover, the authors tend to rely on it to organize the descriptions of their data rather than to model the relative influences of the various factors. Indeed, only the Norris analysis of recruitment to the European Parliament provides any systematic attention to the relative weight of supply and demand factors in estimating candidates' electoral success.

Despite the ambiguity about the interaction of supply- and demand-side effects, what is quite remarkable about the studies collected here is the amount of agreement on what variables are important in explaining recruitment. A major achievement of the book, then, is its demonstration that scholars from widely different political systems can speak intelligibly to each other about one of the most important aspects of democratic politics.

What they have to say to each other cannot be easily generalized, however, which leaves the reader wishing for a concluding chapter that pulls the disparate country findings together. Several themes do emerge from the chapters: the socioeconomic and professional biases in recruitment, the continued underrepresentation of women, the institutionalization of a political class, and the increasing disaffection of voters with existing mechanisms for choosing leaders. In addition, some patterns emerge in several countries that invite speculation about causation, such as the connection between local control of nominations and the value of incumbency or the importance of party lists in promoting women's candidacies. Yet, these patterns are hardly news to anyone who has followed the liter-

ature and genuine comparison on the fundamental questions of who pursues legislative office, and how the winnowing of candidates takes place remains elusive.

If the book does not live up to its full potential, it is nevertheless a landmark in the development of collaborative approaches to political recruitment. The authors have established the baseline survey data and institutional contexts for their respective countries. They have also identified the key variables and created the conceptual foundation for a truly comparative effort in the future.

The Politics of Sex: Prostitution and Pornography in Australia since 1945. By Barbara Sullivan. New York: Oxford University Press, 1997. Pp. viii+280. \$64.95 (cloth); \$22.95 (paper).

Lynn Sanders
University of Chicago

This careful book examines changes in the political and legal discourse surrounding the Australian regulation of prostitution and pornography. Since World War II, in Australia as in the United States, these two "sex industries" have expanded dramatically. Massage parlors, escort services, videos, and the Internet have joined or replaced brothels, street solicitation, sex shops, and printed materials to produce the wide variety of commercial sex now available. These transformations have in turn led to alterations in the politico-legal discourse of regulation, which Sullivan traces by looking at state and federal statutes and parliamentary debates about prostitution and pornography over the last century.

Sullivan's book is organized chronologically, illustrating how commercial sex has been regulated and discussed in the Australian states at different times. At the beginning of her book, she justifies her focus on legislation: parliamentary debates show how "public figures constitute authoritative meanings" (p. 9). New understandings of sex work have emerged, Sullivan suggests, in a context of broader social transformation. They track with a growing emphasis on mutuality in Australian definitions of normal (hetero) sexuality, with the success of feminist efforts to acknowledge sex work as real work, and with an increase, from 2% in 1972 to 11% in 1989, in the representation of women in Australian parliaments. Sullivan surrounds her discussion of regulation with references to these broader changes. The idealization of reciprocity and equality in heterosexual relationships, for instance, is reflected in pornography regulations that acknowledge both the right to use it and the demand not to see it, and in prostitution regulations directed at clients as well as prostitutes.

Readers interested in theory, about law and social change or about the processes by which some sexual practices emerge as normal and others as pathological, will not find it here. Sullivan refers to broad changes and

processes but does not venture far to explain them. Both the strengths and weaknesses of Sullivan's heavily descriptive account are evident, for instance, in her discussion of prostitution in the current period. Australia, Sullivan remarks, "has a greater diversity of laws addressed to prostitution—and has implemented a larger range of decriminalisation strategies—than any other country in the world" (p. 199). The details of these differences are fascinating. But Sullivan does not make strong claims about why the different Australian states have arrived at the radically different regulatory strategies she describes or how, within jurisdictions, legislators balance conflicting priorities.

This agnosticism may frustrate some, and indeed, absent a clear theoretical apparatus, it is often difficult to keep track of the details Sullivan offers. She may back off from theory for a specific reason: the book reflects her own dissatisfaction with feminist arguments, such as those advanced by Carole Pateman and Kathleen Barry, that sex work is sexual slavery. "‘Big’ arguments" about women's oppression cannot address the diversity of sex work or guide feminist politics, she says (p. 241). Sullivan definitely succeeds in her aim to dispute essentialist claims both by feminists (that sex work is always slavery) and by others (that these industries serve enduring natural needs—as suggested by the remark that prostitution is the world's oldest profession). Clearly what Australians have considered pornography and prostitution has hardly been fixed; in the Australian Capital Territory and in Queensland, for instance, prostitution now may include acts of masturbation and voyeurism (p. 205).

The book does offer an interesting case for students of comparative politics and sociology, law, constitutionalism, and social movements. There are many similarities in the ways that Australians and Americans have defined and regulated pornography and prostitution, which are particularly interesting because Australians enjoy no constitutionally protected right to free speech. The First Amendment rhetoric that governs so many American discussions of pornography is absent from Australian debates, and Australian legislatures have been much more willing to accept the claim that violent pornography is dangerous. Arguments by Australian feminist activists have also entered legislation about prostitution, especially in the claim that sex work is real work.

A paradox about sex work in Australia emerges from Sullivan's book. Commercial sex is, in a way, increasingly tolerated but only as it has been increasingly removed from visibility. Though prostitution is regulated in diverse ways, all Australian legislatures have moved in the recent past to penalize street solicitation (and open homosexual cruising, whether solicitation or not); pornography, too, is regulated to keep it from the eyes of those who would be offended by it. Such efforts to keep sex from view of course require extensive regulations and expansive bureaucracy. Indeed, the book, in general, sketches the development of increasingly complicated, perhaps frightening, mechanisms for regulation and surveillance, which Sullivan only occasionally bemoans. These regulations often contain substantive reflections of feminist arguments she applauds. In her

view, "debates about what should count as normal and appropriate sexual practice have often been advantageous for women" (p. 241).

Male Femaling: A Grounded Theory Approach to Cross-Dressing and Sex-Changing. By Richard Ekins. New York: Routledge, 1997. Pp. xv+185. \$69.95 (cloth); \$17.95 (paper).

Suzanne J. Kessler

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Richard Ekins, Director of the Trans-Gender Archive at the University of Ulster, has studied male cross-dressers and "sex changers" in contemporary Britain. His analysis is based on access to archival material and the opportunity to meet, over the last 17 years, more transvestites, transsexuals, and transgenderists than most gender researchers.

Although vague about his role vis-à-vis his informants (describing himself as a "casual participant" and "overt observer"), he notes that "in the course of . . . mutual exchange I have earned the confidence of the transgender community" (p. 4), and the reader does have the sense that Ekins is a trusted reporter and knows what he is writing about.

Ekins coined the term "male femaling," a phrasing that at first seems awkward but is actually a very useful expression for clustering the various ways that "genetic males" (his usage) appropriate female/feminine properties: in fantasy or in reality, sporadically or continuously, publicly or privately, alone or with companions, deliberately or impulsively, permanently or temporarily.

Whether one "females" through one's body or one's clothing is not particularly important, argues Ekins, nor is it conceptually important whether one's "femaling" has an erotic component. Theoreticians and clinicians who assign gender-related categories on these bases are focusing on dimensions that may not be the most relevant to those doing the "femaling." Traditional categories like transvestite or transsexual (or even the newer category transgender) presuppose fixed identities and meanings, and Ekins convincingly shows "the ambiguous, ambivalent, multi-contextual, multi-dimensional, emergent nature of much cross-dressing and sex-changing phenomena" (p. 2).

The bulk of the book describes the five-phase career path of "male femaling": beginning, fantasizing, doing, constituting, and consolidating. Through numerous powerful illustrations, Ekins demonstrates that the meanings people ascribe to their gender choices are much more fluid than has customarily been credited.

One "male femaler" who Ekins profiles enjoys clothes shopping in female attire without finding these trips sexually arousing at the time. But when "he" returns home, thoughts of *being* one of the women in the communal dressing room and of *desiring* them triggers sexual arousal. Is the desire experienced as a woman or as a man? Why does the pleasure ap-

prehended through the penis not inhibit the “male femaler’s” identification with the female? These are some of the fascinating questions that Ekins considers. There are no obvious answers—none that would permit us to unequivocally classify the “male femaler.”

Ekins’s central point is that “male femaling” is more complex than most previous researchers and clinicians have considered. Even when “male femalers” adopt one of the available categories, their adoption does not make the category natural or their placement in it correct. “In actual lived experience, development and order are constructed post facto, and anew from the standpoint of each new present. The phases . . . may be circled and cycled again and again” (p. 130).

The core of the book follows three theoretical chapters that Ekins says the reader can skip. I agree. His explanation of grounded theory is not developed enough to permit readers not previously familiar with Barney Glaser and Anselm Strauss’s (*The Discovery of Grounded Theory: Strategies for Qualitative Research* [Aldine, 1967]) initial formulation to grasp it. According to Ekins, those who work within grounded theory’s framework “enter the research setting with as few predetermined ideas as possible, and should be prepared to change those they already have, readily and frequently. They avoid imposing their own particular version of reality on their subjects” (p. 37). It is one thing to pledge this; it is another to demonstrate it and distinguish this commitment from similar commitments of other post-positivist theories. His summaries of other theoretical approaches to cross-gender dressing and category switching are unnecessary for advanced students and will not suffice for the beginner.

Ekins is correct that he is riding on the crest of a wave of interest in cross-dressing and sex-changing. He is mistaken that he is the only researcher to have written from the informants’ point of view. Nor has he (as he claimed) provided an analysis completely different than anything that has come before it. Anne Bolin, Dallas Denny, Holly Devor, Deborah Feinbloom, John Talamini, and Annie Woodhouse (all of whom he cites) have collected first-person accounts and attempted to frame them within some kind of gender theory.

I have two additional criticisms. In spite of discussing his data in ways that he believes deeply challenge gender essentialism, Ekins has in fact maintained a sex/gender (biological-cultural) distinction and failed to fully integrate the social constructionist view of gender held by progressive gender theorists. Secondly, although he acknowledges the feminist critique that “male femaling might be seen as reinforcing gender role stereotypes thereby reproducing rather than changing the existing arrangements between the sexes” (p. 40), he provides no response to it and regards his role as researcher to “plot these reproductions.” That will hardly satisfy most critics.

Notwithstanding these limitations, the book is worth reading. Ekins has selected vivid cases of gender arrangements and encourages readers to think about them in the broadest possible way. He concludes by urging others to study “female maling,” “male maling,” and “female femaling.”

This recognition that all "gendering" is an accomplishment and that "male femaling" is but a single example is exactly the message readers need to take with them.

Queer Fictions of the Past: History, Culture, and Difference. By Scott Bravmann. New York: Cambridge University Press, 1997. Pp. xv+174. \$54.95 (cloth); \$16.95 (paper).

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In *Queer Fictions of the Past*, Scott Bravmann argues that historical narratives serve as cultural and political interventions into the present by constructing contemporary queer subjectivities. Bravmann contests queer historiography's construction of stable homosexual identities that elide racial, gender, and class differences. Bravmann concludes that "hybrid texts" that mix biography with fiction are better suited to understanding diverse lesbian and gay subjectivities.

Bravmann begins by challenging narratives that assert conditions associated with modernity, such as anonymity and economic freedom from the traditional family structure, allowed a homosexual identity to develop. Rather than understanding such texts as fact, they must be understood as narratives that actively construct the "fiction" of the modern homosexual. The modernist account "anchors present identities in a stable, coherent personal and social past . . . overrides, disallows, and denies other experiences; and implicitly grounds conceptions of gay identity within the specific experiences of urban, middle-class white men" (p. 10). The linear trajectory of materialist social histories is belied by differences among queer subjects, past and present.

Bravmann argues that recent historiographies attuned to multiple social differences attest to the inadequacy of the modernist and materialist thesis. Contrary to Bravmann's interpretation, however, it remains unclear that differences produced by the intersections of race, class, and gender cannot be at least partially explained by more sophisticated structuralist accounts. For example, the development of working-class queer identities can in part be attributed to the separation of work and leisure time allowed by a modern economy.

According to Bravmann, a search for racial diversity and gender parity has led social scientists and activists alike to re-form the 1969 Stonewall riots into a glorious tale of multiracial gender-neutral heroism and triumph, which those in the present should strive to replicate. While Stonewall has been racialized, other aspects of lesbian and gay history have not been. "This absence is . . . a veil which obscures racial differences and racial *meanings* under the unifying category of homosexuality" (p. 77). Similarly, Bravmann argues that the contemporary desire for gender inclusion has been refracted back onto Stonewall to highlight lesbian par-

ticipation in what was in reality only a minor event in lesbian history. Relying on one photograph for evidence, Bravmann contends that lesbians were quite possibly not present at Stonewall and, even if they were present, were probably not as integral to this event as queer historiography would have us believe. In short, the multiracial, cogenerated Mecca of Stonewall is a case of historical wishful thinking.

The political strategy associated with the modernist promise of a stable identity in a world of change is "coming out." By itself, Bravmann claims, this strategy is of questionable political value because it mandates homogeneity by demanding primacy for a core lesbian or gay identity. The post-Stonewall emphasis on visibility provides the basis for a politics among people who otherwise have little in common. By openly confronting the racial and gender tensions present at Stonewall, we can assess the potential for developing communities that recognize difference.

The crux of Bravmann's argument is that historiographies should be understood as representations and performative sites that intervene in the present. "Hybrid texts . . . that weave autobiography, poetry, documentary material, feminist theory, personal narratives of desire, critical analyses of the structures of social domination, (science) fiction, and at times visual images into complex representations of queer historical subjects" (p. 96) are more effective interventions into the present. Queer fictions of the past by lesbians and gay men of color destabilize identity categories and therefore contest the modernist project of historiography. Such narratives, for example, interrogate the use of national borders as a site for studying history and culture. Historical representation, in other words, should be considered "a way of struggling over and addressing current social problems" (p. 113).

This provocative book made me think critically about both my own scholarship and the work of others. The intelligent deconstruction of the sacred cows of queer historiography should remind scholars and activists alike to examine our own epistemological assumptions and our roles in creating queer fictions of the past and their potential to influence the present. But Bravmann does not sufficiently acknowledge that different "narrative" forms address different questions. Judging one form by the standards of another misses the complementarity of different approaches. While hybrid texts address the internal dimensions of gay and lesbian subjectivities, they are less able to explain the development of the visible lesbian and gay movement.

Although representations of lesbians and gay men may intervene in the present, the dense narrative style of *Queer Fictions* ensures that it will do so only for a few. Nonetheless, this book would be useful in graduate courses on epistemology, the sociology of knowledge, or queer theory and postmodernism. This book should remind sociologists to think critically about the questions we ask, the subjects we choose, and the groups we omit.

Armed Robbers in Action: Stickups and Street Culture. By Richard T. Wright and Scott H. Decker. Boston: Northeastern University Press, 1997. Pp. xiv+154. \$37.50 (cloth); \$15.00 (paper).

Mercer L. Sullivan
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This lean and direct volume delivers on its title. The authors located and interviewed 86 active armed robbers in St. Louis. In semistructured interviews, they elicited accounts of why and how these people committed their crimes. The book presents their findings along with discussions of sampling, the hazards and contingencies of the field research, how their findings compare with previous research, and policy implications.

The book's main claim to originality is that it is the most comprehensive study to date of active, as opposed to incarcerated, armed robbers. As such, according to the authors, it is able to explore the motivations, attitudes, and values of this population in a way quite different than has previously been possible based on research with incarcerated persons who are perhaps chastened and are certainly both demonstrated failures and removed from the heated logic of contingency of freedom.

The accounts of the fieldwork do not recommend this sort of research to the faint of heart. As might be expected, the authors were suspected of being police agents by some of their violent interviewees. They also experienced some close direct encounters with violent events. The descriptions of the fieldwork are matter of fact and to the point, as is the rest of the book. Building on an earlier field study, the authors built a snowball sample through existing contacts. The final sample consisted primarily of black, poor, male practitioners of high-risk, low-yield street robbery, along with a few females and a handful of higher-level practitioners of commercial robbery.

Their chapters progress from initial motivations to commit robbery, through choosing of targets, and then to the mechanics of the act. Most sample members are described as living in a "street culture" based on "desperate partying." Though poor, the cycle of desperate partying leading to robbery for means to continue partying and back to robbery set them apart from other poor people.

Some of the most fascinating analyses concern the logic of choice of target. As might be predicted, the robbers preferred to prey on females and elderly people because they thought them more vulnerable. With regard to race of victim and geographical area, robbers of similar motivation and circumstance revealed quite different views. While some judged white victims to be wealthier and less likely to resist, others thought black victims more likely to be carrying cash. Similarly, areas outside the inner city were preferred by some for having more lucrative targets, while others preferred to operate close to home because they were less conspicuous in the local area and knew more escape routes.

The authors give careful consideration to theories about the moral and

sensual attractions of doing evil, but the weight of their evidence points to instrumental rather than expressive motivations among this group of offenders. While some described being thrilled by the sense of power, most committed robbery primarily for money. Few expressed regret, but few seemed primarily driven by inner compulsions to do harm or to dominate. The expressive side of their motivations was linked to their involvement in lifestyles of desperate partying and displays of contempt for the world of conformity in which their skills and education could allow access only to low wages and social marginality. Most were repeat offenders, many with prior experience and most with present expectation of future incarceration.

The authors claim in the introduction that their contact with active rather than incarcerated robbers will yield new insights for public policy. As the reader progresses through the book, it becomes difficult to imagine what these insights might be. The robbers portrayed here are indeed desperate and ruthless characters, mired in poverty and personal pathology, heedless of consequences. As such, they appear resistant to social programs, psychiatry, and criminal justice sanctions alike. Yet, this book is as coherent as it is concise. The policy prescriptions are specific and firmly grounded in the analysis. They include educating people in general not to display signs of vulnerability and not to resist if they are accosted, educating specific groups about specific dangers, such as johns exposing themselves to predatory prostitutes, and providing better banking services to poor people so that they do not have to carry cash in the street.

Within its stated objectives, this book succeeds admirably and seems destined to provide baseline data on its delimited field of inquiry for some time to come. The frustrations of reading this book result from the questions not addressed, best exemplified by the casual acceptance of the notion of "street culture." The descriptions in the book convince the reader that there is something real and specific indicated by this term. Indeed, street culture figures here as the master concept, the fount of most armed robbery. Its origins, however, and the conditions under which it reproduces, expands, or contracts are utterly outside the discussion.

Talking City Trouble: Interactional Vandalism, Social Inequality, and the "Urban Interaction Problem"¹

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This article uses ethnography and conversation analysis to pinpoint what "goes wrong" when certain so-called street people "harass" passersby. The technical properties of sidewalk encounters between particular black street men and middle-class white female residents of Greenwich Village are compared with interactions expected from studies of other conversation situations. The men attempt to initiate conversations and to deal with efforts to close them in ways that betray the practical ethics fundamental to all social interaction. In this way they undermine the requisites not just for "urbanism as a way of life," but the bases for how sociability generally proceeds. These acts of "interactional vandalism" both reflect and contribute to the larger structural conditions shaping the local scene.

In July 1996, New York City passed an antipanhandling statute in the context of other local initiatives to control so-called street people, including rapid response to even minor infractions of the law, city partnerships with private business associations to thwart unauthorized commercial street activity, and—as evidenced by certain interim reconstruction within Pennsylvania station—the architectural redesign of public spaces (Duneier 1999).² These were responses to widespread claims that, along

¹ We wish to thank Hakim Hasan, Gene Lerner, and Don Zimmerman for invaluable and detailed guidance; we are also grateful for the helpful comments of Deirdre Boden, Debbie Carr, Jane Collins, Eric Grodsky, Tess Hauser, Pam Oliver, Constance Penley, Jane Piliavin, Lincoln Quillian, Franklin D. Wilson, and Katherine Zippel. We also thank Mardi Kidwell and Michele Wakin for their consultation and support. Several *AJS* reviewers were especially helpful. Direct correspondence to Harvey Molotch, Department of Sociology, University of California, Santa Barbara, California 93106.

² These efforts embraced the concept of "community policing," gaining wide acceptance in New York as in other U.S. cities (Kelling and Coles 1996).

with such things as unsanitary streets, crime, and graffiti, interactions between street people and pedestrians were eroding the city's "quality of life." In the words of the New York City Council's legislative finding, the "increase in aggressive solicitation throughout the city has become extremely disturbing and disruptive to residents and businesses, and has contributed not only to the loss of access to and enjoyment of public places, but also to an enhanced sense of fear, intimidation and disorder" (NYC Council 1996).³

This article has four primary goals, all related to these evident tensions and the efforts they stimulated to reform New York's street life: (1) to identify the precise properties of interactions between certain street people and pedestrians that make them so problematic for participants; (2) to exhibit a research strategy through which they and other problematic interaction settings can be studied to determine why they are experienced as so troublesome; (3) to illustrate how microinteractions (talk on the streets, in our case) can help constitute larger social structural phenomena (like new police policies) just as those phenomena "come back" to constitute the nature of microinteraction—the micro-macro interface as a structuration process (Giddens 1984); (4) to use what has been learned to revisit the classic paradigm of urban interaction in light of our findings.

To accomplish these various aims, we "zoom in" on several very concrete interactions between a subset of street people and female pedestrians, utilizing the tools of conversation analysis, abetted through an ongoing ethnographic and interview study. We believe that whatever problems may be caused by the content of what gets said and by difference in class, race, and gender categories, there is something about the micromanagement of the encounters themselves that is essential to explain the felt difficulties of those "only" passing by.

Although our work focuses on one urban site, we do not regard the substantive problems or research issues as limited to the few blocks we study. During our five-year research period, statutes of the kind passed in New York also became law in Seattle, Atlanta, Cincinnati, Dallas, the District of Columbia, San Francisco, Santa Barbara, Long Beach, Philadelphia, New Haven, Raleigh, and Baltimore, among other places. An awkward tenseness in dealing with street people was massively reported not only in journalistic accounts but to us directly by our colleagues, friends, and associates. In describing a small number of interactions, we offer a conceptual and strategic framework for studying problematic interaction across a wider realm.

³ The Mayor's memorandum in support referred to the legislation as an effort to curb "inconvenience, annoyance, or alarm" (Office of the Mayor, City of New York, June 25, 1996).

SETTING

Our site is Manhattan's Greenwich Village, a place of contemporary extremes of wealth as well as marked ethnic and racial difference. Although this neighborhood, far more than most, is one of social variety, we center on what is plausibly the most loaded site of difference not only here but in U.S. society as a whole—encounters between black street men and middle-class white women.⁴ Our focus on relations among these two groups arose when initial ethnographic observations implied they were more problematic than any others, an impression confirmed by listening to many hours of tape involving interactants of different sorts. Our knowledge of the Village is based on extensive ethnographic fieldwork, primarily on three adjacent blocks along Sixth Avenue, from Eighth Street and Greenwich Avenue to Washington Place, over the period September 1992–October 1998, with daily observation from September 1992 to June 1993 and complete immersion during the summer months of 1996 and 1997 (for more detail, see Duneier [1999]).

These blocks of the Village are densely built-up with stores, bars, and restaurants, primarily in midrise buildings. Traffic is heavy, both on the streets and sidewalks. Adjoining sidestreets provide residence (as well as some additional commercial activity) at varying levels of density, from large apartment buildings to single-family townhouses. Some Villagers are very affluent (the median 1990 family income on the blocks we study was \$66,869).⁵ Property values are some of the highest in the United States; narrow rowhouses can cost upward of \$1 million, with condominiums sometimes in the same range. Partly because of New York's rent control laws, some units rent at rates within reach of middle-class tenants, at least those who can make do with the modest spaces they usually contain. Otherwise, this is an area that reflects the increasing social and economic bifurcation found in the United States, sometimes said to be especially marked in the global cities (see, e.g., Sassen 1991; Fainstein 1986; Mollenkopf and Castells 1992).

Not captured by any standard sociodemographic profile is the Village's cultural tradition, well known across the world as arty, bohemian, and free thinking. As one continuing manifestation, it has come to hold the city's highest concentration of gay men. In part because of these distinctive traits (and the kind of retailing and semiotic features that accompany them), it is also a visitor destination for people coming from other parts

⁴ From slave times forward through Southern lynching and contemporary urban white anxieties, the black male fills the position of sexual threat and mythic "true brute" (Gardner 1995, p. 116; see also, e.g., Hodes 1998; Blauner 1989).

⁵ These data correspond to the adjacent census block group, 006500-1, which has 2,018 residents, 6.6% of whom are African-American and 3.1%, Hispanic.

of the world or other neighborhoods of the city.⁶ Troubles based on race and class here clearly cannot be attributed to an especially reactionary group of people; instead any such difficulties arise under more or less "best case" conditions.

Part of the Village's heritage is also a prior history of ethnographic description (Ware [1935] 1994), especially Jane Jacobs's late 1950s observations of Village social interaction as representing the ideal for what makes up a "great" American city (Jacobs 1961). Jacobs described the Village sidewalks as having much convivial contact among acquaintances and even strangers, but within a context of mutual respect for appropriate limits on interaction and intimacy. "Cities are full of people with whom, from your viewpoint, or mine, or any other individual's, a certain degree of contact is useful or enjoyable; but you do not want them in your hair. And they do not want you in their hair either" (Jacobs 1961, p. 56).

For Jacobs this balance of contact and separation made for interactive pleasantness, adding up to "an almost unconscious assumption of general street support when the chips are down" (Jacobs 1961, p. 56). Because of the presumed good intentions ratified by the experience of even minor interchanges, the Village's "eyes and ears on the street," in her famous dictum, indicated they were safe and thereby produced safety in fact. There was a choreography based in Goffmanian "civil inattention"—acts of glance and gesture that provide the benefit of recognition while simultaneously "conveying that the other was of no special threat" (see Gardner 1995, p. 92; also Anderson 1990; Lofland 1973, p. 140). There was a system of trust.

However, as much as today's Village may still contain people living as these depictions suggest, certain changes have taken place stemming from the fact that there is another, less venerated Village, which likely did not exist in Jacobs's day. She does not report homelessness, begging, or racial tension (although ethnic tension is described), perhaps because racial segregation and well-policed skid row areas then kept the marginal more at bay (Bittner 1990; Wiseman 1970).⁷

In 1997, there were a total of 31 vendors, scavengers, and panhandlers

⁶ Many have documented the Village's distinctiveness (e.g., Banes 1993; Beard and Berlowitz 1993; McDarragh and McDarragh 1996).

⁷ But there were many white ethnics working as vendors on the Lower East Side, and panhandlers had been a mainstay of city life during the Depression (although none of them are reported in, e.g., Ware's Village study). Ware does make passing reference to beggars and homelessness in the case records of social workers, but not as a problem in public space (her book is Depression era). In terms of gender, young men were expected, according to Ware (p. 147) to "chase women" and engage in the "Tom-cat code" of local Italian culture. Both Ware's and Jacobs's work precede feminist problematization of gender and public space:

on our blocks,⁸ almost all of whom were African-American men. Taking advantage of a New York City ordinance that makes special allowance for the street sale of printed matter,⁹ some of the street people sell books (mainly used) and magazines from tables they set up on the sidewalk. Contrary to stereotype, some of these vendors are drug free and have apartments, a status akin to conventional citizens who support families with the proceeds of their work. Lower down, in terms of the status street people accord one another, are scavengers who work as "hunters," supplying the vendors with the goods gathered from dumpsters and recycling bins. Still lower in the hierarchy are panhandlers. Many of the scavengers and panhandlers, as well as some of the magazine vendors, are excessive drug users or alcoholics who sleep on the same sidewalks where they spend their waking hours or in nearby doorways or in the subways. This is a rough guide only, and some of the people have multiple roles at the same time or shift specialty over time—experiencing both upward and downward mobility in terms of ways of making money, substance dependence, and homelessness.

Although on various blocks of the Village there are some white men working as scavengers and a few women of color who live as vendors or panhandlers, none in our experience are white women. White women on Village sidewalks are heading for a destination; if they are operating in a more leisurely manner, they are taking a walk or walking a dog. They are not on the sidewalk to make their lives. Our two protagonist groups come into direct contact because of the niche the white women fill in the street men's routine as potential customers or sources of spare change. In the possibility of talking with them (or at them), they are also vehicles for helping pass the time, an ongoing challenge, says Liebow (1993), for those without regular employment or residence.

METHODS

Access to sidewalk interactions and entree to the local street scene in general began when one of the authors—Mitchell Duneier ("Mitch")—lived in the area and became a frequent customer of the vendors. Mitch is a white man in his midthirties. Through slowly built up relations, these vendors introduced him to their scavenger-suppliers; these relations then led to panhandlers. Once Mitch was in the network, contacts and introductions took place, almost always cordial, across the various spheres.

⁸ Because people come and go from the scene at various times of day and night, only about a third of this total is visible on the streets at any given time.

⁹ New York City Law no. 33 (1982).

Mitch became a general assistant to the street vendors, sometimes watching their merchandise while they went on errands, occasionally also buying up merchandise offered in their absence, and assisting on scavenging missions. He also performed such favors as going for coffee. He eventually worked for two full summers as a scavenger and vendor. He was treated as a routine fixture of the blocks, occasionally referred to as a "scholar" or "professor"—but overwhelmingly as another street character known only by a single name, with no title or family name. His role and designation was "Mitch."

Mitch carried out approximately 30 interviews—instances when he explicitly requested an informant to tell their story. These tape-recorded sessions, held on street corners, in coffeeshops, on the floors at subway landings, lasted between three and six hours; Mitch paid his informants \$50 on a one-time basis. But most of the data, including all data detailed in this article, came from Mitch's continuous participant-observation among the people of the street, including their interactions with passersby. During the last two years of the study, Mitch also taped ordinary conversations, usually keeping the recorder running on a milk crate under his or a street colleague's vending table.¹⁰

Because Mitch lived in the area, he knew many residents in addition to the street people. These people—among them many white women—were continuous informants. But Mitch also managed, on several occasions, to interview the very women whose interactions with the street men he previously had observed. This was done either by catching up with the women after they passed the scene or simply waiting for them to again pass by so that an interview could be arranged at a different time. Through one such effort, an interview was gained with a woman whose conversation figures large in this report (referred to below as "Laura").

Not participating in the fieldwork directly, the second author, Molotch (also a highly educated white man) has had some running familiarity with the area based on having lived there during Jacobs's era (in 1963, two blocks from the main corner in the present study). He has returned several dozen times over the years for brief visits to be with friends who have maintained residence near these blocks. During the study period, he developed an ongoing relationship with one of the vendors on the streets

¹⁰ Street people were told of the taping and after a period of weeks seldom behaved in ways different than at prior times. Even on tape, the men trusted the researcher with embarrassing details of their lives as well as of acts that were either illegal or could have resulted in violence toward them from others. Taped and written records of any such details have now been destroyed. For additional methodological discussion regarding the taping process as well as general issues of reflexivity and mutual awareness of the research activity, see Duneier (1999).

through repeated meetings, shared meals, and phone conversations, along with more casual contacts with some of the other men. He was present on the block on the day of one of the two primary interaction stretches analyzed in this paper.

Studies of troubled public interactions have tended to rely on postevent interviews (Gardner 1995; Feagin 1991) or reconstructions based on a combination of interviews and observation (e.g., Suttles 1968; Anderson 1990). Motivated by a desire to go beyond our own impressionistic accounts and interview data, we moved toward conversation analysis as a method. Subjecting talk to the technique of conversation analysis requires capturing virtually every aspect for later examination, including split second silences and small utterances like "umm," "oh," and "uh" as well as interruptions and overlaps (as when two people speak at the same time). Also, stretches of talk should be long enough to permit the analyst to determine the meaning of a given feature of talk in light of prior or subsequent utterances in the same stream. These conditions can be difficult to achieve under even ordinary conditions; with honking horns, sirens, yelling, and other background noise, the streets present an extreme challenge. Complicating matters, street people move about, talk away from a microphone, and, especially if eating, drinking, or under the influence of drugs or alcohol, do not speak clearly enough for accurate transcription. Likewise, pedestrians are in motion and, often trying to avoid street people, talk away from a microphone or slur words meant more to evade talk than to convey substance.

Although we have recorded hundreds of hours on the streets, the two primary stretches that we here present are among the few instances of data technically good enough for our full purpose. We think of these finds among our stacks of tape as akin to fossil fragments that, although a small sample indeed, can yield considerably more than casual clues to the way life is constructed.¹¹ Schegloff (1987) has provided a full-scale demonstration of the payoff of even a single episode of conversation. Conversation analysts have now accumulated a large technical knowledge base of how people pattern their talk under relatively unproblematic conditions. The key principle is that in everyday talk parties keenly attune to the cues offered by the other, mutually seeking and finding the appropriate moments and methods to begin a conversation, take turns within it, or facilitate its ending. Conversation analysis strives to determine, through close

¹¹ We know of no other successful efforts at tape recording such spontaneous interactions involving street people. For other instances of "small sample" analyses, see Schegloff (1987), Whalen, Zimmerman, and Whalen (1988), and in a very different realm, Barker and Wright's (1951) study of one boy's day.

(even split second) empirical observation, precisely how individuals, through mutual awareness of unfolding conversational trajectories, manage to talk with one another. Besides the original formulation (Sacks 1992) and successor summaries (Heritage 1984), a more recent presentation by Schegloff (1996) in the pages of this journal has painstakingly laid out a concise statement of the basic precepts of conversation analysis and of how it contrasts and at least potentially complements more traditional work in the discipline.

Without repeating Schegloff's exegesis yet again, we note that the conversation analysis program consists of "noticings" of initially unremarkable features" of talk and conduct, "unmotivated" by any political or theoretical goals of citizens or conventional sociology (Schegloff 1996, p. 172). We too make use of these "noticings," but in contrast to basic scholarship in conversation analysis, we *are* motivated by a substantive set of concerns: how in the process of deploying the procedures described in conversation analysis, people make trouble in the streets. The talk carries great tension—tension we think we can find in its formal properties as defined by conversation analysis. Thus, we use what conversation analysis has already learned, trading on its specific "discoveries," as Schegloff (1996, p. 174) appropriately terms them. We are doing a kind of sociological and policy-oriented *applied* conversation analysis.¹²

Most of the data used so far in conversation analysis comes from taped phone conversations, videorecordings of family and work settings, and some institutional milieus such as court proceedings (rather than sidewalks). But the findings have been sufficiently robust to hold even across diverse language and national groupings—besides the languages of western Europe—Japanese (Lerner and Takagi, 1998), as well as Farsi and six other Indo-European languages (Boden 1983). We thus infer, given the diversity of the base, that conversation analysis (or CA) provides an appropriate template against which to look for distinctive attributes of the conversations we examine. We will compare the technical properties of our street conversations with CA findings of how talk ordinarily operates. We draw upon only a few of the most replicated and straightforward findings from CA scholars to do this. We contrast our actual findings with "expected" findings, with the difference revealing the distinctive nature of these street interactions. This provides for a quasi-experimental design otherwise difficult to approximate in ethnographic method.

¹² As CA scholars gain an "understanding of how interaction works, and based on this understanding, it then may be possible to examine distributions of phenomena in terms of structural or other identities," as Maynard (1988, p. 317) says. See his essay for examples of work in which such an approach appears at least implicit.

OUR PROBLEMATIC INTERACTIONS

It is noteworthy that, given their prevalence in discourse in the Village and across the city, only a small fraction of interactions seemed to involve the kind of problems complained of by so many. We found that, on our three blocks, only three of the 21 "regulars" caused problems and even they would do so for only a minority of the moments they were on the streets. They are Mudrick, Keith, and Butterroll,¹³ three of the homeless panhandlers who also occasionally work as assistants to magazine scavengers and vendors. While our analysis here centers on their behaviors that passersby tend to find objectionable, it cannot be overemphasized that at other times on the sidewalk (and in other parts of their lives) each of these men would be seen as acting in positive and straightforward ways toward others, including the women in their lives—girlfriends, mothers, and granddaughters as well as general passersby. When they do act problematically, the other men on the block regard their behavior as deviant and detrimental to their entrepreneurial activity.¹⁴ They tell the three such things as "shut up," "stop messing with my business," or "you giving the rest of us a bad name." Usually the behavior persists despite the entreaties to stop (one of the vendors, in response, left the area to find a space where he could be alone).

We present various examples of talk involving the three men. In formal CA terms, these episodes revolve around fundamental aspects of any conversation—opening them and closing them.¹⁵ Doing the kind of coordination involved in accomplishing openings and closings is prerequisite for any conversation and hence for urban civility in general. For two stretches of talk we present, involving Mudrick and Keith, there is both enough auditory detail to permit CA treatment and also sufficient length to display how sequences unfold, including the way different types of participants pursue distinctive strategies over time. For Butterroll's talk, we have shorter sequences that, while ancillary, further display problems in opening a conversation.

Based on the examples that follow, we study the mechanisms behind (1) problematic openings in which street men make efforts to open conversations that are not reciprocated by women passing by even when the men persist and (2) problematic closings in which the women try to close

¹³ Except as stated in n. 22 below, all names in this article are used by the participants themselves in real life. Each has been read the contents that refers to him, and each gave consent.

¹⁴ We use the term "deviant" advisedly, based on the labeling done by the confreres on the street.

¹⁵ One of the significant books presaging advances in discourse analysis was entitled *Opening and Closing* (Klapp 1978).

conversations, but the men do not respond in a way appropriate for ending the exchange even as the women persist.

Our main analyses turn out to rest on four different forms of data derived from the same interaction streams. We have, at least in regard to Mudrick and Keith (1) researcher's memories and notes from first observing the conversations on the street, (2) voice tapes (which we listened to repeatedly), (3) conventional transcriptions of those tapes, (4) detailed CA transcriptions, made for us by Mardi Kidwell, a person trained in the technical conventions of conversation analysis (see Sacks, Schegloff, and Jefferson 1974).

For Mudrick and Keith, our presentation strategy is to first display and analyze our conventional transcription and then represent the same material using CA notations, pointing out the kind of knowledge gained with the fuller treatment. Only by recourse to this level of detail, as will become evident, can the nature and full force of the sidewalk tension be appreciated. All through, we supplement conversation analysis with our ethnographic understandings of the street—body language, the physical setting, individual histories, as well as past events known to us. In this way, we take the liberty to merge the two distinct research orientations, using CA to enrich the more conventional sociological ethnography (rather than the other way around)—an appropriate stance given our ultimate analytic target of a recognized social problem.¹⁶

FAILED OPENINGS: QUESTIONS AND COMPLIMENTS TO WOMEN

A common genre of street talk involves men calling out to women who do not respond. These are hence not really conversations at all, even though talk occurs. In various settings, most famously the edge of construction sites, men, sometimes handing off turns to one another as a "team performance" (Gardner 1995, p. 106), evidently earn esteem of other men in earshot.¹⁷ Gardner (1995, p. 146) calls attention to these types of interaction as something women frequently experience as abusive; they signal, perhaps, the oft claimed fact of male privilege in public spaces, enforced at the extreme through women's physical vulnerability and the omnipresent threat of rape (Bart and O'Brien 1985; Stanko 1990; see also Spain 1992; Hayden 1981, 1995). We note, however, that these calls are often interpret-

¹⁶ We take the Schutzian natural attitude as fundamental and privileged not only for making mundane reality (see Pollner 1987; Cicourel 1970), but also for making "findings" about that reality, as in the practice of sociology we are here conducting.

¹⁷ Goffman (1963, p. 63) refers to women serving as "open persons," who can be addressed without conventional opening devices; African-Americans, gays, and children are also such persons—depending on time and circumstance.

able as “merely” questions (“Where ya goin, honey?”) or compliments (“nice legs”) and in content not necessarily even hostile (“Are you married?”). But a formal indicator, from within the talk itself that these are not taken as “innocent questions” and that something is indeed “going wrong,” is that the women tend not to respond. Nor do they, as people do in ordinary talk, ask for a repeat of the question or for a clarification. They are not interested in responding. This is all striking given CA findings that questions ordinarily induce some kind of answer, requests produce a declination or are granted, compliments are an engine for acknowledgments. These expected couplets are examples of what conversation analysts call “adjacency pairs”; they massively recur (Schegloff and Sacks 1973; Sacks et al. 1974). Generally, withholding a second part of such a pairing is noticed, something evident in the participant’s tendency to repeat their first part if the second does not follow. To not respond at all is notably disaffiliative.¹⁸ For men to keep asking, when knowing “perfectly well” they have been heard, is—in our interpretation—a kind of disaffiliative escalation.

Following is a stream of examples of questions and compliments from Mudrick that gain no response, including a pursuit in which he explicitly states he knows he was heard “the first time.” Mudrick, approximately 57 years old, with a history of alcoholism, scavenges and occasionally sells printed matter on the streets. This verbatim transcript, with nothing left out of the stretch, comes from talk taped as the researcher, Mitch, and Mudrick were left to mind the table of used magazines and books for one of the vendors who had gone on an errand. Although Mudrick is temporarily “in business” (and hence stationary enough to be taped) he does not alter the kind of patter we have often seen him engage in with women passersby when panhandling or merely passing some time. He begins this interaction as a white woman who looks about 25 approaches at a steady pace:

1 Mudrick: I love you baby.

She crosses her arms and quickens her walk, ignoring the comment.

2 Mudrick: Marry me.

Next, it is two white women, also probably in their midtwenties:

3 Mudrick: Hi girls, you all look very nice today. You have some money? Buy some books.

They ignore him. Next, it is a young black woman.

¹⁸ Speaking of the “maintenance of the relevant forms and observances,” Heritage comments, “it is deviance from these institutionalized designs which is the inferentially rich, morally accountable, face-threatening and sanctionable form of action” (1984, p. 268).

4 Mudrick: Hey pretty. Hey pretty.
She keeps walking without acknowledging him.

5 Mudrick: 'Scuse me. 'Scuse me. I know you hear me.
Then he addresses a white woman in her thirties.

6 Mudrick: I'm watching you. You look nice, you know.
She ignores him.

7 Mudrick (to Mitch): She was looking dead at me. You could see
it. They be watching you. Most ladies, they be waiting
for you to say something to them.

Three white women in their twenties approach.

8 Mudrick: Hi ladies. How you all feeling, ladies. You all look very
nice, you know. Have a nice day.

The women ignore Mudrick. Thinking that Mudrick's approach is not
"good for business" prompts a question.

9 Mitch: Let me ask you a question, Mudrick. When you do that,
explain to me the pleasure you get out of it.

10 Mudrick: I get a good kick out of it.

11 Mitch: Explain to me the kick you get out of it.

12 Mudrick: It make me feel good and I try to make them happy, the
things I say to them, you understand? The things I say,
they can't accept. They gotta deal with it.

Sometimes Mudrick, as with other street men, does gain some small re-
sponse from the women. Here's an example of that "success," achieved
on a woman Mudrick was able to begin speaking with just before she
reached the vendors' tables:

1 Mudrick: Hey pretty.

2 Woman: Hi how you doin.

3 Mudrick: You alright?

4 Mudrick: You look very nice you know. I like how you have your
hair pinned.

5 Mudrick: You married?

6 Woman: Yeah.

7 Mudrick: Huh?

8 Woman: Yeah.

9 Mudrick: Where the rings at?

10 Woman: I have it home.

11 Mudrick: Y' have it home?

12 Woman: Yeah.

13 Mudrick: Can I get your name?

14 Mudrick: My name is Mudrick, what's yours?

She does not answer and walks on.

In this sequence Mudrick used all nine of his turns (out of a total of 14 turns for both of them) as strategy to initiate conversation. Every one of his utterances is designed to sustain talk in that it provides the first part of what would ordinarily be an adjacency pair. He asks questions of different forms at lines 3, 5, 7, 9, 11, 13, and 14 and pays three compliments (one on line 1, two in line 4). In fact, some of these turns have forms that have been previously noted by CA researchers as quite specific ways to generate responses. Hence, the "huh?" on line 7 is a request for clarification ("request for repair" in CA terminology), line 11's "Y' have it home?" is a request for a confirmation; asking for the woman's name in line 13 is another request and the asking again for the same information on line 14 is a "pursuit." In sequential terms, this represents a pattern of increasingly strong efforts to generate a response.

In substantive particulars, Mudrick's line of questioning is especially artful because in asking the woman whether or not she is married (line 5), he gives her a question she will likely want to answer. He evidently knows that, as reported by Gardner (1995, p. 131), an item of information that "may be manipulated by the woman to her advantage" is to inform a strange man that "she is married or has a boy friend," and hence is not a candidate for further talk much less romantic liaison. But since Mudrick is pursuing a conversation rather than a date, the question works for him because of its special likelihood of gaining an answer—which it does. Since he sees no wedding ring (maybe he saw she wore none even before asking his question), he has her trapped in a contradiction, one she feels compelled to then explain ("I have it home"). So Mudrick has her on the edge of a "real" conversation (two responses) through his savvy crafting of talk content as well as form.

Otherwise, the woman gives no indication of a desire to talk. Mudrick's opener on line 1 ("Hey pretty") contains, in CA terms, a summons as well as a compliment and it brings the only response that could possibly be interpreted as a question (but see below) from the woman (her "Hi how you doin"). The woman offers no summons, compliment, question, or other talk that implies she wants to interact. From Mudrick's standpoint, he got some talk but under circumstances—now becoming visible to us in both substantive and technical terms—that most would find demeaning. From the woman's standpoint, Mudrick ignores her cues that she does not want to talk, ordinarily evident when one does not reciprocate by also asking questions, offering compliments, or using other techniques to display a desire to carry on. The two individuals are actively failing to collaborate in accomplishing a "normal" interaction.

Even in the face of the woman's disaffiliative moves, Mudrick performs the substantively radical action of asking for her name, a crucial bit of

what Goffman calls "access information" that women understand may enable a strange man to learn one's phone number, address, workplace, or other personal routines (see Gardner 1995, p. 131). Not hearing an immediate reply, he bolsters his request by offering his own name (line 14); something that, in CA terms, can be a way to simultaneously ask for the other person's name (Sacks 1992, 1:3, 4). But not chancing it will work on its own, he asks explicitly—and in effect, "again," with the question "What's yours?"—another pursuit. Under most circumstances, one would read the disaffiliative features of this woman's turns to indicate she would never provide something so intimate as her name. Coming as it does in this place in the stream of technical displays, it is indeed a question "out of place."

By asking for the woman's name in the face of her prior cues, Mudrick aggressively displays he pays no allegiance to the way the woman has painstakingly designed the timing and sequencing of her responses (or lack of them). At the heart of Mudrick's incivility was his betrayal of the underlying tacit conventions of sociability. We refer to this kind of impropriety as "technical rudeness" to distinguish it from offenses to substantive norms and rules (which are things named, discussed, and legislated).¹⁹ For her part, the woman—as with prior women Mudrick talked to—is also technically rude by her nonresponse to his repeated "requests" for talk.

Evidence of the asymmetry in these two people's desire to talk is actually stronger than the transcript indicates because the timing of the woman's responses provides additional display, for Mudrick as for us, of her reluctance. People know how to read and deploy timings in extremely precise ways; three-tenths of a second delay is more than enough to signal a conversational event to interactants as well as to the analyst (e.g., that a compliment was not heard, was misunderstood, or is being ignored). Similarly, people know that a silence following their last utterance may well signal "there is nothing more to say" and is hence a move toward closure.

By presenting the same conversation using the transcribing conventions of conversation analysis, we can see the timing (as well as other features of interest to specialists). Notice, in the following stretch, that the woman delays her responses (when she offers them), but Mudrick speaks immedi-

¹⁹ CA scholars do not classify behaviors as being such things as "rude" or "not rude"; these are folk categories that people invoke as opposed to formal properties of turn taking. Our aim is to show a connection between the way individuals substantively experience encounters and the CA-identified formal properties of those interactions. "Technical rudeness," a term for which we thank Michele Wakin for suggesting, identifies a variant of that connection.

ately following her turns—evidence again, of his desire to keep the conversation going in contrast to the woman's desire to close. Even asking a question of the sort ordinarily designed to get a quick affirmative ("you alright?" line 4) gets Mudrick not an answer, but a full 2.2 second silence that he must end by taking still another turn (line 6). As for the woman's one "question" (Hi, how you doin?), it does not, when heard on tape, have the rising inflection associated with requests or inquiries, but is instead spoken in the manner of a closure, a "greeting substitute" that proposes to end the interaction. Similarly, when the woman says her ring is at home (line 18), she hits the word "home" with an intonation intended to close the subject, not continue it.²⁰ Here it is again, this time in CA notation (numbers in parentheses indicate silences in tenths of a second; brackets like "[" mark places where a turn is overlapped, as in a very quick response, by the next turn below it):

- 1 Mudrick: Hey pretty.
- 2 (0.8)
- 3 Woman: Hi how you doin[.
- 4 Mudrick: [You alright?
- 5 (2.2)
- 6 Mudrick: You look very nice you know. I like how you
- 7 have your hair (pinned)
- 8 (0.8)
- 9 () (O↑h↓)()(said in high pitched voice)) ((tape recorder shakes))
- 10 Mudrick: You married?
- 11 Woman: Yeah.
- 12 (.1)
- 13 Mudrick: Huh?
- 14 (.)
- 15 Woman: Yea[:h
- 16 Mudrick: [() where the rings at.
- 17 (0.5)
- 18 Woman: I have it ho:↑me↓
- 19 Mudrick: (Y') have it home?
- 20 Woman: Yeah.
- 21 Mudrick: Can I get your name?
- 22 (.)
- 23 Mudrick: My name is Mudrick what's yours.

²⁰ As she says the word ("ho:↑me↓"), she drags (":") her emphasis on the "ho" with an upward inflection ("↑") to the "me," said with a downward inflection ("↓"); this gives the pronouncement a tone used to end a contentious "debate," as in resting one's case.

The line of talk Mudrick uses is similar to Butterroll's (but Butterroll's occurs only in short snatches). Butterroll, 42 years old, is an alcoholic who has spent the last 10 years of his nights in the subways, either at the bottom level, which the men call "the dungeon" or the intermediate level, the "bat cave." The others regard Butterroll as the most aggressive of all the panhandlers on Sixth Avenue, an opinion which jibes with our own. On at least two occasions during the summer of 1998, his behavior attracted sufficient police interest to bring him jail time (once for 10 days) for violating the city's ordinance against aggressive panhandling.

Butterroll, sitting on a crate, rhythmically shakes his coin cup as he greets women coming out of the subway (all are white women in these examples). They ignore him with silence after each of his "turns":

- 1 Butterroll: Hello, doll baby.
- 2 Butterroll: You like that?
- 3 Butterroll: Bullshit!

The woman walks on.

Here Butterroll makes explicit what is otherwise implicit in such interactions: he wants to talk (again, a white woman's silence follows each of his utterances).

- 1 Hello.
- 2 You might as well speak.
- 3 I like you too.

And

- 1 Hello, doll baby.

The woman apparently grimaces.

- 2 I only spoke!
- 3 At least you can speak.
- 4 And maybe one of these days! Yeah?
- 5 (He laughs) You know what I'm talking about.
- 6 Never say never, baby.

Once again we see women confronted by a man who continues in pursuit in the face of disaffiliative silences. In terms of content and also true to type, the proposed greetings, question, and compliment ("doll baby") carry sexual innuendo.

We know that "streetwise" middle-class people, women and men alike and across racial categories, do develop skills—"the art of avoidance," as Anderson (1990, p. 209) terms them—to deal with their felt vulnerability toward violence and crime. According to Anderson, whites who are not streetwise do not recognize the difference between different kinds of black

men (e.g., middle-class youths vs. gang members). They may also not know how to alter the number of paces to walk behind a "suspicious" person or how to bypass "bad blocks" at various times of day (Anderson 1990, p. 231). But we argue that even those who are streetwise in all these ways still run into forms of copresence they would rather not have. In our terms, distressing disaffiliative displays can come even while no one substantively opposes, threatens, or reprimands. Yelling and profanity—which do sometimes occur on the blocks (from both sides)²¹—is far less frequent than technical rudeness. Criminal acts of violence or theft toward passersby under these "broad daylight" conditions is extremely rare: police data indicate the neighborhood (sixth precinct) has one of the lowest crime rates in the city. So much of the trouble is at another level.

THWARTED CLOSINGS, ENTANGLING THE WOMEN

We have indicated how women resist invitations to open and how men ignore their resistance. Because the women generally succeed in avoiding such talk, it is rare to find instances when any kind of "real" conversation takes place. But our stretch of talk involving Keith is such an instance. As such it allows us to focus on how the men may not respond to women's cues for closure of conversations once they are under way. Keith accomplishes this conversation with the use of other parties—a dog and Mitch. This gives us a mixed case of turns, in which "disinterested" parties provide us further clues to how things work. The dog and the researcher function as perturbations which, in the way they are handled by the protagonists (differently than how they handle one another), further reveals the kind of struggles taking place.

Pets, as also in the case of small children, figure into social strategies; walking the dog can convivially attract interactions (Messent 1985; Adell-Bath et al. 1979; Robins, Sanders, and Cahill 1991). In the Village as elsewhere, strangers pet one another's animals or ask of a toddler's identity or habits ("Oh, is this your grandson?"). Gardner (1995, p. 93) reports in her Indianapolis observations that "stranger etiquette" permits someone with a dog or a child to be "approached at will." Perhaps to the advantage of both parties, the dog or child serves to modulate intimacy, permitting, in effect, indirect communication via the dependent creature. A pet serves as social "conduit," say Robins et al. (1991, p. 22), a conclusion based on their study of dog owners' interactions in a city park. People say things to other people's dogs that can make no sense to dogs and ask questions

²¹ An occasional woman passerby will respond with angry profanity, sometimes generating a similar outburst from the man as she moves out of shouting distance.

dogs can not answer ("Was it too cold for you, little guy? Poor baby"; Robins et al. 1991, p. 15). But "by addressing the dog instead of the person, regulars avoided greeting and saying good-bye to a person they did not really know nor perhaps were not ready to know." In ongoing interactions, the parties can engage in exploratory, low-risk parries with each demonstrating their degree of commitment without needing to insult the other when reaching their interactional limits.

But dogs (and children) can also induce (and sustain) unwanted interaction. Butterroll, for example, directs remarks to dogs as he panhandles its white woman owner

- 1 Look at that dog.
- 2 Oh, she's glamorous.
- 3 I love you baby.

The woman and dog walk on.

A small child is crying as she walks by with her mother; Butterroll keeps shaking his cup as he says (with no prior opener) "toward" the mother:

- 1 Mommy's right.
- 2 Don't give Mommy a hard time.
- 3 She's right.

While each of Butterroll's lines is met with silence (no matter if the woman has a dog, a child, or is alone), our talk stretch involving Keith follows a very different course. Keith, 42 years old and an alcoholic who suffers from health problems with one of his legs, sleeps on a bed of cardboard on a building roof. Keith and Mitch were minding two adjacent tables for one of the magazine vendors who had gone off to place a wager at a nearby betting parlor. They were sitting on milk crates adjacent to the tables with their backs to the street and their bodies oriented to the sidewalk. A white woman named Laura, whom we later learned through interview was 34 years old and living in a nearby small apartment,²² walks her pug dog, Dottie, down busy Sixth Avenue. She is a graduate of an elite woman's college, has lived in the Village seven years, and works as a secretary to a venture capitalist. At the sight of Keith and Mitch, she straightens her back and looks dead ahead. As she passes Keith's table, he interrupts his conversation with Mitch to call the dog by name. Street men often know the names of neighborhood dogs (and babies and toddlers) and routinely call out to them:

²² We gained this interview only after seeing how useful the taped interchange with her was going to be; we were forced to wait almost a year before her return to the scene coincided with Mitch's presence at the tables. "Laura" and "Dottie" are pseudonyms.

- 1 Keith: Hold on, I gotta talk to my baby.
2 Keith: Come here, Dottie

Dottie goes toward Keith.

- 3 Mitch: Hi, Dottie. This is Dottie, right?
4 Laura: Yes.
5 Keith: Miss.

Pulling on the leash, the dog tugs her owner over to Keith who begins playing with Dottie.

- 6 Keith: Sit down.

The dog sits as Laura, the dog's owner stands by looking distracted, pulling on the leash. As Keith pets the dog he says to Mitch:

- 7 Keith: One o' my babies. No, no . . . pushy woman (in jest, looking down toward the dog jumping on him), kiss me. OK. Get off me, get off me. Oh that's better, Look at 'er, she's a laughin' or somethin'.
8 Mitch: How old is Dottie?
9 Laura: Eight months.
10 Keith: She see her boyfriend (referring to another dog nearby). She likes younger men.
11 Laura: They ran into each other tonight for the first time.
12 Keith: An' he's big as a ox (referring to the "boyfriend").
13 Laura: She looks very happy (referring to Dottie).
14 Keith: He's big as a ox (referring to the "boyfriend").
15 Mitch: Really?
16 Keith: He's about five months old.
17 Laura: Six.
18 Keith: Six. He's like this much bigger than her owner (referring to the owner of the "boyfriend"). She's like, "Ohh, what a man."
19 Laura: She's a pioneer. She's out there.
20 Keith: Always.
21 Laura: Yes.
22 Keith: Well she can't make me no granpa. That's my baby here (referring to Dottie).
22 Laura: Come on. Dottie Dog. ((clapping)) Hey! Come.
24 Keith: Drop the leash for a minute, walk away I wanna see what happens.
25 Laura: No, you know I can't. I'm not gonna drop my leash.

swer; it comes so fast it is a virtual continuation of Mitch's turn). This interaction event between Mitch and Laura displays, within this otherwise disaffiliative stretch, the common pattern found in the CA literature for performing an affiliative gesture.

Keith's control over the dog introduces a certain parity into his relationship with Laura. In the following sequence, Keith talks to the dog who is alternately in his arms, on his lap, or otherwise in his control. In calling her "Miss," and in subsequent lines (4–18), Keith does all the talking, either speaking to the dog, "for" the dog, or referring to the dog. In the 14.2 seconds that pass from line 1 to line 18, the woman says nothing (no chuckle, nor an "oh" nor an "uhm" that people often inject between sentences; see Schegloff 1981). She fails to participate either as a stand-in for the dog (e.g., expressing its feelings) or as an "audience" appreciating the pleasure her dog could be seen as gaining from Keith's fondling. The animal's presence as a "conversation piece" generates minimal talk from Laura compared to what dogs make possible.

Keith's control over the dog allows him to tolerate many silences (at lines 4, 9, 11, 13, 15, 18), relaxing his need to fill in with a banter of compliments and questions that otherwise would appear necessary to delay the woman's departure (recall Mudrick's contrasting rapid fire). Here is the CA version (with arrows highlighting silences):

- 4 (0.8)
- 5 Keith: Miss
- 6 (0.2)
- 7 ((car alarm goes off))
- 8 Keith: One o' my BABIES=NO(.) no.
- 9 (1.0)
- 10 Keith: (pushy woman/kiss me)
- 11 (0.5)
- 12 Keith: OK. Get off me (.) GET OFF me::
- 13 (2.2)
- 14 Keith: Oh that's better?
- 15 (3.0)
- 16 (Look at 'er)
- 17 (She's a laughin') in 'er () or somethin'
- 18 (3.0)

For his part, Mitch felt the woman's silences and the imposed constraints on her movement as painfully awkward—an experiential symptom, we believe, of technical conversational conventions being ignored to entrap the listener. The last silence, at line 18, is especially long—truly long enough to signal a disinterest in continuing the conversation (Jefferson 1989). The awkwardness of the moment (Keith is not saying "good-

bye" and the woman is not able to) is the interpretation that Mitch gives, in retrospect, for his coming to Keith's aid (line 19) by asking the woman a question of his own. Predictably, the woman again responds readily to Mitch:

19 Mitch: How old is Dottie?

20 Laura: = Eight months.

The couplet goes smoothly with zero silence between turns (indicated by "="); Laura's voice evidences a positive tone. This again demonstrates that her problems are with Keith in this interaction, not with strangers in general.

Much of the conversation that follows (lines 21–35), including the exchanges between Keith and Laura, is an artifact of Mitch's interchange with the dog owner that has given new life, along with Keith's hold over the dog, to the conversation. But when Laura does volunteer a rare comment to Keith, it is a substantive disagreement. As she offers up the information that the dogs had not met before, she contradicts Keith's statement that the nearby dog is Dottie's "boyfriend" (22–24). Notice that her disagreement comes even faster than immediately, actually overlapping Keith's prior turn (note the transcript brackets). Whereas people frequently overlap when agreeing with one another, they hold off their response when it is going to be a negative by providing a silence or creating a delay with utterances like "well," "umm," or "gee" (Jefferson 1973; Pommerantz 1984). Laura's use of an overlap to deliver a negative is a strongly disaffiliative move—technically rude in our terminology. Her only other comment to Keith (29, 30), correcting his estimate of the dog's age, is again a kind of contradiction, albeit softer than before, and again it contains no cushioning delay.

- 21 (1.0)
 → 22 Keith: >Sh see her boyfriend (0.1) She likes youn[ger men.
 → 23 Laura: [They ran
 24 into each other tonight for the first time
 25 Keith: An' he's big as a ox=
 26 Laura: =(She looks) very happy.
 27 Keith: He's big as a ox=
 28 Mitch: Really?
 → 29 Keith: He's about five months old?
 → 30 Laura: Six
 31 Keith: (.) He's like this much bigger than her (own/owner).
 32 She's like, "Ohh, what a ma::n". Huh-huh
 33 Laura: She's (.) a pioneer. She's out there.
 34 Keith: Always

35 Laura: Yes.

36 (3.0)

This last three second silence (line 36) is reinforced by other cues-to-end that are not evident in the CA transcript, like Laura's distracted facial gestures and continuous jerks on the leash. Although she uses a "yes" (line 35), it comes without the kind of elaboration (e.g., "Yes, how interesting") that might invite an extension of the topic or an invitation for a new topic. It is an agreement but nothing more, a kind of minimal summary statement that more plausibly stands as a signal to end. Keith does not "take the hint" from any of this (including the silences and physical gestures she coordinates with them). Keith does not let go of the dog or the conversation.

Instead, Keith does something else not oriented toward helping with a closure; he raises a somewhat new topic:

37 Keith: Well she can't make me no gran::pa.

38 (0.5)

39 That's my baby here.

40 (2.5)

41 ((yelping, whining sound.))

Standing in silence, pulling on the leash, Laura fails to respond to any of Keith's statements; seconds pass and the dog begins to whine (line 41), an event that induces Laura to make still stronger moves for closure. She looks only at Dottie and calls loudly at the dog (lines 42, 44, 46, 48) to come while Keith continues, now silently, to maintain his hold:

42 Laura: Come o::n.

43 (0.5)

44 Do::tie Dog.

45 (0.2)

46 ((clapping)) Hey!

47 (0.5)

48 Come

49 (0.5)

Laura's agitation coupled with the dog's movement toward her leads Keith to a bold move: he makes a substantive demand that the owner shows, in an uncharacteristically direct way, she will not abide:

50 Keith: Drop the leash for a minute walk away I wanna see what

51 happ[ens

52 Laura: [No, you know I can't. I'm not gonna drop my leash.

Again, Laura's declination comes as an overlap—quite in contrast to the usual pattern (found in CA) in which individuals preface a declination with a pause or prefatory buffer (like “gee” or “well uh”; see, e.g., Sacks 1987; Davidson 1984). Such buffers, among other things, allow space for requesters to inject a revised version that would forestall the refusal (e.g., quickly injecting something like “Maybe when there's less traffic”). Her no (line 52) is, in technical terms, harsh because it does not allow enough time for such a face-saving maneuver. She does soften her technical rudeness by following up with “You know I can't,” thus trying, albeit as afterthought, to observe more substantive versions of courtesy.

As Dottie responds to her master's calls, Laura desists from telling Keith to let go of the dog, it seems to us, because this would risk further conversational entanglement. Nor does she move physically closer to retrieve the dog, perhaps for the same reason or out of deeper fear (suggested, we think, in her voice as well as stance). As Keith playfully continues trying to hold on, the dog gets away over a 3.5 second interval, but with Keith maintaining a stream of questions that might have been answered by a dog owner more in sympathy with her dog's questioner. But the woman, gaining access to her dog, need no longer bother:

- 53 Keith: Where you goin'.
54 (1.0)
55 OWhere you goinO.
56 (2.5)
57 Now wait 'till I get (.) you can't wiggle down like a
58 snake. You gotta wait 'till I get ready. See ya later
59 baby.

The woman walks off. The conversation ended without so much as a good-bye or other form of reciprocity to Keith's “see ya”—still another adjacency pair deformed (good-byes ordinarily happen in pairs).

INTERACTIONAL VANDALISM: WHY IT IS TENSE OUT THERE

We are now in a stronger position to understand why comments and requests made by the black street men to the middle-class white women are so fraught with tension. From the dog walker's standpoint, Keith has shown in a string of turns that he betrays the system of practical ethics requisite for conducting social life (see Cicourel 1970). His “rudeness” is made visible not just through the content of his remarks, sometimes quite benign in themselves, but in disattention to the procedures whose tacit recognition provide for trust and a sense of security. So-called small talk is a kind of proving ground of benign intentions, as sensed by Jacobs and as more fundamentally worked out by CA scholars (e.g., Boden 1994).

After Keith's active disregard for the kind of conversational work, hard work, that people tacitly coordinate with one another, how could Laura trust him with her pet? Indeed, how could she trust him at all? Ironically enough, what might seem the technically difficult task of honoring a three-tenths of a second cue to end a conversation is presumed as so ordinary that an individual's failure to do so is threatening.

For many residents of the Village, political liberalness may enter into the dilemmas of their interactional experience. There is apparent discomfort of being "pushed" toward what they evidently do sense as performing some kind of rudeness—a problem for most people but perhaps especially an issue for people with the Villagers' politics. The Village is a stronghold of organizations like the American Civil Liberties Union that have secured the street people's very rights to "bother" the residents in the first place.²⁴ Indeed, a civil liberties tradition is a source of local pride; residents readily declare they live in the city to experience diversity; some may well have been attracted to the Village by Jane Jacobs's book or the authors she influenced.

We gave Laura a transcript of her conversation with Keith (as quoted above) along with our interpretations of them from a prior draft of this paper (largely intact in this version). She agreed we got it "fairly close," the exceptions being we overestimated her occupational standing as well as her age (both now corrected). She also added that many people, not just street people, talk to her since she got her dog and she is irritated when men of any sort (it is always men) assume they have a right to pick up her dog. She confirmed her special anxiety in encounters with men like Keith as well as her discomfort in being rude: "I think what you said about our white liberal guilt is true. . . . I was thinking about the fear in my voice and I think you are right about people having a hard time saying no, that you're not supposed to say no."

Laura then reflected on how street encounters had challenged her liberal views: "I'm getting more conservative as I get older and its not taxes. I don't mind paying taxes. I guess I don't mind some of the street cleaning up around here . . . [and I] HATE my reaction" (emphasis hers).

In the months after we taped her encounter with Keith, his leg problems had deteriorated; gangrene had set in after frostbite, complicated by cellulitis, a bacteriological infection that had gone untreated.²⁵ He was now forced to rely on a wheelchair, a possible reason why, as Laura reported to us, she had become more at ease in his presence, sometimes choosing

²⁴ One of the neighbors who has represented some of the men on the street was the late William Kunstler, one of the country's preeminent civil rights lawyers.

²⁵ With Keith's permission (a signed written release), we were able to inspect his medical records.

to talk with him and sometimes to ignore him. Picking up, we think, our use of the term in the analysis we showed her, she said in a tone resigned to the awfulness of the situation: "I have been rude to him and I'll be rude to him in the future."

A similar concern for her "rudeness" was conveyed by another resident, this time a white female academic whose walks were complicated by now having a baby with her. Speaking of the internal conflict caused by her interactions with the homeless in Greenwich Village (but also reflecting on her former life in Chicago as well) she said: "That's what cities are about. . . . You want to be able to trust in the kindness of everyone around you . . . [but] you can't ever look anyone in the eyes, you have to be really guarded and it actually ends up being sort of rude . . . especially now with a baby, it raises a whole host of other issues. People are always trying to touch [my baby] Danielle. When a homeless person who may or may not be diseased comes over or even heckles me or is excessively aggressive, I don't have the vocabulary to not be rude. And what ends up happening is that one has to be rude. And that's actually what's really upsetting about it."

From our perspective, neither the women nor the men lack anything in terms of "vocabulary"; more than that, they know the precise timings and other exquisite interactional maneuvers. The problem arises because the men use their tacit knowledge of conversational technology in a way that undermines the basis of talk. Prior studies show how the structurally more advantaged do something like this against the less advantaged (West and Zimmerman 1987) or how those of approximate equal status strive to do it vis-à-vis each other (Molotch and Boden 1985). Here the less powerful mobilize it as a resource against their "betters," a turnabout contributing to the threat. Akin to the more familiar form of vandalism involving assaults against the taken-for-granted ordering of physical property, these apparently "senseless acts are also, in fact, artfully constructed oppositional moves" (Cohen 1980). We define this phenomenon as "interactional vandalism" in that a subordinate person breaks the tacit basis of everyday interaction of value to the more powerful.²⁶

Dealing with such assaults presents a distinctive challenge, different from finding police when property is done in or protecting one's physical safety through streetwise strategies. It is also different from dealing with coarse vulgarities sometimes found in Gardner's (1995) "public harassment." More than in any of these cases, interactional vandalism leaves victims unable to articulate what has happened—another "problem with no name," to adapt Betty Friedan's phrase.²⁶ For this, indeed, there is no

²⁶ Betty Friedan (1963) was describing the plight of middle-class women caught in the bind of lacking a way to express the sexist sources of their unhappiness.

vocabulary (apart from the esoterica of conversation analysis) much less a ready redress. Mudrick was precisely correct when he says of his calls to the women that "they gotta deal with it." Perhaps the women could effectively revolt by themselves deeply breaching expected behavior—something no doubt possible but involving a radical reversal of how they otherwise conduct themselves—a challenge, in other words considerably beyond most forms of being streetwise.²⁷

We know from direct observation that the street men conform to the practical ethics of ordinary talk with each other and with ourselves—and even when arguing with police or local shopkeepers. But when they wish, they are masters of the breach, superior to many of Garfinkel's students assigned to do it in his legendary classes (Garfinkel 1967) and at a par with telemarketers, con artists, and nightclub magicians practiced at mobilizing people's dependence on conventional cues to keep them on the phone, make them go for the bait, or have them watch the wrong hand (Schiffman 1990; Sisk 1995). Besides such technical acumen, the street men also understand the substantive cultural truths that police do not make an arrest for ignoring a woman's pause and that Village women may feel obliged to appear "nice" even to people like themselves. This is not the place for still another exegesis on the meaning of "culture," but such accomplishments imply the men understand the local modes and folkways very well indeed.²⁸

However much trouble they create for the women, the street men pay their dues. Their way of passing time and gaining livelihood is hardly ideal from any standpoint, nor is the mechanism through which they "do" their gender (West and Zimmerman 1987). In the women's initial tendency to withhold even the civil glance of urban life (a technically disaffiliative move in the first place), the men face a hard challenge. In their mode of response the men gain—at most—a delay in the women's gait, a hesitant response to their parries, or the satisfaction of having enough power to create an attentive anxiety in those whose social position stands higher than their own. The street men's ability in these regards delivers, perhaps, a kind of pleasure at "getting over," as they sometimes say, on

²⁷ Anderson describes an apparent success of such a breach; a young white woman turned the tables on the black girls who habitually asked her for "loans" when she passed by. Seeing the request coming, she caught the girls off guard by asking them for fifty cents, and then seeing them befuddled walked off feeling victorious (Anderson 1990). Carried forward as a system, such behavior would yield, as both Garfinkel and Tönnies would agree, utter chaos.

²⁸ These observations conform to others' research demonstrating the "enormous linguistic, social, and cultural competencies" of those who, by virtue of race, age, or class are otherwise thought deficient (Maynard 1988, p. 315; see also, e.g., Labov 1972; Goodwin 1990).

another person. But they are stuck with talking to someone who does not want to talk, and the indignities, usually tacit, such people deliver to show it. Even as they precisely place their compliments and questions and as they cleverly mobilize dogs, researchers, and babies, the men endure a gauntlet of "small insults."²⁹

CONCLUSION AND SPECULATIONS

The strategy we have used to access these conclusions—contrasting observed technical deviation from patterns expected on the grounds of accumulated CA findings—lends itself to wider applications, including other sites of troubled contact. Researchers can contrast a presumed normal mode of conversational conduct with instances at hand to find and analyze difference. This can occur in any sort of setting, including those in which powerful people are vandalizing the weak. At a time when so many scholars are studying structural power as it operates in discourse (as when trying to depict hegemony vs. contestation among dominants and subalterns), this strategy would have utility.³⁰

A critical aspect of the "abuses, harryings, and annoyances" (Gardner 1995, p. 4) that make up our cases, are the men's methods of depriving the women of something profoundly crucial not just to them but to anyone—the ability to assume in others the practices behind the social bond. Much more is going on than the breaking of conventional "rules of etiquette" or even offering contemptuous substance. Through the pacing and timing of their utterances, the men offer evidence they do not respond to cues that orderly interaction requires. And because social construction of a real world has to be built out of conversations (heavily face-to-face, sometimes through other media), the absence of a "public show of respect" (Garfinkel 1963, p. 238) for this necessarily joint project deeply undermines ontological security (Pollner 1987). That another cannot be presumed to, *of course*, socially collaborate, even if to substantively disagree on what that world actually is, undermines trust—the "great civility" at the base of all human accomplishment, great or small (Shapin 1994, p. 36). Without it, the individual teeters on social vertigo, a "bewilderment" as Garfinkel (1967, p. 53) sometimes says. Even when performed among members of the same gender, class, race, or even family (as in the case of Garfinkel's experiments), breaching mundane orderliness undermines and

²⁹ West and Zimmerman use this term to describe how men talk to women, such as interrupting them (West and Zimmerman 1977).

³⁰ Regardless of the degree to which future CA studies show that findings hold across other language and national groups, the relevant principle is that CA findings derived from any group can act as template for deviant instances within that group.

threatens. Not just conversation, but cognition itself being a moral matter, the apparent presence of such uncooperative others understandably gives rise to tensions ungoverned by material or physical threat.

There are, we believe, layers of trouble, each intensifying the effect of the other. The class and status of these men, not just poor but apparently homeless and not just without good attire but sometimes disheveled, bespeaks a kind of distinction that itself has been^{*}much remarked upon as engendering anxiety and suspicion (e.g., Snow and Anderson 1993; Jencks 1994). On top of that, at least in our cases, there is gender hierarchy which, once again from the evidence of much commentary, threatens women in public. Finally, racial difference comes into play. Taking these abstractions to a concrete, intimate level, we are saying that interactional vandalism carries the status, class, race, and gender structures into the interaction experience. The social differences help constitute what the interaction troubles mean to the participants (they become more threatening than they otherwise would be) just as those displays of technical rudeness fill in the meaning of the gender, race, and class statuses on evident display.

In this way, we can see how it might be that interactional tensions both derive from "outside" statuses and forces, while feeding back into constituting them.³¹ In disregarding their gaze, walking past them as though they were not there and returning compliments with silence, the women appear to the men as beyond human empathy and, in their coldness and lack of respect, appropriate as men's interactional toys.³² For the women, the men's behavior and the predicaments to which it leads further reinforces their view of these men—and others who appear to be like them—as dangerous objects. Anxiety transfers to "innocent" panhandlers, book vendors, and, to some degree perhaps, poor black men in general; so-called stereotypes are given their life.

While not in themselves definitive compared to other urban experiences, these microlevel interactional breaches add in, we presume, to heighten residents' support for state controls over certain aspects of informal life—new laws, walls, and penalties (presumably the kind of responses our dog walker has come to accept). We glimpse a bit of the coher-

³¹ No doubt, the intensities and textures of the interaction problem would be different if the interactants were all men, all women, people of the same race, or individuals who knew one another well. For example, although the street men use both compliments and questions on women (both white and black), they only use questions (like the classic and amateur "Any spare change?") and rarely compliments toward men. Among women, black women, perhaps in sharing something of a common racial history with the black men, appeared more willing and able to talk back, move on, and with less apparent guilt than their white counterparts, but on this we lack strong evidence.

³² For some of the men's interpretations of the women's behavior, see Duneier (1999).

ently self-reinforcing system of mutual suspicion and incivility that, in structuration terms, links microinteractional process with social structural outcome.

As Jacobs used Greenwich Village to represent the "great city" and the basis upon which urban interaction can rest, we now use these blocks as a window on city troubles. The street men's eyes and ears do not bode security but angst. In the writings of Tönnies, Simmel, and Weber—encompassing sociology's founding paradigms—urban dwellers somehow solved their interactional problem. As Wirth was to later explain, people dealt with Simmel's "unceasing external contact" (Wirth 1938, p. 11) brought on them by numbers, density, and heterogeneity by erecting and respecting social boundaries. Parallel to Jacobs's concerns about people "getting in your hair," Wirth (1938, p. 12) elaborates that "the reserve, the indifference, and the blasé outlook that urbanites manifest in their relationships may thus be regarded as devices for immunizing themselves against the personal claims and expectations of others."³³

But such "immunization," as interaction scholars know to their intellectual cores, cannot happen as some mechanistic wonder of a setting—whether urban, rural, or in between. Rather, it is accomplished through moment-to-moment and cooperatively coordinated behaviors. But what people "give" one another, people can take away, again regardless of the setting. As the ethnomethodologists have long argued, all social relations require continuing reassurances that the procedures of practical ethics are in play. In this light, the urban interaction problem is only a subtype of the universal social condition. While the presence of strangers and heterogeneity help shape the problem (Lofland 1973, 1998), all people, contra Simmel et al., must manage social boundaries and access the microprocedures to do so—as has been found as far afield as Western Samoa (Duranti 1994) or among the African Poro people (Bellman 1984). We suspect that the difficulties we report on Sixth Avenue have less to do with urbanism, per se, and more to do with the contemporary character of class, gender, and resentment in settings like New York—another topic, but one massively addressed in popular and scholarly discourse.

Not wanting to risk errors of our forbears, we leave open as a historical and anthropological question just where interactional vandalism will or will not be found. Here we try to clarify how it works in one kind of scene, a realm implicated in the quality of urban public life and the cleavages that surround it. A part of this problem, likely one of the key icons standing for it all, are the handful of people who—as an aspect of their social

³³ Although Wirth (1938, p. 14) saw ethnic and class segregation obviating much of the problem, he noted that "frequent close physical contact, coupled with great social distance, accentuates the reserve of individuals toward one another."

location rather than participation in a sociological experiment—fail to respect others' efforts to coproduce mundane society. In behaviors that, we think, arise from and feed into deep social structural separations, the man and the woman do not always afford one another the capacities they need to make life together.

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Three Views of Associationalism in 19th-Century America: An Empirical Examination¹

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This article examines three different theoretical conceptions of associationalism with respect to cross-sectional data on municipal expenditure and voter participation in American cities for the fiscal year 1880: a “neo-Tocquevillian” perspective, which generally views associational activity as an alternative to government intervention; a “social movements” perspective, which views associations as interest groups mobilized to stimulate government action in areas germane to their specific pursuits; and a “social capital” perspective, which is neutral with respect to the impact of associationalism on the size of government but views associational activity as a stimulus to greater political participation. The analysis provides little support for the social capital and neo-Tocquevillian perspectives but strong support for the social movements notion that one of the predominant political influences of associationalism, at least in late 19th-century American cities, was that of interest group mobilization in support of specific municipal appropriations and party platforms.

INTRODUCTION

A long intellectual tradition running from Alexis de Tocqueville ([1835–40] 1988) to Arthur M. Schlesinger (1944) draws upon American political history to support the idea that civic associations play an important role in sustaining democracy. As evidenced by the number of new works on civil society, voluntary associations, and civic participation in recent

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years, contemporary scholars seem captivated with the idea that associationalism is (or was) a vital component of political life and that it poses a counterweight to the ever-present threat of political apathy and passive reliance on the state. Nonetheless, while the resurgence of interest in civil society and civic participation is useful in that it stresses the positive achievements of local community organizations, these assertions stand in dire need of sound empirical analysis designed to examine normative claims about "civil society" in an objective light.

This study focuses on a dimension of associationalism of paramount concern to many observers of democratic politics, that is, the relationship between civic associational activity and support for government expenditure at the local municipal level in the late 19th century. It aims to fulfill two missions: the first a historical inquiry into the impact of civic associationalism on the size and scope of municipal government in this period—the so-called Golden Age of American associationalism (Schlesinger 1944, p. 18);² and a second, more general concern with the relationship between extragovernmental organizations and government activity in local politics. It will compare the number of civic associations in 53 of the largest cities in the United States with their relative municipal expenditures and voter participation rates for the fiscal year 1880.³

More specifically, I hope to test several hypotheses concerning the effects of associationalism on democratic societies: (1) a neo-Tocquevillian argument, which states that by organizing associations for extragovernmental collective action, citizens will provide goods and services for their community and thus decrease governmental expenditures in those areas (e.g., Etzioni 1995; Olasky 1992);⁴ (2) a social movements theory, which

² Schlesinger is actually quoting an 1897 *North American Review* article by W. S. Harwood, "Secret Societies in the United States," 1514:622–23.

³ All cities with an 1890 population of 50,000 or greater were included in this study (1890 serves as the base year for a larger longitudinal study). Of these cities, Denver (Colo.), Camden (N.J.), Lincoln (Nebr.), and the District of Columbia provided insufficient municipal-level information to be included. In two cases, Charleston (S.C.) and Des Moines (Iowa), where the city directories had not been published for 1880, directories from 1879 and 1881 were consulted respectively. St. Joseph (Mo.) was omitted because I was unable to locate its 1880, 1879, or 1881 directories. Other cities were omitted from specific regression models on a case-by-case basis, largely owing to missing data on one or more independent variables.

⁴ Of course, this summary betrays the great diversity, if not division, within the contemporary neo-Tocquevillian camp concerning the proper size and scope of government. My account might best be described as a Tocquevillian argument that has been revived by (some) neo-Tocquevillians as of late. Nonetheless, I take the neo-Tocquevillian belief that power is best placed in "the people's," as opposed to "the state's," hands to reflect a general distaste for big government

posits that individuals form associations to mobilize support for government appropriations in areas germane to their interests and thus increase governmental expenditures in those areas (e.g., Clemens 1997; Oberschall 1973; Popielarz and McPherson 1995; Skocpol 1992, 1997); and (3) a social capital perspective, which is neutral with respect to the size of government but proposes that heightened levels of associational activity should nurture greater interest in public affairs and thus augment voter participation rates (e.g., Gamm and Putnam 1996; Putnam, Leonardi, and Nanetti, 1993; Putnam 1995a, 1995b).

In many ways, the nature of urban politics in the late 19th-century United States represents exactly the type of political situation discussed by theorists of civil society: the late 19th century was a so-called Golden Age of American associationalism, a period when the number of groups, clubs, and fraternities of all kinds grew enormously (Gamm and Putnam 1996; Putnam 1995a; Schlesinger 1944). At the same time, voter turnout was relatively high compared to modern levels, though voting behavior was largely confined to strict party lines (cf. Converse 1972; Burnham 1965). This was also a low point in the history of American political parties, a time when the two national parties lacked stark ideological differences and flailed about in an effort to mobilize voters around specific issues (Shefter 1978; Skowronek 1982). In the words of historian Robert H. Wiebe (1967, p. 27), "America in the late nineteenth century was a nation of intense partisanship and massive political indifference. With the order of things so uncertain, party identification gave men a common label, a comfortable rhetoric, to share with their group; it might also provide some sort of national attachment at a time when any rewarding ties between the community and the world beyond were rare indeed. . . . Yet the rules of the game required that these passions stay within the bounds of a single party commitment." Furthermore, the unwieldy structure of the national parties and the dominance of single-party urban "machines" mitigated the power of multiparty elections to address local concerns.⁵ In the absence of distinct, ideologically committed (local) parties, civic associations may have represented an important means for constituents to either exert pressure on elected officials or to pursue collective, communal goals (Clemens 1997; Hammack 1982; Skocpol 1992, 1997). The effects of associationalism on municipal social spending and voter participation should thus be particularly evident in this period, if at all.

⁵ On the disjunctions between local and national affairs, Wiebe (1967, p. 28) adds, "Parochialism split the major parties into a myriad of little units that no one had the capacity—or even the inclination—to combine. Like the society that reproduced them, the parties lack a central nervous system."

SOME TESTABLE HYPOTHESES

Three Theories of the Political Power of Associationalism

The founding of the United States of America presented democratic theorists with a troubling question about the nature of social order and political liberty. Given the simultaneous revelations of economic and political freedom, they asked, what will happen to the size and scope of government when put in the control of its citizens?

Neo-Tocquevillian Theory

Alexis de Tocqueville, among others, was a man of strong opinions on this subject. In his seminal volume, *Democracy in America*, Tocqueville argues that, given the choice between earning money and participating in the affairs of state, most citizens will choose the former. Though he was sensitive to the relationship between political freedom and social strife, he felt that the profit motive was ultimately stronger than the will to power—“Love of wealth therefore takes the place of ambition,” he writes, “and *prosperity quenches the fires of faction*” (1988, p. 306; emphasis added). In Tocqueville’s vision then, economic freedom fosters greed, which engenders political apathy, which results in excessive individualism and passive reliance on the state.

This political apathy will in turn result in the almost inevitable growth of government if left unchecked by associationalism. Thus, Tocqueville predicts, “It is easy to see the time coming in which men will be less and less able to produce, by each alone, the commonest bare necessities of life. The tasks of government must therefore perpetually increase, and its efforts to cope with them must spread its net ever wider. *The more government takes the place of associations, the more will individuals lose the idea of forming associations and need the government to come to their help.* That is a vicious cycle of cause and effect (1988, p. 515; emphasis added).

Collective action, such as that among the citizenry of Massachusetts, educates citizens in the virtues of community and cooperation, Tocqueville argues. “The Americans have used liberty to combat the individualism born of equality,” he writes (1988, p. 511), “and they have won.” Associationalism thus has the potential to stem the inexorable growth of the state (1988, p. 515): “The morals and intelligence of a democratic people would be in as much danger as its commerce and industry if ever a government wholly usurped the place of private associations.”

In other words, when individuals gather together to pursue common interests or achieve common objectives, they gain a new sense of their

responsibility to the community and at the same time perform services that would otherwise be required of the state, an argument repeated by "communitarian" thinkers today. "In every case, at the head of any new undertaking," writes Tocqueville (1988, p. 513), "where in France you would find the government or in England some territorial magnate, in the United States you are sure to find an association." Though Tocqueville also acknowledges the role of political factions and economically motivated political conflict in the preservation of democracy, these political associations are valued primarily because they pave the way for associations of all kinds—"So when a people has a political life, the idea of associations and eagerness to form them are part of everybody's everyday life" (1988, p. 521). As reformulated by Tocqueville's contemporary avatars, civic associational activity is thus portrayed as an important restraint on governmental growth (e.g., Barber 1996; Cohen and Arato 1994; Etzioni 1995; Olasky 1992; Van Til 1988). Associationalism is a virtue in itself by this account. It promotes voluntarism and communal self-sufficiency.

Historically, there is some evidence to support this view vis-à-vis late 19th-century America. Arthur M. Schlesinger (1944, p. 18) refers to the period after the Civil War as the "Golden Age of Fraternity," and Gamm and Putnam (1996) have shown that this was a period of dramatic growth in the size and scope of associational activity in the United States. Service organizations and benevolent societies—nonprofit groups that provided services such as medical care, poor relief, and education free of charge—were also prevalent in this era. Describing the role of these organizations in 19th-century American life, historian Peter Dobkin Hall (1982, p. 1) writes, "Private organizations took the place of the state, the family, and the locality in conducting fundamental economic and cultural activities" and "became the most powerful institutions in American economic and cultural life, and their creators, administrators, and members became the most influential decision makers in American society." Presumably, these groups took some of the pressure off tax-funded relief agencies, and in some cases government officials looked to charitable organizations to take over these responsibilities completely, as did the city of New York in the years immediately following the panic of 1873 (Mandelbaum 1965, pp. 70, 172–73).

Like Tocqueville himself, neo-Tocquevillians equate flagging participation in local civic groups with both an increase in excessive individualism and passive reliance on the state (e.g., Habermas 1989; Joyce and Schambra 1996; see Wuthnow [1991], esp. pp. 12–19 for a review). By this perspective then, we should expect to observe a *negative* relationship between

the number of civic association in a city or town and its overall commitment to social spending.

Social Movements/Pluralist Theory

Neo-Tocquevillians assume that civic groups represent beneficent attempts to meet communal goals through voluntary action, thus supporting activities that would otherwise be provided by government agencies or by for-profit firms. In contrast, pluralist theory and the social movements literature portray associations as special interest groups mobilized around the particularistic goals of their members. From this perspective, individuals form associations not to replace the functions of state or market but to coerce the state into assuming policies favorable to members' interests (e.g., Beisel 1990; Clark and Ferguson 1983; Dahl 1961; Gamson 1975; Oberschall 1973; Popielarz and McPherson 1995; Skocpol 1992; Tilly 1978). In *The Rise and Decline of Nations*, for example, Mancur Olson (1982) argues that small groups qua associations are more likely to lobby for special favors and government handouts than to work for the public good. And in a more optimistic light, recent works by Clemens (1997) and Skocpol (1997, pp. 468–69) portray associationalism as an impetus for the mobilization of translocal networks of individuals in pursuit of specific political goals. This latter approach emphasizes the role of naked self-interest in associationalism, while the neo-Tocquevillian perspective represents associationalism as a beneficent commitment to communal goals. Though the predicted outcomes of both forms of social activity may be similar—higher rates of voter turnout, greater public awareness of public issues, more “responsive” government, and so on—associations are portrayed differently in these two accounts. In the former, associations nurture fellowship through cooperative engagement, while in the latter they exacerbate social tensions through segmented self-interest.

Historically, there is just as much qualitative evidence to support the pluralist/social movements view as the neo-Tocquevillian view of late 19th-century American associationalism. Many so-called civic associations appear to have represented factional interests of voters looking to influence government policy in a manner favorable to their own economic situation, regardless of the commonweal. Labor unions are certainly one example of this type of activity; the Cigarmakers' Association of the Pacific Coast (organized in 1876), for example, state as their objectives “to improve the condition of its members and protect them against the encroachments of Chinese cheap labor” (*Langley's* 1880, p. 1128). And watchdog organizations like the New York Municipal Society sometimes vigorously lobbied politicians for specific municipal programs despite its

professed goal of promoting "public economy," as it did in the 1870s in support of greater expenditures on street cleaning (Mandelbaum 1965, p. 167).⁶ Alexander von Hoffman's (1994, pp. 90–117) study of turn-of-the-century Jamaica Plain, Massachusetts, demonstrates how some civic organizations, such as the Jamaica Plain Carnival Association and the Citizens' Association, were tight-knit political organizations mobilized to promote the business interests of members, which they often did by lobbying local officials for tax-funded public works projects that would promote commerce and increase property values in their neighborhood. Von Hoffman argues that the entire neighborhood usually benefited from these efforts, not just the members of the association, but it remains less clear what inhabitants of other Boston neighborhoods would have had to gain by paying for infrastructural improvements in Jamaica Plain. In any case, it seems clear that this type of associationalism hardly saved taxpayers money. Nonetheless, while many groups had a vested interest in increased municipal spending, others, such as New York's Taxpayers' Association of the Tenth, Eleventh, and Seventeenth Wards, were organized for the explicit purpose of lobbying *against* any increase in taxation (Mandelbaum 1965, p. 162). Though groups on both sides often lobbied in the name of the public good, naked self-interest seems clear in most cases. Associationalism of this type, though not undesirable, represents a very different type of collective activity than that represented by the Tocquevillian model.

James Madison's writings in *The Federalist Papers* ([1788] 1961) support this latter view of associationalism. For Madison, apathetic voters and passive reliance on the state were hardly as problematic as excessive factionalism and unwanted interference in political affairs. He begins "Federalist no. 10" (Madison 1961, p. 78; emphasis added): "Among the numerous advantages promised by a well-constructed Union, none deserves to be more accurately developed than its *tendency to break and control the violence of faction*." From Madison's point of view, Americans were naturally suspicious of public engagements and sought to influence decisions concerning taxation and social spending at every opportunity. "A landed interest, a manufacturing interest, a mercantile interest, a moneyed interest, with many lesser interests, grow up of necessity in civilized nations, and divide them into different classes, actuated by different sentiments and views," Madison (1961, pp. 79–80) comments. "To secure the public good and private rights against the danger of such a faction

⁶ This lobbying seems especially egregious in light of the devastated finances of the city at this time. Still reeling from the panic of 1873 and the depression that followed, New York's political leaders had been pressed into scaling back government expenditures at all costs.

[or factions], and at the same time to preserve the spirit and the form of popular government, is then the great object to which our inquiries are directed." Seen from this perspective, the motives behind civic participation venture far beyond Tocqueville's (1989, pp. 525–28) notion of "self-interest properly understood"—for Madison, associations are political factions representing citizens with common financial interests and an interest in the fiscal policies of the government. Where associationalism does flourish, it is not to provide for the commonweal but to lobby for some specific political benefit germane to the interests of its members.

Thus, while both Madison and Tocqueville would agree that economic motives always play some role in politics and that social conflict is an inevitable manifestation of democracy, they make noticeably different observations about the relationship between self-interest, civic engagement, and the effect of associationalism on government spending. In the Tocquevillian conception, associations represent not competing interests but a collective interest in providing public goods and services in lieu of government, a notion of some popularity in contemporary American politics as well. Madison, on the other hand, does not discuss the public functionality of associations except to assume that whatever their activities, members will tend to try and forward their interests by lobbying for government policies favorable to them. This aspect of associationalism is widely represented in the contemporary literature on social movements and political protest, though the word "association" is almost never used in this context.⁷

Note too that these two perspectives are not necessarily mutually exclusive. It may be that associations can have both effects. In the aggregate, associationalism may increase public awareness and provide an effective counterbalance to passive reliance on the state, while at the same time, specific associations may represent factional interest groups lobbying to influence social spending policy. The aim of this article is to examine the claims of both perspectives in the light of quantitative historical evidence from the late 19th century. If we find that the presence of specific types of associations is related to social spending patterns in areas in which those groups would appear to have a plausible "interest" in either promoting or opposing spending, then we can take this as support for a Madisonian or interest group interpretation of associationalism during this period. A significant negative relationship between the presence of nativist

⁷ An additional distinction needs to be made between Madisonian "factions" and the associations of the late 19th century: due to organizational innovation (Clemens 1993, 1997) and structural changes in the party system (McCormick 1986), political lobby groups at the century's end resembled those of the 20th century more than they did the informal, elite factions of Madison's day.

Protestant fraternities and poor relief, for example, might indicate that nativist groups actively opposed spending tax dollars on social services for new immigrants in their city. If, on the other hand, we find that the aggregate number of associations of *all* types has a consistent negative relationship with social spending, this would provide support for the neo-Tocquevillian perspective.

Social Capital Theory

A third theoretical camp, social capital theory, deserves mention here, as it synthesizes some of the concerns of both the foregoing approaches to associationalism. Like social movements theorists, proponents of the social capital perspective ask how it is that individuals organize to act together in pursuit of common goals (e.g., Brown and Ashman 1996; Coleman 1993), and, like neo-Tocquevillians, they assume that such participation will foster greater social cohesion and civic commitment (e.g., Fox 1996; Galston 1996). In their seminal study, *Making Democracy Work*, for example, Robert Putnam et al. (1993, p. 167) portray the impact of associationalism in reference to the classical "collective action dilemma," in which individuals refuse to cooperate out of distrust, fear, or simply the desire to "free ride." "Success in overcoming dilemmas of collective action and the self-defeating opportunism that they spawn depends on the broader social context within which any particular game is played. . . . Spontaneous cooperation is facilitated by social capital [i.e., trust, norms, and networks that can improve the efficiency of society by facilitating coordinated action]" (p. 167). Associationalism is thus viewed as a solution to this problem to the extent that it engenders the "trust, norms, and networks" (i.e., social capital) that facilitate collective action.

Where social capital theory diverges from both the social movements and neo-Tocquevillian perspectives, however, is in its estimation of the particular effects of associationalism. In contrast to neo-Tocquevillian theory, for example, Putnam (1995b, p. 671) finds that "among the U.S. states, differences in social capital appear essentially uncorrelated with various measures of welfare spending or government size" in the period between the 1970s and the 1990s—though late 19th-century American cities may provide a better opportunity to examine the relationship between civic participation and social spending given the "nationalization" of American politics and the rise of special interest lobbying in the latter period. And in contrast to social movements perspectives, conventional social capital accounts also place little stock in the notion that associations might represent specific and sometimes divisive group interests, thus promoting factionalism as opposed to social cohesion (cf. Levi 1996; Goldberg 1996; Harrison 1996; Portes and Landolt 1996). Instead, the social capital

perspective looks to the potential for associational activity to promote interpersonal network ties that enable individuals to “pursue shared objectives” (Putnam 1995*b*, p. 665) and postulates that the end result of such activity on the societal level is greater civic commitment, trust, and collective engagement. Putnam et al. (1993, p. 167) posit, for example, that “membership in horizontally ordered groups (like sports clubs, cooperatives, mutual aid societies, cultural associations, and voluntary unions) should be positively associated with good government”—“good government” is broadly defined as both more government spending (in certain select areas, such as local health care), more efficient bureaucratic management of government responsibilities, and public engagement in communal affairs.

Historically, evidence for the social capital perspective is readily available but often hard to interpret. Some descriptions of fraternal organizations, by far the most prevalent form of association in the period, confirm this impression. Schmidt (1980, pp. 1–20) notes that the Freemasons were intentionally apolitical in outlook and that they sought to improve relations among men regardless of ethnic, religious, or socioeconomic background, an exemplar of social capital attainment. A popular 19th-century tract, *The Odd Fellow's Amulet* (Bristol 1851, pp. 177–97), discussed in Blumin (1989, pp. 228–29), likens the fraternal lodge to a liberating “middle region” where the “drapery” of social class and ethnicity can be removed, thus revealing “the true man” in each member, an image strikingly evocative of Habermas's (1989) conception of traditional civil society and the public sphere. Presumably, such “open” communications foster social trust and civic engagement. Gamm and Putnam (1996) also argue that the secular decline in associationalism in 20th-century America is responsible for a decline in voter participation over the same period. While there is evidence for such a relationship on the national level, there is a clear need for more substantive evidence in support of this relationship on the state and local levels, where the proposed effects of associationalism should presumably be most evident. Using electoral returns from the congressional election of 1880, for example, one should expect to find a consistent positive relationship between civic associational activity and voter participation among a broad sample of cities, *ceteris paribus*, an indication that associationalism does (or did) in fact correlate highly (and positively) with civic engagement.

DATA AND MEASURES

Measuring Civic Associational Activity

In trying to operationalize the notion of civic associational activity, contemporary analysts often look to the presence (or absence) of so-called

civic groups in a given locale. Robert Wuthnow (1991, p. 7) argues that the constituent groups of the voluntary sector, or civil society, "can thus be defined residually as those activities in which neither the formal coercion nor the profit-oriented exchange of goods and services is the dominant principle." Thus, civic groups are presumed to be those that exist for purposes other than the acquisition of economic gain or the legitimate use of state-coercive power, though either may be indirect aims of the group. In other words, a manufacturing corporation or a municipal regulatory agency would not be considered a civic group, whereas a horticultural society, a workingmen's club, or a women's suffrage group would. While civic groups may indirectly represent political or commercial interests, they are neither legitimate arms of the state nor the direct recipients of commercial profits (though their members may be).

Finding reliable measures of civic associational activity is often difficult, however, particularly for this period of history. Public opinion surveys and membership levels in civic groups are sometimes used as indicators of contemporary civic activity, but cross-sectional data of this nature is scarce and often unreliable for the period in question. Some empirical measure of participation density (such as number of memberships per 1,000 inhabitants) would have been preferable, but no such data is obtainable for a wide cross section of cities in this period. As a proxy for these measures, scholars of the period often resort to counting the number of distinct civic groups in a given time and place, thus assuming that the aggregate number of groups provides a reasonable estimate of civic participation (factored by some measure of the number of inhabitants in the defined area).⁸ Relying on city directory listings of active clubs and associations, Gamm and Putnam (1996) track numbers of voluntary associations

⁸ This approach seems justified not only in light of data available for study of the late 19th-century United States but also from the theoretical perspectives they are meant to operationalize. Because civic groups are framed in terms of their ability to enable individuals to "pursue shared objectives," the number of participants in these groups seems less significant than the overall number of groups. In other words, given the assumption that any particular civic group represents a specific locus of network ties formed around some particular set of interests, the aggregate number of civic groups represents the existence of competing interest groups in a given locale fairly well. Furthermore, given that membership rates do not indicate whether a few people belong to many groups or many people belong to a few groups, aggregate statistics of the overall presence or absence of the groups themselves seem just as reliable an indicator of the level of civic associational activity in that place. Though some types of club may be more sensitive to fluctuations in membership than others, many of the lodges, fraternities, clubs, and orders studied here operated on a horizontally differentiated system whereby new branch organizations would be created when existing ones became oversubscribed.

per 1,000 in 21 cities from 1850 to 1920. Amenta, Carruthers, and Zylan (1992) use a per capita measure of the number of Townsend clubs in the United States from 1934 to 1950 to represent the strength of interest group mobilization in support of old-age pension legislation. Similarly, Skocpol et al. (1987) factor the number of local branches of the Grand Army of the Republic into an analysis of state spending on pensions for Civil War veterans. While it does seem plausible that city directories would offer incomplete listings of associations, particularly those representing minority groups and clandestine organizations (though *not* "secret societies" per se—in contrast to their name, these groups were open to publicity and are generally well represented in city directory listings of fraternal and sororal organizations), city directories provide at least a consistent representation of associational life in cities across the country and are thus a reasonable basis (if not the *only* basis) for quantitative estimates of associational activity.⁹

In order to differentiate between the aggregate effect of civic associationalism on social spending and the effect of specific kinds of civic associations on specific areas of spending, I analyze the independent effect of nine different types of associations found among the city directory listings for 1880 on municipal social spending and voter participation (see table 1). The first category, *arts/cultural groups*, represents artistic, literary, and cultural clubs.¹⁰ Public libraries, theaters, and opera houses were not included, though groups affiliated with them were. *Benevolent societies* represents a range of benevolent societies and charitable organizations, including relief societies, dispensaries, asylums, and private social service agencies. *Labor organizations* comprises trade unions and labor fraternities, such as the Knights of Labor.

Fraternal organizations (other than labor fraternities, such as the Knights of Labor) are included in two separate categories, the first of which comprises groups of an explicitly apolitical orientation, the second of which comprises groups organized around a specific political agenda: the variable *Masonic organizations* represents the number of Masonic

⁹ Entries for the directories were usually sought by registrars traveling door-to-door, which precludes suspicion of systematic discrimination on the part of editors. By this method, any association would have had an equal chance of inclusion in their local directory. In addition, there is ample evidence that institutional isomorphism in the publishing industry contributed to consistent associational searching and reporting practices among the nation's city directories. Publishers often published directories in a number of different cities, and accounting methods were generally consistent across the country, though individual discrepancies surely exist.

¹⁰ Learned societies and other groups of a scholarly nature were coded in a separate variable, *learned societies*, not included in this analysis (results available on request).

TABLE 1
DESCRIPTIVE STATISTICS

Variable Name	Description	Mean	SD	No. of Cases
EXPENDITURE*	Total municipal expenditure, excluding debt (in millions of dollars)	1.743	3.580	53
SCHOOLS†‡	Municipal expenditure on education (in thousands of dollars)	315.871	552.839	50
POOR‡	Municipal expenditure on public poor relief (in thousands of dollars)	124.766	418.627	37
VOTE80§	Voter turnout for 1880 congressional races	65.08	18.87	35
GREEN80§	Vote for Greenback Party congressional candidates, 1880	2.57	9.00	35
Population size ¹	City population (in thousands)	145.642	215.085	53
Population growth ¹	% population growth from 1870 to 1880	1.49	.39	53
Population density ¹	Population density (pop. per sq. mile) × 1000	7.03	5.63	53
Manufacturing jobs ¹	Number of jobs classified by census as employment in the manufacturing sector (in thousands)	24.028	40.361	53
Firm size ¹	Manufacturing jobs/number of manufacturing establishments	20.71	13.06	53
South dummy	Former Confederate state dummy variable	.13	.34	53
New England dummy	New England city	.17	.38	53
City age**	Years since incorporation as a municipality	57.12	41.96	52
% foreign-born ¹	% foreign-born inhabitants	26.14	10.28	53
% white ¹	% white inhabitants	91.96	13.13	53
Party competitiveness§	(100 - [(% votes for Democratic candidate) - (% votes for Republican candidate)])	86.13	12.75	47
Civic associations††	Total number of civic associations	193.51	188.95	51
Arts/cultural groups††	Total number of arts, literary, theater, and musical clubs	10.68	10.94	53

Benevolent societies††	Total number of benevolent societies, all types	27.85	34.26	53
Labor organizations††	Total number of labor organizations, trade unions, and labor fraternities	4.77	8.35	53
Masonic organizations††	Total number of Masonic lodges (not including state chapters)	20.10	17.70	52
Nativist fraternities††	Nativist political fraternal and sororal organizations††	4.63	9.76	51
Political groups††	Groups organized around a specific political cause, such as abolitionism, suffrage, or other local political issues (excluding nativism)	1.25	1.97	53
Professional organizations††	Medical, dental, pharmaceutical, legal, and other "professional" organizations	3.70	3.65	53
Religious organizations††	Religious groups (excluding churches and congregations): sodalities, benevolent societies, temperance clubs, bible societies, etc.	36.67	37.07	52
Veterans groups††	Veterans groups, fraternities, and clubs	3.77	5.12	53

NOTE—Dependent variables are in all caps.

* Department of the Interior, Census Office. Robert P. Porter, Special Agent 1884 *Report on Valuation, Taxation, and Public Indebtedness in the United States as Returned at the Tenth Census (June 1, 1880)* Washington, D.C. GPO Table 4, panel A.

† School expenditures for several cities were estimated from table 3, which listed school district revenues by county. These revenues were then adjusted by the percentage of the county's population residing within city limits. This was done for twelve cities: St. Louis (Mo.), Cleveland (Ohio), Pittsburgh (Penn.), Kansas City (Mo.), Indianapolis (Ind.), Allegheny (Penn.), Columbus (Ohio), Scranton (Penn.), Dayton (Ohio), Reading (Penn.), Evansville (Ind.), Des Moines (Iowa).

‡ Department of the Interior, Census Office. Robert P. Porter, Special Agent 1884 *Report on Valuation, Taxation, and Public Indebtedness in the United States as Returned at the Tenth Census (June 1, 1880)* Washington, D.C.: GPO Table 4, Panel C.

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¶ Department of the Interior, Census Office. George E. Waring, Jr., Special Agent 1887 *Report on the Social Statistics of Cities*. Tenth Census of the United States, vol 19, pls 1 and 2. Washington, D.C.: GPO. Manufacturing statistics for Los Angeles, California, estimated using Los Angeles County figures included in Department of the Interior, Census Office 1883 *Report on the Manufacturers of the United States*. Tenth Census of the United States. Washington, D.C.: GPO.

* City area statistics from Department of the Interior, Census Office. Frederick Howard Wines, Special Agent 1888 *Report on the Defective, Dependent, and Delinquent Classes of the Population of the United States*. Tenth Census of the United States, vol 2. Washington, D.C. GPO. Table 136.

** Department of Commerce, Bureau of the Census. 1917. *General Statistics of Cities: 1916. Including Statistics of Parks, Playgrounds, Museums and Art Galleries, Zoological Collections, Music and Entertainments, Swimming Pools, and Bathing Beaches, and Other Features of the Recreation Service*. Washington, D.C.: GPO. Table 1

†† City directories for the year 1880

‡‡ Patriotic and political fraternities and sororities identified using the list of "Patriotic and Political Orders" included in Stevens (1907, p. xviii).

lodges in each city,¹¹ while *nativist fraternities* includes nativist-patriotic fraternities listed in Stevens's (1907) *Cyclopedia of Fraternities*. Alvin Schmidt (1980, p. 14) comments, "Almost all American fraternal groups are intent on formally keeping politics out of their meetings and discussions. In fact," he adds, "they are proud to be nonpolitical."¹² Nonetheless, nativist/patriotic fraternities such as the Order of Native Americans, Order of the American Union, Order of United American Mechanics, and the American Protestant Association (later reincorporated as the American Protective Association) are an important and underappreciated exception to this observation. According to Stevens (1907, p. 290), "the patriotic and political orders" were typified by their dedication to "nativism, opposition to the alleged designs of the Roman Catholic hierarchy on the public school system in the United States, 'America for Americans,' and loyalty to country." Thus, by the logic of Madisonian factionalism, we should expect contrasting effects of Masonic and "patriotic" fraternalism on social spending. Presumably, the number of Masonic groups should not be related to social spending levels in any significant way, whereas patriotic fraternities should have a significant negative effect on social spending policies that might be perceived to benefit Catholics and immigrants. By the social capital perspective, on the other hand, both groups should foster political participation and civic engagement in equal measure.

The variable *political groups* represents a variety of associations created to pursue specific political objectives, such as suffrage organizations, activist groups, and local collectives organized to establish local monuments, memorials, and other politically significant landmarks. The variable *professional organizations* represents groups such as medical societies, dental societies, bar associations, and other groups comprising what are commonly known as "the professions" (i.e., groups in which selective membership is based on credentials certifying members' possession of a specific body of specialized knowledge). Not included in this variable are trade unions, labor fraternities, and commercial groups, such as local boards of trade, agricultural societies, and mercantile exchanges, though all of these groups were included in the aggregate measure of associational activity, *civic associations*.

All religious groups, sodalities, and societies other than churches and

¹¹ Non-Masonic lodges such as the Odd Fellows, Red Men, and Knights of Pythias are not included in either category, though they are included in the aggregate measure of associations per 1,000 (Results for these groups are available on request, as are those for all fraternal/sororal groups.)

¹² This disdain for political activism is unique to Anglo-American fraternal organizations, he also notes. Comparable organizations in France, Italy, and Germany have traditionally been much more politically active than their American and British counterparts.

congregations are categorized under *religious organizations*. (Churches and congregations are excluded from the aggregate measure of association activity).¹³ *Veterans groups* includes national veterans fraternities such as the Grand Army of the Republic as well as local veterans organizations.¹⁴ Lastly, the variable *civic associations* includes all of the above groups plus the remaining fraternal organizations, trade associations, commercial groups, and mutual benefit societies not included therein. (Militia groups, hospitals, and profit-oriented private corporations are often included in city directory listings of civic associations but were omitted from this analysis.) While categorization of these various civic groups by their aims and function was sometimes difficult in the face of specious names and the general absence of descriptive criteria, this process was aided by several reference volumes that document a large portion of the civic groups present in the country at this time (Schmidt 1980; Sills 1968; Stevens 1907). A residual category was also included for those associations that could not be identified and reliably placed in any single category, though this occurred for less than 1% of the cases for each city.

The Dependent Variables

Municipal expenditures.—Three different measures of municipal social spending are incorporated as dependent variables in order to assess the potential effect of associational activity on different areas of municipal government. I first examine each city's total expenditures, excluding debt service (tables 2 and 3). This measure gives the broadest sense of a municipality's commitment to spending private money on public goods and services. Though interpretation is complicated by the fact that overall reported spending will vary somewhat with local accounting practices, graft, and public-private ventures in areas such as transportation and utilities, I use this aggregate measure because it best represents how much money passes through municipal agencies' hands in any given year. State

¹³ I chose to exclude all churches and religious congregations from the aggregate measure of civic associations for two reasons: first, because of the disparity in average group size between Catholic and Protestant congregations (Catholic churches are many times larger than comparable Protestant organizations and thus there are usually many more Protestant congregations per constituent than in the cities I examined); and second, because the presence of specific religious groups in a particular city was highly correlated with the demographic composition of that city (particularly with the percentage of foreign-born residents). Religious groups other than the congregations themselves were included in the analysis as a separate independent variable, however.

¹⁴ Results for this variable are included only for the analyses of voter participation and Greenback Party support. The presence of veterans groups was not significant in any of the other regression models (results available upon request)

TABLE 2

COEFFICIENTS FOR REGRESSION ON TOTAL MUNICIPAL EXPENDITURE (IN THOUSANDS OF DOLLARS): U.S. CITIES, 1880

Independent Variables	Model 1	Model 2	Model 3	Model 4	Model 1A	Model 2A
Constant ..	-36.550 (610.693)	-57.480 (678.176)	444.683 (848.271)	-229.069 (855.127)	106.762 (316.211)	-131.373 (319.195)
Urbanization:						
Population size	10.869*** (10.869)	12.220*** (3.119)	11.808*** (3.252)	7.821** (3.523)	9.160*** (1.220)	7.772*** (1.315)
Population density	-2.802 (37.790)	-12.631 (44.103)	-7.971 (44.936)	16.256 (43.910)	28.705* (16.867)	36.453** (16.436)
Population growth ..	9.305 (348.395)	2.827 (356.646)	-168.534 (394.444)	-20.522 (379.940)	-88.949 (146.799)	-36.812 (141.798)
Industrialization:						
Manufacturing jobs ..	.	-7.092 (15.485)	-4.355 (17.068)	-5.510 (16.222)	575 (6.356)	-0.162 (6.064)
Firm size	3.155 (12.269)	.351 (12.693)	3.728 (12.143)	.271 (4.721)	1.523 (4.534)
Regional Effects:						
South dummy	-333.337 (441.288)	-265.384 (420.194)	-269.989 (164.188)	-246.943 (156.822)
City age	-3.238 (5.221)	-3.254 (4.960)	-1.630 (1.945)	-1.689 (1.854)
Associationalism:						
Civic associations	3.728** (1.584)	...	1.380** (.609)
F statistic	51.532	29.758	21.071	21.123	113.872	110.320
R ² ..	.771	.772	.778	.805	.951	.957
No. of cases	50	50	50	50	49	49

NOTE.—Data for Boston, Mass., are included in models 1 and 2 but are excluded in models 1A and 2A. Coefficients are unstandardized, and SEs are given in parentheses.

* $P < .1$ ** $P < .05$.*** $P < .01$

and federal subsidies to municipalities are also included in these figures, though they were practically nonexistent before the beginning of the 20th century (Shultz and Harriss [1931] 1965, p. 423).

I also examine social spending on education (tables 4 and 5) and public poor relief (tables 6 and 7).¹⁵ Given the fiscal and demographic demands of the era, school spending would likely reflect the costs of building new schools as well as paying teachers. Parochial schools would not have benefited from this spending (unless special exceptions were granted), however, which may have created a conflict of interest among taxpayers belonging to particular religious communities. Poor relief spending is much more diffuse and thus more open to the incursion of interest group pressure. While municipalities maintained some public institutions for poor relief, they also helped sponsor private charities. Thus greater public spending on poor relief would have benefited private associations as well as public institutions, presumably encouraging lobbying activity on the part of associations who stood to gain from these appropriations.

Voter participation.—Data on voter turnout and electoral support for the 1880 congressional election was collected for each city in the sample using data collected by Clubb, Flanigan, and Zingale (1986). Voter participation figures are based on the number of persons eligible to vote (given state voting requirements) divided by the number of persons who actually voted (tables 8 and 9).¹⁶ In addition, voter support for the two major parties, plus the Greenback and other “third” parties (namely, the Populist Party) were analyzed to see if there was any significant relationship between the number of associations in a given city and voter support for

¹⁵ Cities where poor relief was relegated to county agencies or special poor relief districts were omitted from the analysis of poor relief expenditures; in 12 of 50 cases, school expenditures were extrapolated by weighting the county’s school budget by the proportion of the county’s population within city limits.

¹⁶ Voter turnout statistics were calculated by Clubb, et al. (1986) based on a review of state-level voting requirements at the time of election. The baseline figure representing the total number of eligible voters is thus only an approximation of their actual number. Though there is some question as to the reliability of these figures—fraudulent voting practices may have inflated the numerator (or deflated it, in the case of Jim Crow obstructionism in the South), while census estimates of eligible voters may artificially underestimate the denominator—there is little reason to believe that there is any consistent variation between error in this dependent variable and the independent variables of significance, though there may well be a correlation between both types of distortion and other independent variables, such as percentage of foreign born and the southern cities dummy. Political scientists discount the impact of such distortions on cross-sectional electoral data (see Shortridge [1981] on underestimation of eligible voters and Allen and Allen [1981] on vote fraud). Thus any inaccuracies in estimates of voter participation should simply be incorporated into the error term of each regression equation.

TABLE 3

COEFFICIENTS FOR REGRESSION ON TOTAL MUNICIPAL EXPENDITURE (IN THOUSANDS OF DOLLARS): U.S. CITIES, 1880

Independent Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Constant	21,298 (334 266)	98,599 (323 031)	18 718 (312.128)	40 179 (308.146)	141,643 (330 265)	15 629 (309.582)	-176,959 (316.604)	72 284 (314.951)
Population size	8,834*** (1,289)	8,986*** (1 554)	8,907*** (1 198)	6,354*** (1 874)	9,022*** (1,277)	8,823*** (1 194)	8,533*** (1 171)	8,490*** (1 320)
Population density	31,379* (17 249)	30,269 (19 050)	27,188 (16,461)	19 559 (17,007)	29,068* (17,062)	32 266* (16,425)	29 430* (15,827)	32,414* (16,988)
Population growth	-65,275 (150 214)	-89,055 (148 561)	-71,682 (143,400)	-89,448 (142,155)	-103,537 (152,390)	-59 628 (142 863)	-49 997 (138,558)	-96,517 (145,798)
Manufacturing jobs	1 140 (6 419)	.437 (6 476)	.129 (6,200)	6,937 (6 983)	2,413 (7 794)	1,690 (6 178)	1 857 (5 984)	-207 (6,337)
Firm size	401 (4 743)	.999 (4 810)	.999 (4,620)	-757 (4 603)	-323 (4 978)	316 (4 569)	2 361 (4 504)	.384 (4,686)
South dummy	-242 939 (168 144)	-274,855 (168 225)	-268,928 (160,019)	-339 530** (163 028)	-280 082 (167,633)	-207 789 (162,082)	-103 413 (167 162)	-258,851 (163,166)
City age	-1,621 (1,953)	-1 753 (2,079)	-.960 (1 932)	-1,623 (1 883)	-1 641 (1,965)	-2,347 (1 918)	-1 870 (1,827)	-2,268 (1,993)
Arts/cultural groups	5 167 (6,320)

TABLE 4

COEFFICIENTS FOR REGRESSION ON EDUCATION EXPENDITURE (IN THOUSANDS OF DOLLARS): U.S. CITIES, 1880

Independent Variables	Model 1	Model 2	Model 1A	Model 2A
Constant	179.999 (157.366)	-50.333 (130.442)	123.686 (87.725)	-35.739 (54.673)
Population size	2.177** (.603)	.814 (.537)	1.736** (.339)	.807** (.225)
Population density	-10.273 (8.336)	-1.991 (6.698)	-4.162 (4.679)	1.026 (2.815)
Population growth	-28.203 (73.175)	22.397 (57.957)	-14.940 (40.726)	19.964 (24.288)
Manufacturing jobs	-1.996 (3.166)	-2.391 (2.474)	-1.175 (1.763)	-1.571 (1.039)
Firm size	-.285 (2.355)	.869 (1.852)	-.298 (1.310)	.540 (.777)
South dummy	-99.688 (81.865)	-76.458 (64.097)	-89.132* (45.550)	-73.703** (26.861)
City age	-.600 (.969)	-.605 (.757)	-.332 (.539)	-.371 (.317)
Civic associations	1.274** (.242)924** (.104)
F statistic	15.410	25.579	35.659	99.903
R ²720	.833	.859	.952
No. of cases	50	50	49	49

NOTE.—Data for Boston, Mass., are included in models 1 and 2 but are excluded in models 1A and 2A. Coefficients are unstandardized, and SEs are given in parentheses.

* $P < .1$.

** $P < .01$

one or another political party (tables 10 and 11).¹⁷ Because cross-sectional voting data for municipalities was not readily available, county-level data were used, though only counties in which the relevant municipality made up at least 50% of the total county population were included in the analysis. Nonetheless, discrepancies between city- and county-level voting be-

¹⁷ In this article, I include only the results for the Greenback Party, whose 1880 presidential platform included the following goals: "all money to be issued and its volume controlled by the National Government, an eight hour work day, enforcement of a sanitary code in industrial establishments, curtailment of child labor, the establishment of a Bureau of Labor Statistics, the regulation of interstate commercial facilities by Congress or an agency of its designation, a graduated income tax, the ballot for women, and equal voting rights for Negroes" (Rosenstone, Behr, and Lazarus [1984] 1996, p. 66).

havior and regional variations in jurisdictional boundaries may be a source of error in the results.¹⁸

Hypotheses

By creating variables that represent different types of associationalism and different areas of municipal social spending, I have tried to operationalize the three contrasting theoretical perspectives discussed above. Several hypotheses follow concerning the proposed relationships between the aforementioned variables.

According to the neo-Tocquevillian account, the aggregate number of associations should be negatively related to social spending as a result of a *substitution effect*, whereby citizens organize to perform services otherwise left to government agencies. Given this view of associationalism, we might also expect service-oriented associations, such as benevolent societies and religious organizations, to have a similar effect on social spending, particularly in those areas in which they are most active, such as poor relief.

The pluralist/social movements conception of associationalism is less explicit about the effect of the aggregate number of associations but clear with regard to specific types of association. In "Federalist no. 10," Madison (1961, p. 83) states that the best way to alleviate the effects of factionalism is to promote more factions, thus counteracting the potential for any one group to wield excessive power. "Extend the [civil] sphere," he writes, "and you take in a greater variety of parties and interests; you make it less probable that a majority of the whole will have a common motive to invade the rights of other citizens; or if such a common motive exists, it will be more difficult for all who feel it to discover their own strength and to act in unison with each other." Nonetheless, this effect is only speculative, and Madison gives no indication where this threshold lies relative to the size of the electorate. Where this perspective does provide the strongest predictions is in terms of associations' potential interest in particular facets of social spending policy. We might expect the relative number of nativist patriotic fraternities, for example, to be negatively correlated to social spending programs such as poor relief (much of which was provided by Catholic charities) or public education (which they often felt was

¹⁸ For example, counties in the New England states are unusually large in national terms, which led to the overexclusion of New England cities in this facet of the analysis.

TABLE 5
COEFFICIENTS FOR REGRESSION ON EDUCATION EXPENDITURE (IN THOUSANDS OF DOLLARS): U.S. CITIES, 1880

Independent Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Constant	12 460 (74,709)	104 398 (86 832)	54,356 (57 549)	113 145 (88,130)	140 559 (91,219)	95,313 (84,828)	32 005 (85,218)	102 097 (79 735)
Population size	1.312*** (.288)	1 324*** (.418)	1 536*** (.221)	1 292** (.536)	1,669*** (.353)	1,631*** (.327)	1,533*** (.315)	1 316*** (.334)
Population density	-.681 (3.855)	-.465 (5.121)	-5.355+ (3 035)	-5 609 (4 864)	-3 986 (4 713)	-3 053 (4 501)	-3 927 (4,260)	-1,039 (4 301)
Population growth	15.869 (33,573)	-15.192 (39,934)	-1.344 (26 440)	-15,019 (40,656)	-21,997 (42 090)	-5,812 (39 145)	-2,354 (37,295)	-19,679 (36,911)
Manufacturing jobs	-.439 (1 435)	-1,502 (1,741)	-1,526 (1,143)	-.168 (1,997)	-286 (2,153)	-828 (1,693)	-.761 (1 611)	-1,664 (1 604)
Firm size	-.128 (1,060)	-.056 (1,293)	.275 (.852)	-.461 (1 316)	-.585 (1 375)	-.284 (1 252)	-.377 (1 212)	-.228 (1,186)
South dummy	-53 927 (37 580)	-100 628** (45 220)	-88,296*** (29 504)	-100 141** (46,626)	-94 014** (46 300)	-69,767** (44 412)	-35 305 (44 994)	-82,157* (41,308)
City age	-.320 (.436)	-.624 (.559)	196 (.356)	-.331 (.539)	-.337 (.543)	-.555 (.525)	-.409 (.492)	-.731 (.505)
Arts/cultural organizations	6,724*** (1 413)							

TABLE 6

COEFFICIENTS FOR REGRESSION ON PUBLIC POOR RELIEF EXPENDITURE (IN THOUSANDS OF DOLLARS): U.S. CITIES, 1880

Independent Variables	Model 1	Model 2	Model 1A	Model 2A
Constant ...	49.305 (40.440)	40.835 (42.400)	55.848* (27.546)	55.934* (29.269)
Population size073 (.118)	-.028 (.183)	-.045 (.083)	-.044 (.126)
Population density	-863 (1 733)	-1.155 (1.792)	-1.108 (1.180)	-1.105 (1 232)
Population growth	-28.202 (21.386)	-24.269 (22.229)	-31.262** (14.565)	-31.302* (15.331)
Manufacturing jobs	1 899*** (.587)	2.109*** (.658)	2.280*** (.405)	2.278*** (.454)
Firm size	-.087 (.388)	-.077 (.391)	-.156 (.264)	-.156 (.269)
South dummy	-14.434 (14.282)	-11.372 (15.002)	-10.706 (9.741)	-10.735 (10.314)
City age123 (.157)	.143 (.160)	.180 (.107)	.180 (.110)
Civic associations050 (.069)	...	-.0005 (.048)
F statistic	37.609	32.441	70.363	59.369
R ²901	.903	.946	.946
No. of cases	37	37	36	36

NOTE.—Data for Boston, Mass., are included in models 1 and 2 but are excluded in models 1A and 2A. Coefficients are unstandardized, and SEs are given in parentheses.

* $P < .1$.

** $P < .05$

*** $P < .01$

endangered by Catholics).¹⁹ There is also good reason to expect the presence of interest groups such as veterans organizations and labor unions to be significantly related to social spending levels, given their frequent demands for government protection and social services (e.g., Clemens 1997; Skocpol 1992). And given the existing literature on the lobby efforts of late 19th-century arts and cultural organizations, we might expect their

¹⁹ The specific position of these groups vis-à-vis public education is an especially difficult issue. According to Stevens (1907, pp. 290–327), patriotic orders such as the Order of United American Mechanics made support for free, public education one of their primary goals. School policy was at the same time a hotly contested issue, pitting Catholics and Protestants against one another in a struggle to define educational priorities. Thus, it is unclear whether we should expect nativist groups to support higher or lower spending on education, if at all.

number to be positively related to social spending in areas such as public schooling and public works (Beisel 1990; DiMaggio 1982; Gilmartin 1995; Hall 1982; Wiebe 1967; Zelizer [1985] 1994).

The social capital perspective, on the other hand, does not make any predictions about the impact of associationalism on social spending but does predict that the *aggregate* level of associationalism in a city should have a *positive* effect on political participation, regardless of the distribution of particular types of organizations therein. Conversely, by Madisonian logic, the presence of certain apolitical groups, such as Masonic lodges and literary societies, should have no noticeable impact on voter turnout, *ceteris paribus* (though it should by the logic of social capital theory). In order to compare the social movements and social capital arguments regarding the impact of associationalism on voting behavior, I have also included results from a regression analysis of support for the Greenback Party in the 1880 election. Here my aim is to compare the effect of particular types of association on *both* voter turnout and voter support for a specific political cause in order to differentiate between the general effect of associationalism on political participation and its more specific effect as mobilizing force for particular political interests.

Alternative Explanations of 19th-Century Social Spending

A number of independent variables are included in the model in addition to the associational variables discussed above. These variables were selected for their ability to operationalize alternative explanations of municipal social spending and voter participation in this period.

One obvious alternative explanation of social spending patterns would be that city expenditures rise in proportion to population size, especially given the enormous growth in urban population in this period (Monkkonen 1988; Schlesinger 1933; Weber 1899). In addition to population size, two variables are included in the regression model to control for the effect of urbanization on city spending: the number of inhabitants per square mile, *population density*, and the percentage change in population size between 1870 and 1880, *population growth*.²⁰

Another likely factor in relative levels of municipal social spending is

²⁰ In modeling municipal finance (tables 2–7), I include raw population size as an independent variable and also use raw scores for other demographics such as the number of foreign-born inhabitants, manufacturing jobs, and civic associations. This was done to facilitate examination of the independent effect of population size on municipal expenditures net other factors. In the models of voter turnout (tables 8–11), on the other hand, I exclude population size and instead use per capita measures of these same demographic variables, owing to the fact that the dependent variable, voter turnout, is itself roughly scaled by population size.

TABLE 7
COEFFICIENTS FOR REGRESSION ON PUBLIC POOR RELIEF EXPENDITURE (IN THOUSANDS OF DOLLARS): U.S. CITIES, 1880

Independent Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Constant	57 060* (28 740)	53 577* (27 922)	52 651* (27 892)	61 304** (28 661)	59 880** (28 620)	51 445* (26 155)	51 287 (30 195)	51 125* (28 219)
Population size	-.034 (.102)	-.108 (.119)	-.075 (.090)	-.020 (.119)	-.054 (.085)	-.089 (.081)	-.058 (.106)	-.101 (.106)
Population density	-1 068 (1 219)	-933 (1 212)	-1 460 (1 250)	-913 (1 216)	-1 039 (1 199)	-1 009 (1 119)	-1 107 (1 215)	-1 117 (1 193)
Population growth	-31 969* (15 276)	-30 476* (14 715)	-29 574* (14 747)	-32 963** (14 844)	-33 511** (15 177)	-29 155** (13 821)	-29 979* (15 131)	-28 649* (14 948)
Manufacturing jobs	2 243** (454)	2 334*** (414)	2 382*** (422)	2 137*** (448)	2 513*** (558)	2 470*** (394)	2 327*** (427)	2 365*** (418)
Firm size	-150 (270)	-141 (267)	-142 (266)	-148 (266)	-213 (283)	-159 (250)	-128 (277)	-165 (266)
South dummy	-11 303 (10 394)	-11 258 (9 844)	-9 530 (9 870)	-8 067 (10 407)	-11 817 (10 016)	-6 137 (9 480)	-8 202 (11 698)	-8 794 (10 038)
City age	178 (110)	161 (111)	.207* (.112)	.177 (.108)	.172 (.109)	.147 (.102)	184 (.109)	.157 (.111)
Arts/cultural groups	-.077 (.400)							

NOTE.—Coefficients are unstandardized, and SEs are given in parentheses Data for Boston, Mass., are excluded

* $P < .1$.

** $P < 0.05$

*** $P < .01$

TABLE 8
COEFFICIENTS FOR REGRESSION ON VOTER TURNOUT 1880 CONGRESSIONAL RACE:
U.S. CITIES

Independent Variables	Model 1	Model 2	Model 3
Constant	15.098 (39.337)	-53.332* (25.199)	-65.453* (23.455)
Population density	- .582 (.468)	-.521 (.361)	.176 (.495)
Population growth	-4.177 (7.494)	-6.659 (5.641)	-6.583 (5.030)
% foreign born	-.103 (.337)	-.854* (.330)	-1.011** (.297)
% white	1.249** (.317)	1.259** (.279)
South dummy	-10.475 (12.487)
New England dummy	-28.520** (9.302)	-28.342** (8.206)
Party competitiveness749* (.337)	.487* (.230)	.572** (.205)
Civic associations (per 1,000 inhabitants)	2.038 (3.802)
F statistic	3.488	8.278	9.161
R ²376	.639	.720
No. of cases	35	35	33

NOTE.—Coefficients are unstandardized, and SEs are given in parentheses.

* $P < .05$.

** $P < .01$.

the growth of manufacturing and the impact of industrialization on the urban landscape. According to Douglass C. North (1985), industrialization brought about higher relative levels of social spending as new technologies reduced transaction costs for both producers and consumers. In addition to reducing the cost of tax collecting, increased demand for infrastructural improvements is believed to have inspired "rent-seeking" behavior on the part of industrialists. In other words, as competition grew and production prices dropped, manufacturers increasingly looked to reduce transaction costs as a way of bolstering profits and maintaining competitiveness. Infrastructural improvements such as road and bridge construction, new railway connections, and better communications networks were effective but expensive ways of reducing transaction costs, and presumably industrialists attempted to pass the costs of such investments onto the public whenever possible, a likely source of mounting social spending in industri-

alizing cities during this period.²¹ I include two variables as proxy measures of industrialization: the total number of manufacturing jobs per capita, *manufacturing jobs*, and the average number of employees per manufacturing establishment in the city, *firm size*.²² Presumably, municipal social spending should be positively related to both the number of manufacturing jobs in the population and the size of factories in that city, larger factories being more capital intensive and thus more likely to practice rent-seeking behavior on the part of owners.

It is also likely that there were significant regional differences in municipal social spending and political participation during this period. Cities of the former confederacy were still recovering from the Civil War, and the antistatist political climate of the Reconstruction South might have predisposed voters to resist government-sponsored social spending or to not participate in the political system at all (e.g., Bense 1984, 1990; Turner 1932). A dummy variable indicating cities that were part of the confederacy is included in the forthcoming regression models, *South dummy*, as is one indicating New England cities, *New England dummy*, in the models of electoral turnout.²³ So too is a variable recording the year in which each city was incorporated as a municipality, *city age*. Though this variable is not a direct measure of city age (i.e., the year in which the city was founded), it represents the birth of the city's status as a legal entity with the fiscal rights and responsibilities therein. It is added to the models in an effort to account for the fact that newer cities might have had both less developed economies and greater public expenses as they tried to erect an administrative and economic infrastructure comparable to that of older cities.

²¹ Interestingly, North (1985, pp. 388–91) also argues that the increasing division of labor brought about by industrialization caused the proliferation of associations and interest groups discussed above.

²² The latter measure is intended to help differentiate between small, artisanal manufacturing firms and larger, industrial factories. It was calculated by dividing the aggregate number of employees in manufacturing industries by the aggregate number of manufacturing firms in each city.

²³ The New England dummy was included after examination of the effects of several different regional specifications on the estimation of voter turnout results. It is likely that New England displayed unique voting behavior for several reasons: (a) it was the most heavily industrialized part of the country, which thus set it in opposition to rural supporters of the Greenback, Populist, and antitariff factions; (b) New England Brahmins fought vehemently against the incursion of new immigrants into their political and social system; (c) New England's traditional (proto-Tocquevillian) "town-meeting" style of local politics set it apart from the rest of the nation; and (d) a split within the Republican Party alienated many New England Republicans during the 1880 presidential election.

TABLE 9

COEFFICIENTS FOR REGRESSION ON VOTER TURNOUT, 1880 CONGRESSIONAL RACE: U.S. CITIES

Independent Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Constant	-55.926** (24.853)	-63.812** (26.189)	-49.876* (25.671)	-80.253*** (26.174)	-62.791** (22.720)	-54.469** (25.591)	-56.123** (26.152)	-51.199** (24.183)
Population density	-.707* (.379)	-.411 (.367)	-.569 (.367)	-.210 (.451)	-.05916 (.447)	-.460 (.381)	-.580 (.464)	-.696* (.358)
Population growth	-5.935 (5.572)	-7.722 (5.634)	-6.206 (5.697)	-8.037 (5.303)	-6.496 (5.100)	-5.884 (5.874)	-7.644 (5.957)	-8.916 (5.541)
% foreign born	-.915*** (32.750)	-.876** (327)	-.781** (343)	-.594* (335)	-1.053*** (314)	-.867** (335)	-.875** (341)	-.995*** (325)
% white	1.334*** (.318)	1.231*** (.314)	1.188*** (.327)	1.168*** (.298)	1.292*** (.290)	1.230*** (.323)	1.222*** (.326)	1.455*** (.323)
New England dummy	-30.348*** (9.242)	-28.161** (9.196)	-29.744*** (9.466)	-30.431*** (8.740)	-28.643*** (8.220)	-27.838*** (9.494)	-27.448*** (9.589)	-26.494*** (8.983)
Party competitiveness	.490** (.227)	.576** (.238)	.507*** (.233)	.600** (.220)	.577** (.207)	.492** (.233)	.539** (.243)	.388* (.227)
Arts/cultural groups (per 1,000 inhabitants)	-.028 (.020)

NOTE.—Coefficients are unstandardized, and SEs are given in parentheses

**** $P < 0.5$.**

*** $P < 01$

TABLE 10

COEFFICIENTS FOR REGRESSION ON VOTE FOR GREENBACK PARTY CANDIDATES, 1880
CONGRESSIONAL RACE: U.S. CITIES

Independent Variables	Model 1	Model 2
Constant	124.600*** (34.842)	128.635*** (35.537)
Population density	-.274 (.224)	.152 (.338)
Population growth	-6.470* (3.651)	-7.978** (3.766)
Manufacturing jobs012 (.019)	.002 (.021)
% foreign born245 (.202)	.258 (.204)
% white	-.761** (.298)	-.848** (.303)
South dummy	-19.365** (9.308)	-22.878** (9.663)
New England dummy	-4.227 (5.577)	-3.783 (5.590)
Party competitiveness	-.520*** (.161)	-.538*** (.161)
Civic associations (per 1,000)	3.599 (2.763)
F statistic	3.017	3.053
R ²481	.544
No. of cases	35	33

NOTE—Coefficients are unstandardized, and SEs are given in parentheses

* $P < .1$

** $P < .05$

*** $P < .01$.

In analyzing voting behavior, it also seemed important to include measures of the demographic makeup of cities, particularly given the salience of race and ethnicity in electoral outcomes (e.g., McCormick 1974). In these models, two control variables are added that operationalize demographic and socioeconomic factors that may have contributed to both social spending patterns and the nature of civic associational activity in different cities. The percentage of the city's inhabitants who were born in a foreign country, % *foreign born*, roughly accounts for immigration to that city. Though this measure does not account for the overall immigrant population in a city (which arguably would need to include second-generation immigrants as well), it provides a good indicator of the number of new immigrants. Presumably, heavy rates of immigration might increase social spending by adding to the burden of poor relief agencies, school

boards, public hospitals, and so on. Furthermore, an extensive literature on social control and the reaction of native-born, Protestant elites to late 19th-century immigration suggests that city spending might have increased as these elites attempted to monitor and "reform" new immigrants (Beisel 1990; Boyer 1978; Schiesl 1977; Wiebe 1967). A second variable representing the percentage of the population who were believed to be "white" (% *white*) was included in the voter turnout models (tables 8–11).²⁴

Given that political scientists have found that close elections generally stimulate higher levels of voter turnout (Chambers and Davis 1978; Hofstetter 1973), a measure of party competitiveness was also created. The variable *party competitiveness* was calculated by finding the difference in electoral support for Democratic and Republican congressional candidates, taking its absolute value, and subtracting from 100. A completely one-sided election will thus produce a party competitiveness index of zero ($100 - [100 - 0]$), while a deadlocked, two-party election will produce a value of 100 ($100 - [50 - 50]$). While it seems equally likely that close elections might also motivate greater municipal spending (i.e., patronage spending in exchange for votes), the timing of such processes is too subtle to model in a cross-sectional sample of this type. The presumption here is that municipal social spending levels reflect long-term trends in voter advocacy and that short-term effects of electioneering and vote buying are relatively minor and can thus be relegated to the error term of each model.

Readers will notice one conspicuous absence here: this study self-consciously omits extensive analysis of the effects of urban political "machines" on the fiscal performance of cities. This choice has several intuitive, methodological, and empirical justifications. Intuitively speaking, machine politics was more or less common practice in late 19th-century American politics (Lowi 1964; Shefter 1977, 1978; Skowronek 1982).²⁵ Thus, the presence of a Democratic or Republican regime in a given city is not likely to have made a significant difference in their fiscal policies,

²⁴ These two variables, % whites and South dummy, had a significant negative correlation of .88, which might have influenced the results of ensuing models through multicollinearity between these two independent variables. A number of different models were tried for each dependent variable in order to find the optimal combination (see comparative results in table 8—additional results available upon request).

²⁵ Social science historians commonly describe the political climate of this era as one of "courts and parties," where state and local parties tried to attract voters by distributing jobs and "favors" rather than by articulating any clear ideological vision for the polity. According to Stephen Skowronek (1982, p. 26), "parties [in the late 19th-century United States] were less notable for their programs than for this procedural unity they lent the state."

TABLE 11
COEFFICIENTS FOR REGRESSION ON VOTE FOR GREENBACK PARTY CANDIDATES, 1880 CONGRESSIONAL RACE: U.S. CITIES

Independent Variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Constant	122 787*** (35 178)	125 086*** (36 188)	115 571*** (33 715)	123 871*** (36 324)	133 416*** (36 279)	123 801*** (35 781)	121 291*** (35 097)	118 041*** (33 264)
Population density	-.204 (.243)	-.278 (.237)	-.202 (.218)	-.202 (.306)	-.056 (.308)	-.260 (.240)	-.107 (.283)	-.154 (.222)
Population growth	-6 606* (3 682)	-6 427 (3 773)	-6 926* (3 504)	-6 561 (3 973)	-7 166 (3 767)	-6 301 (3 835)	-8 075** (3 876)	-4 901 (3 560)
Manufacturing jobs	.013 (.019)	.013 (.020)	.011 (.018)	.012 (.022)	.023 (.023)	.012 (.020)	.003 (.021)	.012 (.018)
% foreign born	.262 (.205)	.246 (.207)	.147 (.201)	.266 (.237)	.180 (.221)	.242 (.207)	.267 (.205)	.324 (.196)
% white	-.771*** (.300)	-.762** (.304)	-.646** (.292)	-.764** (.321)	-.831** (.310)	-.760** (.303)	-.775** (.299)	-.849*** (.286)
South dummy	-18 573* (9 431)	-19 416* (9 518)	-17 881* (8 947)	-19 401* (10 102)	-23 937** (10 076)	-19 189* (9 535)	-19 093* (9 359)	-17 775* (8 878)
New England dummy	-3 624 (5 680)	-4 302 (5 699)	-2 620 (5 415)	-4 409 (5 810)	-5 093 (5 712)	-4 135 (5 738)	-3 031 (5 697)	-5 437 (5 331)
Party competitiveness	-513*** (163)	-523*** (171)	-532*** (155)	-.523*** (.168)	-.528*** (.165)	-518*** (.165)	-484*** (.165)	-444*** (.158)
Arts/cultural groups (per 1,000 inhabitants)	.010 (.012)

though important exceptions surely exist. From a methodological standpoint, the rewards of gathering information of this nature do not seem to merit the substantial effort involved in its collection. And, most important, empirical studies by other scholars point to the spurious effect of urban machines on municipal fiscal policies (e.g., Brown and Halaby 1984; Menes 1995)—that is, they contradict the assumption that machine cities would spend more than nonmachine cities as a result of profligate patronage politics.

ANALYSIS

This article presents preliminary findings from a pilot study of 53 of the nation's largest cities (cities with a population of at least 50,000 as of 1890, the base year for a larger longitudinal study) using published municipal-level data from the 1880 U.S. census and associational listings drawn from city directories of that same year.²⁶ By sampling only the largest cities in the country, I control for the potential effect of city size on the size and number of associations: big cities seem likely to have both larger groups and groups of a more diverse nature (i.e., more different types of clubs) than those in small towns, and cities are likely subject to different fiscal pressures than small villages and towns.

Following the dictates of the theories proposed above, analysis began with regression models in the following form (see table 1 for descriptive statistics):

$$\begin{aligned} \text{Municipal expenditure} = & b_0 + b_1 + b_2 + b_3 + b_4 + b_5 + b_6 \\ & + b_7 + b_8 + e, \end{aligned}$$

where b_1 is population size, b_2 is population density, b_3 is population growth, b_4 is manufacturing jobs, b_5 is firm size, b_6 is South dummy, b_7 is city age, and b_8 is civic associations; and

$$\text{Voter turnout} = b_0 + b_1 + b_2 + b_3 + b_4 + b_5 + b_6 + b_7 + e,$$

where b_1 is population density, b_2 is population growth, b_3 is % foreign born, b_4 is % white, b_5 is New England dummy, b_6 is party competitiveness, and b_7 is civic association.

The regression equation for municipal expenditures was also used in

²⁶ Of the 58 cities with 1890 populations above 50,000, I was only able to locate city directories for 53 (see n. 3 above concerning omissions due to insufficient census data). Of the 53 cities, all have an 1880 population greater than 20,000 with the exception of Los Angeles, California, which had a population of 11,183. (For more information on city size and regional distribution of the sample, see tables A1 and A2 in the appendix.)

analysis of spending on public schools and public poor relief. By analyzing these three different dependent variables, we can assess the effect of civic associational activity on three different areas of social spending: human resources development, poor relief, and the total city budget. Other than the explanatory variables representing civic associational activity, the other independent variables are included to control for the effects of urbanization, industrialization, regional differences, and immigration. A variable representing population size was omitted from the voter participation model in lieu of per capita measures of quantities such as demographic characteristics and civic associations. Though each dependent variable may be better represented by more nuanced models, the goal in this analysis was to incorporate a reasonable set of controls with which to examine the specific effects of associationalism on social spending, not to model spending itself.

One additional note needs to be added before discussing the results of these analyses: though several cities were omitted from the analyses because of missing data on one or more variables, the city of Boston was consciously excluded from most of the models after careful examination of preliminary results. As of 1880, the year for which all analyses take place, the municipality of Boston had per capita expenditures well above the national average in each of the three areas of spending examined here. In each model, Boston was identified as an outlying case that might be having a disproportionate effect on the regression results.²⁷ Boston's unusually high municipal expenditures (and correspondingly high numbers of civic associations) were the likely result of the municipal annexation of Charleston, West Roxbury, and Brighton in 1874, which brought about new short-term financial obligations and a windfall of local associations throughout this period of adjustment. Political institutional reforms were undertaken in 1885, and the city's municipal budgets were generally on the decline through this period, 1874–86 (Huse [1916] 1967, p. 175–229). Results with Boston, however, are included in models 1 and 2 in tables 2, 4, and 6, but they are excluded in models 1A and 2A; the improved goodness-of-fit is clearly evident. Only results without Boston are given in tables 3, 5, and 7.

RESULTS

Concerning the three competing theories of the effect of associationalism on municipal social spending, the results point to several conclusions: most

²⁷ It should also be noted that the positive relationship between municipal social spending and associationalism was especially strong in the Boston case (results available upon request) and that exclusion of this case thus allowed for a more rigorous examination of this relationship among the remaining cases.

noteworthy is the relationship between aggregate civic associational activity and municipal social spending. Controlling for a wide range of independent variables, the aggregate number of civic associations had a significant and *positive* effect on social spending levels (tables 2 and 4), with the exception of poor relief spending (table 6). This finding contradicts both Tocqueville's idea that increased associationalism should help limit state growth and the contemporary neo-Tocquevillian belief that big government "crowds out" civic participation. If rampant associationalism has any relationship to municipal social spending, it is a positive one, thus tentatively supporting the social movements/pluralist view of associationalism, which equates associationalism with sectional rent-seeking behavior. In contrast to social capital theories, I also fail to find any significant relationship between the aggregate number of civic associations and voter participation rates across this sample of cities.

Turning next to the results of the several different types of associations examined, I find further support for the pluralist conception and a consistent lack of support for neo-Tocquevillian and social capital perspectives. None of the associations included here were significant and *negatively* related to municipal social spending (tables 3, 5, and 7), in contrast to the neo-Tocquevillian hypothesis; nor were they significant and *positively* related to voter turnout (with the exception of Masonic organizations—table 9), in contrast to the predictions of social capital theory. Though Madison's pluralist predictions about the general effect of associationalism are not entirely clear (i.e., he postulates that the power of rival factions may be cancelled out through electoral competition but does not specify any threshold whereby this effect will be attained), there is substantial evidence to believe that specific types of associations sometimes exerted enough interest group pressure on municipal officials to affect social spending patterns. Notice, for example, that the coefficient for the number of labor organizations is significant and positive for total and educational spending, as are those for political groups and professional organizations (tables 3 and 5). Arts/cultural groups and religious organizations were also positively related to social spending on education (table 5), and in each case a clear argument can be made for the group interest behind such a relationship. The rationale is attributive, and thus *ex post facto*, but the logic of action seems clear and justifiable (though the observed significant correlations may well be the result of factors other than interest-group lobbying—e.g., members' passive electoral support for candidates espousing mutually satisfactory political goals). In each case, the logic of group interest clearly provides an explanation for the significant relationship between the presence of a particular type of association and social spending in related areas. Note too that the number of labor organizations and veterans groups were both significantly related to public sup-

port for Greenback Party candidates in the congressional election of 1880 (table 11), which also supports the logic of the pluralist/social movements perspective.

The only result not directly justifiable by the logic of sectional group interest is the significant positive relationship between the number of Masonic lodges in each city and their relative levels of total municipal expenditures (table 3)—the Masons supposedly had no clear interest in supporting municipal expenditure, and they claimed to be thoroughly apolitical in origin. The number of Masonic lodges was also significant and positively correlated with voter participation rates (table 9), which may indicate that, unlike other forms of associationalism, Freemasonry did in fact cultivate social capital among members, thus promoting civic commitment, social trust, and support for municipal social spending. Note too, however, that the number of veterans groups in each city was significant and *negatively* correlated with voter turnout rates (table 9), a finding particularly surprising in light of the recent literature on the importance of pensions for Civil War veterans in political discourse of that era (Skocpol 1992; Skocpol et al. 1987). We might thus conclude that different types of association have different effects on members' political behavior and civic engagement.

The control variables included in the model generally perform as expected. Population size was a strong predictor for total spending (tables 2 and 3) and educational spending (tables 4 and 5), as was population density for total spending (tables 2 and 3). Interestingly, however, population growth was significant and negative for poor relief spending (tables 6 and 7), which implies that rapidly growing cities generally spent less on public poor relief than did their counterparts. The number of manufacturing jobs, a rough indicator of industrialization, was significant and positively correlated with poor relief (tables 6 and 7), on the other hand, a finding that may indicate that the newer commercial cities of the West and Midwest generally lacked the social services infrastructure of their older, more industrialized East Coast counterparts. (It may also support the economic notion of "rent-seeking" behavior on the part of industrialists.) And as would be expected, southern cities generally spent less on education than other cities (tables 4 and 5) and supported the Greenback Party in higher numbers (tables 10 and 11), though there were no significant effects on any of the other dependent variables. City age was not a significant predictor of any measure of social spending, though a more accurate measure of city age may have provided better results.

With regard to voter participation (tables 8 and 9), the percentage of foreign-born inhabitants in a city was significant and negatively correlated with voter turnout and the percentage of white inhabitants was significant and positive, both of which may be artifacts of (illegal) measures to keep

blacks and foreigners away from the polls. Party competitiveness was also a significant factor; close races between Democratic and Republican candidates generally fostered high turnout at the polls. Voter turnout was also generally lower in New England cities than elsewhere, possibly owing to infighting in the Republican Party, which resulted in the presidential candidacy of Ohio congressman James A. Garfield (who was later shot by a disgruntled Republican office seeker). This dissension with the Republican Party may have kept New England's predominantly Republican voters away from the polls for that year's congressional elections as well.

DISCUSSION

Scholars and political commentators are more than happy to portray America as "a nation of joiners," associations as purveyors of "the 'I' into 'we,'" and participants as beneficiaries of "habits of the heart." None of these assertions are at issue here. Instead, I have endeavored only to ask what, if any, are the effects of this behavior on government? Does America's extensive history of civic associational activity betray a now lingering commitment to the commonweal, as demonstrated by fiscal restraint and a can-do spirit of voluntarism? Or does the historical record indicate that associations more accurately resembled Madisonian factions, characterized by self-seeking interest group mobilization? This is not an either-or question, naturally, but the answer may help shed light on increasingly ideological debates about the role of public and private spheres in contemporary American society.

In many ways, commentators' views of associationalism reflect larger, more deeply held opinions about the nature of society, the imperatives of democracy, and the appropriate balance between compromise and conflict in a diverse, pluralistic world. Schlesinger's (1944) "Biography of a Nation of Joiners" reflects an abiding concern for the protection of political freedom and the belief that the rise of totalitarianism in Weimar Germany was at least partially due to government obstruction of civic associational activity.²⁸ Writing in a much different milieu, Henry David Thoreau was far more skeptical of the impact of civic associations (1929, pp. 364–65): "The American has dwindled into an Odd Fellow," he writes, "one who may be known by the development of his organ of gregariousness, and a manifest lack of intellect and cheerful self-reliance." And even George

²⁸ In his final paragraph, Schlesinger (1944, p. 25) concludes, "It was with calculated foresight that the Axis dictators insured their rise to power by repressing or abolishing political, religious, labor, and other voluntary groups. . . . Hence joiners were among the earliest casualties of the totalitarian system." That the Nazi Party was itself founded as a voluntary group of "joiners" is not considered, however.

Washington was forthcoming in his 1796 farewell address about his contempt for "all combinations and associations, under whatever plausible character, with the real design to direct, control, counteract, or awe the regular deliberation and action of the constituted authorities" (Richardson 1896–99, p. 217).

In this study, I have attempted to distinguish the variable effects of associationalism on voter turnout and municipal social spending. The results suggest that while neo-Tocquevillians may be right in suggesting that associations bring people together to perform collectively beneficial acts, the assumption that this serves as a successful counterbalance to state spending is not justified in the light of historical evidence, nor is the assertion that associationalism generally promotes greater political participation.²⁹ Empirically speaking, if civic associationalism had any relationship to government spending, it was a *positive* one. Though voluntary organizations and civic groups may have provided a number of services for their communities that would otherwise have required tax dollars and state supervision (Tocqueville's assertion), the results of this study show a positive relationship between the relative number of civic associations in American cities and the portion of taxable assets spent by municipal agencies.³⁰ Similarly, with regard to voting, there is substantial evidence to believe that associationalism promotes self-seeking more than it does communal engagement. In the aggregate, the prevalence of civic associations had no significant relationship with voter turnout levels across this sample of cities. Taken at face value, these findings suggest that we have reason to question neo-Tocquevillian and social capital theories of American political development and ample evidence to equate some types of associations with Madisonian *factions*, or modern-day interest groups. Note too that this study documents the presence of these interest groups in 1880, more than a decade before contemporary accounts would place them (cf. Clemens 1997; Skocpol 1992).

Without further data, however, we cannot be certain whether the positive relationship between associationalism and municipal social spending found here reflects associations' direct efforts to lobby for higher social spending or whether nonprofit groups merely proliferated in response to a strong demand for social services. Nonetheless, I would argue that these

²⁹ On the other hand, it may be that this measure of aggregate civic associational activity is simply not sensitive enough to the finer nuances of civic participation, such as the percentage of the population involved in such activities, public opinion concerning social trust, etc.

³⁰ Additional regressions were run using fiscal effort as the dependent variable, a measure of municipal social spending that takes into account the amount of taxable assets available to fiscal planners on a yearly basis. Findings were consistent with those published here (results available upon request).

findings are best understood in light of the fact that municipalities often funded social services such as poor relief at least partially through subsidies to private charitable organizations during this period. The Catholic diocese of New York City, for example, is said to have received nearly one and a half million dollars in public (i.e., taxpayers') money between 1869 and 1871 (Mandelbaum 1965, p. 70), and such subsidies seem likely to have stimulated the desire for more.³¹ Thus, it is possible that the proliferation of private service agencies inflated social spending on poor relief by increasing competition for resources and creating lobby pressure for increased spending. On the other hand, it may well be that public and private associations efficiently worked together to provide a better overall level of services in their community and that the higher spending levels observed here simply reflect a greater local commitment to the provision of public goods and services.³² Future research might examine whether public or private institutions were more efficient and responsive providers of these services. Comprehensive reviews of associational action might also clarify the intended (and unintended) effects of voluntary organizations on the behavior of elected politicians—did associations actively pursue their “interests” by directly lobbying politicians, or did their influence come indirectly, through the power of expressed interests and the aggregate electoral strength of members?

A more important question is to distinguish the political effects of associationalism at the individual and organizational levels, that is, whether it is the sheer number of associations, or the aggregate number of persons participating in them, that really matters.³³ There is an obvious need to

³¹ See also John Mohr's (1994) account of the persistent competition among public and private benevolent societies for legitimacy, funding, and “clientele” in turn-of-the-century New York City.

³² The efforts of late 19th-century city dwellers to form elite arts and cultural organizations are well documented, e.g., and their ardent support for public works projects and morally uplifting social programs for the poor and uneducated seem understandable in light of the social pressures incumbent on American cities during this period (e.g., Beisel 1990; Boorstin 1965; DiMaggio 1982; Gilmartin 1995; Katz 1986; Wiebe 1967; Zelizer 1994).

³³ In contrast, Putnam (1993, pp. 174–76) proposes a hypothetical difference in effect between horizontally and vertically ordered associations (thus side stepping the elusive meso-micro link)—vertically ordered groups should foster excessive individualism and “mistrust” by nature of their hierarchical structure, he hypothesizes, while horizontally ordered groups should stimulate participation and trust—a distinction that future research might investigate more carefully. On the other hand, it seems just as likely that it is the coercive nature of some organizations (such as the Mafia or the Ku Klux Klan) and not their vertical ordering that might account for their “undesirable” social effects. Furthermore, many types of groups most typically associated with

reconsider the question on two different dimensions: participation density (i.e., the number of people actually participating in associations) and organizational density (i.e., the number of active associations). If a small number of people are conjointly members of many overlapping organizations (as classic models of elite organizations would have it), then the total mass of network ties and social trust accrued will scarcely benefit from the production of yet more homogeneous spin-off groups. On the other hand, if a large number of people are members of a small number of (very large) organizations (as are many contemporary catch-all, nonprofit organizations), then we risk compromising the hands-on, collectivizing benefits of grassroots organization for higher participation rates. One might hypothesize that organizational density is an important predictor of interest group mobilization, while participation density plays a more important role in the generation of social capital, and, furthermore, look for interaction effects between the two. Is there an optimal balance between associational size, number, and membership, and, if so, how does it vary with the spatial and demographic dimensions of cities and towns?

If there is a simple lesson to be drawn from this study for observers of contemporary politics and society, it is that civic associations help people (particularly self-enfranchised *groups* of people) get what they want from government, for better or worse. Indeed, Madison may have been right in suggesting that the best cure for factionalism is more factionalism. Given the wide range of special interests represented by associations, past and present, associationalism may be desirable if for no other reason than to counteract the nearly inevitable presence of other factions. (Most likely Tocqueville would agree.) On the other hand, excessive associationalism may simply lead a few associations to wield political power disproportionate to their actual membership—imagine several massive organizations, replete with hundreds of local branches, mobilizing behind a select few issues (or candidates) each electoral season, a situation strikingly (and ironically) similar to that described by Tocqueville in the following example of the “omnipotence of the majority”:

A few years ago some pious people undertook to make the state of the prisons better. The public was roused by their exhortations, and the reform of criminals became a popular cause.

New prisons were then built. For the first time the idea of reforming offenders as well as punishing them penetrated into the prisons. But that happy revolution in which the public cooperated with such eagerness and which the simultaneous efforts of the citizens rendered irresistible could not be accomplished in a moment.

fellowship and reciprocity are nonetheless firmly structured around vertical leadership hierarchies, such as sports teams, symphony orchestras, and fraternal societies.

Alongside the new penitentiaries, built quickly in response to the public's desire, the old prisons remained and housed a great number of the guilty. These seemed to become more unhealthy and more corrupting at the same rate as the new ones became healthy and devoted to reform. This double effect is easily understood: *the majority, preoccupied with the idea of founding a new establishment, had forgotten the already existing ones.* Everybody's attention was turned away from the matter that no longer held their master's, and supervision ceased. The salutary bonds of discipline were first stretched and then soon broken. (Tocqueville 1988, pp. 249–50; emphasis added)

What escaped Tocqueville (and captivated Madison) was the role civic associations might play in this pernicious process. Rampant associationalism, its predominating influence unchecked, may thus nurture the very condition Tocqueville hoped it would combat: associations have a pronounced tendency to accumulate social and political resources out of proportion with their actual size, despite any collective benefits they may provide. Once again, Tocqueville (1988, p. 252) writes, "My greatest complaint against democratic government as organized in the United States is not, as many Europeans make out, its weakness, but rather its irresistible strength. What I find most repulsive in America is not the extreme freedom reigning there but the shortage of guarantees against tyranny."

This also suggests a pressing need to reconsider associationalism in light of the time-worn "collective action problem" presented by Mancur Olson some years ago (1965): How does associationalism affect the political behavior of those *outside* the nonprofit sector? While the lobbying power of civic associations may provide a strong incentive to some voters to participate in the political system, it may similarly dissuade others from participating by augmenting the perceived benefits of free-riding in the face of already-enfranchised interest groups. Why vote if the members of groups X, Y, and Z already have a lock on the election? Furthermore (and perhaps more regrettably), if individuals can count on nonprofit organizations to provide public goods and services of their own accord, non-participants may be just as likely to "lean on" (i.e., free ride on) nonprofit groups as they do on the state. Why join if the "joiners" already have things under control? Thus, while existing associations may rely on a committed core of active members, their very presence in society may exacerbate political apathy among the general public rather than prevent (let alone remedy) it. Future studies should examine the effect of associationalism on the political behavior of these nonparticipants, an important, if not sizable, group rarely considered by political analysts in relation to those actively engaged.

In sum, this study gives good cause to reconsider the political ramifications of associationalism. It is my contention that civic associations be seen as both a stimulus and a constraint to civic-mindedness and commu-

nal action. In developing and newly democratized societies, associationalism may provide a powerful stimulus to political participation, effectively kick starting individual interest in the political process. But in developed, democratic societies, associationalism may simply accentuate some of the less desirable tendencies of majoritarian politics and quasi-corporate voluntary organizations—political apathy, “tyranny of the majority,” passive reliance on both the state *and* the nonprofit sector, and so on. Much further study is needed before we can accurately assess the costs and benefits of this form of political organization, whether it nurtures civic-mindedness or alienates the general population, whether it stimulates grassroots organization or enfranchises “professional” political networks, whether it promotes self-reliance or exacerbates collective dependence, and, ultimately, whether it helps promote democracy or only multiplies its shortcomings.

APPENDIX

TABLE A1
SAMPLING DISTRIBUTION BY CITY SIZE

CITY SIZE	SAMPLE DISTRIBUTION		NATIONWIDE* DISTRIBUTION	
	<i>N</i>	%	<i>N</i>	%
>500,000	4	8	4	2
500,000–100,000	15	28	16	7
100,000–50,000	15	28	15	7
50,000–10,000	19	36	182	81
Total	53	—	223	—

* Nationwide statistics taken from U.S. Bureau of the Census (1976), tables A57–72, A172–94.

TABLE A2
SAMPLING DISTRIBUTION BY REGION

REGION	CITIES		URBAN POPULATION*	
	N	%	N	%
Northeast	27	51	7.4	52
North central	17	32	4.2	30
South	7	13	2.0	14
West	2	4	.54	4
Total	53	—	14.1	—

* Population *N* is given in millions

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Career Patterns of Executive Women in Finance: An Optimal Matching Analysis¹

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This article examines the objective and subjective facets of female finance-executive careers. Optimal matching and qualitative analyses are used to show how the careers are shaped by workplace structures and by the early 1970s enforcement of women's employment rights. Changing opportunity structures in turn shaped respondents' perspectives. Many younger respondents were unaware that their mobility was partly due to the creative action of their female predecessors and took personal credit for their own rapid progress. Finally, it appears that as women have experienced more freedom in pursuing finance careers, their career trajectories have become more rigid.

INTRODUCTION

The popular press often reports that successful women in male-dominated occupations have followed unusual career paths marked by flukes and accidents (e.g., Deutsch 1996; Thomas 1995). Trade publications make similar claims. For example, *CFO* magazine reports that most of the female chief financial officers it interviewed "prefaced their career history with the comment, 'I did something unusual'" (Walbert 1995, p. 36). To the extent that women's careers in male-dominated fields are now more regular, popular accounts assume that change to have been a gradual de-

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velopment brought on through years of political pressure from organized lobbies. Yet we know little about what these careers actually look like.

This article studies the complete, intact careers of female finance executives in the context of their shared labor market history. Despite the surge of women entering management over the past 35 years, women constitute less than 5% of senior executives (U.S. Department of Labor 1995). Women who have reached senior management levels represent one end of the distribution of employed women. As an extreme case, executive women highlight social processes that may affect other employed women more subtly. My respondents are predominantly white, upper middle class, and rich in human capital: they were well positioned to take advantage of legal and social changes affecting women's place in society.

In studying these careers, I have three central empirical concerns. First, are there distinct patterned trajectories in careers or, as many of my respondents and much of the public believe, are those careers random and accidental? Second, have the changes in women's managerial careers in finance over the past 35 years been gradual or sharp? Third, what effects, if any, did the legal and social changes of the early 1970s actually have on women's advancement in finance, a prestigious, male-dominated field targeted for reform? My theoretical concern is to see if the extreme case of female finance executives usefully outlines in exaggerated form the possibility, under certain conditions, of agency amidst structural constraints.

These questions grow directly out of existing sociological research on careers. This research has a variegated history. Several early studies examined career paths within professional occupations (Becker 1952; Reissman 1956; Smigel 1964). These studies found careers progressing in an orderly sequence toward higher levels of responsibility within an occupation or organization. However, Wilensky (1961) and, later, Rosenbaum (1979) and Evans and Laumann (1983) cast doubt on the notion that most careers move forward in an orderly fashion. An exception may be careers progressing up internal labor markets (ILMs) that advance smoothly along a foreseeable path (Kerckhoff 1995).

The status-attainment model reduced careers into a few variables predicting current job status at a point in time (Blau and Duncan 1967). Critiques of status attainment emphasized the dependence of careers on organizational structure (White 1970; Rosenbaum 1979; Stewman and Konda 1983) and sectoral labor markets (Doeringer and Piore 1971; Beck, Horan, and Tolbert 1978). These developments coalesced into a new structuralist approach (Baron and Bielby 1980), which emphasized the impact of work structures such as firms, industries, and occupations on individual outcomes (Kalleberg and Berg 1988; Kerckhoff 1995). The attention to workplace structures deemphasized the study of individual ca-

reers weaving through these structures, although some analysts examined a few job transitions that were parts of longer careers (Rosenbaum 1979; Rosenfeld 1980).

Some studies have used models that take into account the dynamic nature of opportunity structures. This line of research has examined the effect on job shifts of historical labor market conditions (Blossfeld 1986), industry and occupation expansion and contraction (DiPrete 1993; DiPrete and Nonnemaker 1997), sector interdependence (Korn and Baum 1994), organization founding and failure (Haveman and Cohen 1994), and national differences in job distributions (DiPrete et al. 1997). Although this work has emphasized the importance of changing employment structures, it has studied either aggregate shifts in mobility or short-term individual mobility over just two points in time rather than the ways intact careers are shaped by changing structural conditions.

Most research that does focus specifically on careers has ignored the real time in which job histories unfold and has ignored historical context. Common strategies include using synthetic cohorts (Spilerman 1977; Spenner, Otto, and Call 1982) or concatenating job shifts to create simulated career sequences (Stewman and Yeh 1991; Yamagata et al. 1997). Researchers have further simplified careers by examining only intraorganizational job shifts (Rosenbaum 1979; Stewman and Konda 1983; Althauser and Kalleberg 1990) or by ignoring duration (Gaertner 1980; Stewman and Yeh 1991). ILM studies have focused on finding job transitions common to subgroups of firm employees (Althauser 1989; Althauser and Kalleberg 1990). As Stewman and Yeh (1991) point out, these simplifications are partly due to a lack of data on complete careers and partly due to the problem of parsimoniously comparing complex real careers. These simplifications have allowed analysts to learn much about structural properties of organizations but less about the real careers of individuals as they unfold through decades of time. An important exception is Stovel, Savage, and Bearman (1996), which analyzes intact male careers in one bank within historical context. Yet these data lack information on portions of careers developed outside the organization.

Most of the research discussed above has either restricted itself to men's careers or studied all workers in an organizational unit while ignoring the theoretical importance of gender (Yamagata et al. [1997] is an exception). Yet a growing number of studies focus on women. These studies often analyze short segments of women's work histories and disconnect careers into individual job shifts (Waite and Berryman 1986; DiPrete and Soule 1988; Rosenfeld and Spenner 1992). Numerous studies find that women's work lives are often affected by family obligations (Gerson 1985; Hertz 1986; Hochschild 1989, 1997) and geographically circumscribed by husbands' careers (Markham et al. 1983; Markam 1987; Steil and Weltman

1991). Some women respond to the challenges of juggling workplace and family responsibilities by becoming entrepreneurs (Carr 1996). The insight that women's market work and family lives are intertwined brings the study of women's careers closer to the life-course literature, which emphasizes the interweaving of career and family events (O'Rand and Krecker 1990; Moen 1992).

Some life-course studies argue that individuals compare their work lives to familiar models and struggle to shape them into these cultural patterns (Levinson 1978; Betraux 1982).² This would lead us to expect patterned careers. However, clear cultural models may not exist to help women in finance construct careers into recognizable patterns. Many respondents have few female role models and believe their career paths are unusual and marked by flukes and accidents. Furthermore, women face contradictory paradigms for structuring their lives. For example, the male managerial cultural pattern of intense commitment to the organization during the first several years of the career coincides with the life-cycle point at which most women in our society bear children (Hochschild 1971; Hertz 1986).

The life-course emphasis on entire life histories reintroduces the intact career of individuals as a viable unit of analysis. Ideally, data on intact careers would include spells out of the workforce or in part-time jobs, as these may have implications for future advancement. The study of intact careers should also consider the sex type of all jobs held, since time spent in female-dominated, sex-neutral, and male-dominated jobs may affect women's mobility (Jacobs 1989; Rosenfeld and Spenner 1992).

Since careers unfold within real time and space, the study of careers over the life course is in part a historical analysis. Social practices usually reproduce social structures with small revisions, but historical events can interrupt social regularities and rearrange structures (Sahlins 1991; Sewell 1996). Historical events shape careers by interacting with social structural constraints and openings.

A significant event in women's history is the early 1970s feminist pressure for the enforcement of legislation protecting women's employment rights. Title VII of the Civil Rights Act of 1964 prohibited employment discrimination on the basis of race, sex, and other criteria. Starting in the mid-1960s, this and other legislation helped open employment opportunities for African-Americans (Collins 1983). However, *women's* employment rights were not enforced until the early to mid-1970s (Freeman 1973; Ferree and Hess 1985; Kessler-Harris 1994). Earlier studies have not systematically addressed how this enforcement of women's employment rights has shaped women's careers.

² In addition, individuals plan their work lives in response to structural constraints (White 1970).

For several reasons, these female finance-executive careers, ranging from 1956 to 1994, constitute a particularly useful case for examining intact careers in historical context. First, we can expect that legal and social upheaval had a direct impact on financial organizations, since the federal government monitored banks and targeted some for early lawsuits (Ashenfelter and Hannan 1986; Reskin and Hartmann 1986). Second, respondents are predominantly from middle- and upper-class backgrounds, are highly educated, and have worked continuously in the labor force since college. They are rich in human capital and are well poised to take advantage of these legal and social changes. Third, unlike occupations that women entered as men were exiting due to diminished rewards, relative to other male-dominated jobs (Reskin and Roos 1990), executive positions in finance-related fields have remained high paying, prestigious, and male dominated (Korn/Ferry International 1993). We might expect more resistance to women's advancement in finance-related executive positions than in occupations that men are abandoning. This is a case of formidable structural obstacles confronted by people with ample human capital and legal resources. It provides an ideal opportunity for the study of the interaction of human agency and social structural constraints.

This overview raises three important issues. First, what do intact careers look like, how are they shaped by organizational and occupational structures, and do they fall into patterned trajectories? The careers literature is inconclusive on whether patterned career trajectories exist in many fields. If patterns do exist for financial managers, the extent to which these are shaped by ILMs and other workplace structures is unclear. Second, how have careers changed over time? Much of the literature ignores temporality and uses methods insensitive to historical change. Third, what have the effects of the legal and social changes of the early 1970s been on the theoretically important case of female finance executives, a group poised to marshal their resources against structural barriers to advancement?

Theoretical interests inform the choice of methods, and methods shape theoretical questions (Ragin and Zaret 1983; Ragin 1987; Abbott 1988). In Ragin's (1987) terms, most sociological research on women's work lives is either "variables-oriented" and quantitative or "case-oriented" and qualitative. In the trade-off between generality and complexity, variable-based strategies pursue generality, while case-based approaches explain complexity. Variable-based research uses linear methods to predict the relative weight of abstract individual and social structural properties, envisioned as variables, on individuals' attainment or wages (e.g., Jacobs and Blair-Loy 1996; Marini and Fan 1997). This approach powerfully measures probabilistic relations between variables in large populations. Yet variables-oriented research assumes that "a certain effect exists independent

of context, that is, independent of the values of the other causal variables in each case" and presupposes a model that is essentially additive rather than one that accommodates multiple, intersecting causes (Ragin 1987, p. 63–64). Since we have no reason to believe that effects on women's careers are independent, linear, and not offsetting, I do not use models that make these assumptions.

Qualitative, case-based research on women's work generally studies a case of particular theoretical interest based on a small, nonrandom sample of respondents interviewed in-depth by the researcher (e.g., Gerson 1985; Hertz 1986; Hochschild 1989, 1997; Williams 1989).³ Case-based approaches cannot make general statements of empirical regularity about large populations. Yet they can uncover and interpret constellations of social and individual forces that change or reproduce social processes.

Qualitative samples are small for theoretical reasons. Many researchers gather data and develop theory in an interactive manner until "saturation," when interviews yield no new theoretically interesting information and additional interviews are unlikely to be useful (Glaser and Strauss 1967). There are also practical grounds for small samples: in-depth interviews are time consuming to conduct and laborious to analyze.

I pursue the strategy of the extreme case. In contrast to random, variable-based studies that examine mean effects of variables on the middle hump of the bell curve, my study investigates one extreme end of the distribution of employed women. Human action is shaped by available resources and structural barriers (cf. Sewell 1992). My case is high on both axes: respondents are rich in resources and face formidable structural obstacles. Other extreme samples (e.g., female scientists) may also be high on both axes and may also provide useful cases of resourceful women negotiating structural obstacles to advancement. Restricting the sample to female executives in *finance*-related fields controls for variation across different industries and professions. My respondents are similarly affected by the same macroeconomic and political forces, define themselves as a coherent occupational group, and belong to a common professional organization. My findings are not statistically generalizable but may be hypothesized to occur in similarly situated cases and may illuminate similar processes in less extreme cases.⁴ Other case-based studies could investigate

³ For example, Gerson (1985) interviewed 63 young women who grew up in the 1970s and are theoretically interesting as members of cohorts "most responsible for the rise in the percentage of women workers [and] the decline in the birthrate" (p. 10). Hertz (1986) interviewed 21 married couples in which each member had a corporate career in order to examine family and work relations among spouses who are economic equals.

⁴ Hochschild (1983) provides another example of an extreme-case strategy. By elucidating the processes of emotional labor in the extreme case of flight attendants,

other corners of the social landscape by examining samples that are low on one axis and high on the other or are low on both axes.

This article differs from standard qualitative approaches in that I conducted a formal, replicable analysis of unfolding careers. My theoretical questions demand a focus on intact careers in the context of a shared labor market history with methods sensitive to the effect of early career steps on later ones and to the dynamic nature of employment structures. Optimal matching, a subset of sequence analysis techniques, is a method for seeking common patterns in sequences (Abbott and Hrycak 1990; Abbott 1995; Dijkstra and Taris 1995; Stovel et al. 1996). It provides succinct comparisons and classifications of lengthy careers, even if they transgress organizational or labor market boundaries. It allows the study of complex careers over the life course instead of aggregating and reducing them into a few job shifts. Optimal matching is a heuristic technique designed to probe complex social processes rather than to ascertain the relative importance of abstract variables. Unconstrained by the simplifying assumptions of variable-based approaches, I detect and explain the conjunction of factors that create career patterns. A relatively small sample is appropriate for an analysis that examines the complexity and nuance of a theoretically important case rather than makes aggregate predictions. This small, qualitative, and nonrandom sample does not meet the assumptions necessary for most statistical tests. Nonetheless, I occasionally use "difference of means" statistical tests as heuristic devices to ascertain structuring within my data or to illustrate differences between patterns. Sociology needs to develop better customs for assessing the "significance" of patterns discovered in the formal analysis of qualitative data.

This article also analyzes respondents' own interpretations and action. Their subjective assessments contribute to our understanding of the patterning and pace of female executive careers. The argument thus transcends the familiar dichotomy between objective determinants of social processes and the role of subjects' actions and interpretations (Bourdieu 1990; see also Kalleberg 1989; Emirbayer and Goodwin 1994; Emirbayer 1997).

The next section presents the finance-career data and methods in detail. "Patterns and their origins" uncovers five distinct career trajectories and analyzes the factors that shape them. "Changes in opportunity structures" analyzes changes in women's career opportunities over time. The article concludes with discussions of the transformation of structures by histori-

Hochschild enabled other analysts (e.g., Leidner 1993; Pierce 1995) to see emotional labor as a job requirement in other occupations.

cal events, human agency and interpretation, and implications for feminism.

DATA AND METHODS

Data

The data consist of life histories of 56 women in high-ranking, finance-related jobs. Respondents belong to a finance executives' professional organization based in a large U.S. city. This organization helped me gain access to respondents by giving me a membership directory and by endorsing the research, but it did not try to influence the analysis or findings. Each respondent filled out a life-history questionnaire detailing her work, family, and education history from age 17 to the present. I then conducted in-depth interviews with each respondent on her career, family, triumphs, and regrets. This article analyzes respondents' work lives from age 22 to the interview date in 1994.

Respondents work in commercial banks, financial services (including investment banks, real estate investment firms, and financial consulting firms), manufacturing and diversified services companies, and public accounting and law firms. They have reached levels in companies ranging from vice president (in manufacturing), senior vice president (in financial services), and partner (in law and accounting firms) up to chief financial officer, managing director, chief executive officer, and managing partner. Nine women have left large companies to form their own firms.

In 1994, respondents ranged in age from 36 to 60. All have bachelor's degrees; 86% have graduate degrees. Of the respondents, 21 are mothers, 47 were married at least once (20 are divorced, and 6 have remarried). With the exception of one African-American, all the respondents are white. The findings can thus strictly refer only to white female finance executives.

Respondents' annual compensation in 1993 ranged from approximately \$75,000 to \$1 million, with a median of \$250,000.⁵ In past years, some individuals made additional millions with the sale of stock. Currently married respondents earned between 25% and 100% of their household income, with a median of 50%.

All respondents belong to the theoretically interesting case of senior women in elite, male-dominated, finance-related occupations. They define

⁵ The low end of the compensation distribution includes four women who work in nonprofit firms or who recently started a business. Disregarding these cases changes the range to \$125,000 to \$1 million.

themselves as belonging to a coherent professional group, and their careers are subject to the same political and macroeconomic influences. The sample is diverse across industries, types of organization, and age, thereby containing variation on some dimensions.

Job-Level and Organization-Size Codes

This analysis relies on the data collected in the life-history questionnaires and interviews. It uses detailed information and exact dates on job type, firm size, promotions and employer shifts, industry, geographic mobility, and education for every year of respondents' adult lives. I coded respondents' careers using a combination of deductive and inductive reasoning. I read industry reports, general literature, and compensation surveys and talked to three management consultants to understand the determinants of what I call the career value of a position.⁶ Career value is a position's degree of importance, responsibility, and risk. In the present, career value rewards the incumbent with income, prestige, self-esteem, and the opportunity to earn bonuses and stocks. The career value of a position is also an investment in the future. It can be leveraged into a step toward a higher position, or it can anchor a lateral move.

I determined that, among finance-related managers, two crucial dimensions of a position's career value are job level and organization size. Combining my knowledge from industry reports and the management consultants with mappings of respondents' actual career steps, I coded the jobs women held into nine categories (see table 1). Job levels 4–8 are broad categories that are consistent across industries and over time. These categories range from an entry-level, finance-related job, such as a financial analyst or management trainee (coded "4"), to the top position in an organization, such as managing partner of an investment bank or CEO of a firm (coded "8"). I also coded the years respondents spent as entrepreneurs, running their own finance-related firms (coded "en"). Two categories are outside the finance labor market: female-dominated jobs (e.g., elementary school teacher, secretary, etc., coded "fe") and a residual category of non-finance jobs that were not male dominated (coded "nf"). Since prior research has shown that the sex-type of the occupation is an important variable in women's work histories (Jacobs 1989), I coded female-dominated occupations separately.

The salary surveys demonstrate that organizational size is the single

⁶ The compensation reports and industry surveys are Finance Club (1982), *Fortune* (1972–87, 1994a, 1994b), William M. Mercer, Inc. (1953), Standard and Poor (1994a, 1994b, 1995), Top Executive Compensation (1994), and Dunn and Bradstreet (1960–94).

TABLE 1

JOB-LEVEL AND ORGANIZATION-SIZE CODES FOR OPTIMAL MATCHING ANALYSIS

Variable	Definition
Job Code.	
nf	Nonfinance, non-female-dominated job
fe	Female-dominated job (e.g., elementary school teacher)
ps	Professional full-time student, such as law or business
4	Entry-level management trainee or analyst
5	First positions with significant management responsibility (e.g., assistant treasurer in a manufacturing company, vice president in a financial services firm)
6	Mid-senior management positions (e.g., corporate treasurer in manufacturing, senior vice president in financial services)
7	Senior management positions (e.g., chief financial officer in manufacturing, executive vice president in financial services, senior partner in law or accounting firm)
8	Top positions (e.g., chief executive officer, managing partner)
en	Entrepreneur, finance related
Organization Size:	
S	Small organization: under \$50 million in sales in 1993
M	Medium-size organization: larger than small organizations but too small to be on Fortune 500 list
L	Large organization: size between 250th and 500th Fortune 500 manufacturing firms or between 25th and 50th Fortune financial services firms
V	Very large organization: larger than 250th Fortune 500 manufacturing firm or larger than 25th Fortune 50 financial services firm

NOTE—Example nf 4V 4V 4V 4V 4V 4V 4V 4V 4V 4V 4V 5V 5V 5V 5V 5V 5V 6V 6V 6V 6V 6V This person spent a year in a nonfinance job, then worked 13 years as a level-4 analyst in a very large firm, was finally promoted to a level-5 assistant treasurer position, and eight years later made it to a level-6 position as corporate treasurer

* Organization size codes are based on the organization's relative standing within the industry in a given year. Size is measured by sales, except commercial banks, which are measured in assets.

most powerful predictor of a given job's compensation (William M. Mercer, Inc. 1993; Top Executive Compensation 1994). The traditional sociological measure of size, number of employees (Stolzenberg 1978; Kalleberg and Van Buren 1996), is a poor indicator of a given job's importance because many companies have downsized (Useem 1996). In fact, management consultants warned that an organization with a very large number of employees could be considered bloated and lend less rather than more prestige to executive positions. Instead of using the number of employees,

I measure commercial banks in terms of assets, and I measure financial services, diversified services, and manufacturing companies in terms of annual sales. These are the units most widely used by business managers themselves.

I explored several ways to code organization size and to control for the expansion of American companies over time. I finally decided on a size scale of organizations coded by their standing in their industry in a given year relative to industry leaders, the Fortune 500 manufacturing and 500 service companies. These categories are easy to interpret, and they account for the variation predicted by the experts. They also align well with discrete breaks in my data's size distribution.

I assigned each organization one of four size codes (see table 1). I coded an organization "V" for very large if its size is greater than the size of the median Fortune company in its industry the year measured. I coded it "L" for large if the organization is smaller than the median but still within the size range of the Fortune companies and "M" for medium if the organization is smaller than the smallest Fortune company. I coded organizations "S" for small if they were very small across the distribution of organizations.⁷ To be coded "V" or "L," an organization does not have to be actually on the Fortune list, which excludes closely held companies.

I arranged each respondent's jobs in a sequence of numbers, with one job coded each year from age 22 to 1994. Careers are coded each year by job code (first digit) and organization size (following letter). Job level and organization size can vary independently of each other.

Each respondent's career is coded into a sequence with job level each year as the first digit and organization size as the following letter. For example, consider the following sequence: nf 4V 4V 4V 4V 4V 4V 4V 4V 4V 4V 4V 4V 4V 5V 5V 5V 5V 5V 5V 5V 6V 6V 6V 6V 6V. This person, case 12, spent a year in a nonfinance job, then worked 13 years as a level-4 analyst in a very large firm, was finally promoted to a level-5 assistant treasurer position, and eight years later made it to a level-6 position as corporate treasurer.

Career slope.—Successful finance careers are organizationally and normatively oriented upward over time. One way to think about career advancement is with the "career slope." The career slope is not a precise measure but rather a rough heuristic to help us assess the rate of job mobility. The overall career slope is simply the highest *finance job* level achieved minus the starting finance job level, divided by the number of years it took to reach the highest job level. The calculation ignores jobs

⁷ In 1993, organizations with under \$50 million in sales are coded as small.

outside the finance labor market. To calculate career slopes, I first translated each job level into a value that represents the average number of years it took sample members to reach that level. In other words, the slope calculations are weighted to incorporate the empirical fact that certain job transitions (e.g., from level 4 to 5) are generally made in less than half the time than other transitions (e.g., from level 5 to 6). The slopes of overall careers in the sample range from 0.38 to 2.10, with a median of 0.93 and a mean of 0.98. In addition to discussing overall career slopes, the article will report the slopes of particular career segments in order to explore changes in the rate of mobility across particular time periods or across the duration of a career.

Optimal Matching Techniques

After coding job and family steps into strings of events over time, I analyzed these steps with optimal matching techniques (see Sankoff and Kruskal 1983; Abbott and Hrycak 1990). This method uses a metric to develop a measure of distance between the strings of events.⁸ Each pair of sequences has a distance between them that is the minimal sum of the costs of the arithmetic operations required to turn one sequence into the other. Substitution costs are the costs of transforming an event from one sequence into the other, while insertion and deletion costs are incurred when adding or subtracting events from one sequence to make it resemble the other in a given pair. The appendix presents a simplified example.

I analyzed the sequences of job level and organization size codes with Andrew Abbott's *optimize* (ver. 2.17) software program and made three cost adjustments based on theoretical criteria. First, I assigned an extra cost to the transition from outside to inside the finance labor market to reflect the greater barriers to this move compared to a promotion within the labor market. Second, I assigned an extra cost to making a theoretically unlikely jump up several levels in the finance job hierarchy.

The third cost adjustment concerns the cost of insertions and deletions relative to substitutions. Since ages varied greatly, career lengths differed. I did not want sequence length differences to be the main determinant of similarity. It should be "cheaper" to compress or stretch careers with similar positions but different lengths than to turn different jobs into one an-

⁸ The most common sequence metric establishes a "distance" between sequences based on how difficult it is to transform sequences into one another (Levenshtein 1965). The standard algorithm for alignment is the Needleman-Wunsch algorithm, which calculates alignments based on costs associated with substitution and insertion.

other. Hence, I made insertion and deletion costs substantially lower than substitution costs.⁹

The algorithm sums the costs to calculate the paired distances between career lines. I then classified these distances by the clustering techniques in SPSS. Since different cluster algorithms can yield different solutions (Aldenderfer and Blashfield 1984), I explored several distance measures and clustering methods. Average and complete linkage methods produced clusters that differed in only minor ways. The squared Euclidean distance and a complete linkage method yielded the most easily interpretable clusters. The jump in agglomeration coefficients suggests a five-cluster solution.

PATTERNS AND THEIR ORIGINS

Although many senior women in finance believe their career paths are the result of flukes and accidents, I find that the careers cluster into a few general types. The trajectories are shaped by firm ILMs, environmental turbulence, geographic mobility, and entrepreneurship.

Description of Career Patterns Revealed in Clusters

Figure 1 presents career sequences making up the five-cluster solution.¹⁰ Sequence numbers refer to job-level codes (the first digit) and organization-size codes (the following letter) in table 1. The sequences are justified along the right margin and end at the interview date in 1994. Each sequence begins when the respondent was age 22. Each cluster contains careers of different lengths, as I deliberately set the insertion, deletion, and substitution costs so that sequence length would not be the main determinant of similarity. In the figure, the vertical lines surrounding the years 1970–73 demarcate a historical watershed that I will discuss below.

⁹ Substitution costs range from 0 to 1.00. The cost of moving into the finance labor market contributes 19% of the total possible substitution cost of 1; the cost of making a theoretically unlikely jump contributes 26% of the total possible cost, and the matrix of 1 minus the probability in the data of a transition from one particular job level/organization size state to another contributes 55% of the total substitution cost. I explored several options and found that an insertion and deletion cost of 0.48, which is just under half the largest substitution cost, created meaningful alignments not overly reliant on either substitutions or insertions and deletions. The substitution cost matrix is in table A1 in the appendix.

¹⁰ An independent samples *t*-test suggests that the clusters represent real clumping of the data. The group of within-cluster distances and the group of between-sample distances were each fairly normally distributed. The difference between the mean of within-cluster distances and the mean of between-cluster distances was statistically significant ($P \leq .001$).

Clusters 1 and 2 show orderly careers progressing up the finance job hierarchy. Cluster 1 groups the careers moving up in very large firms (V). Cluster 2 groups the careers advancing in large firms (L). These are the corporate climbers.¹¹ Only eight of the 26 sequences in clusters 1 and 2 spent over a year in jobs outside the finance labor market (nf, fe) before entering finance.

Cluster 3 contains careers that advance in medium and small organizations. All but one career here has reached job level 7, a higher level than most of the corporate climbers have attained. We can call cluster-3 careers big fish in small and medium-sized organizations. The cluster-3 careers are less orderly than those in clusters 1 and 2, as most cluster-3 respondents shift between different size firms during their careers. Four of the nine careers began in nonfinance jobs.

Cluster-4 careers appear the least orderly. Of the 15 careers, 10 began outside the finance labor market. Not only do they move around differently sized organizations, six careers skipped over entire job levels (starred), and five careers go through demotions (underlined). They have reached high-level positions in large firms: seven out of the 15 careers have reached level 7 or higher, and 12 out of 15 work in large or very large organizations. They have thus used these moves to great advantage and may be considered movers and shakers.

Cluster 5 contains six out of the nine entrepreneurs in the sample. Three entrepreneurs are clustered elsewhere because they owned their own businesses for a short time and mostly resemble careers in other clusters.

In sum, senior female finance careers fall into a few general patterns with minor variations. Almost half of the careers are corporate climbers: they move steadily up the job hierarchy in large organizations (clusters 1 and 2). About 17% are big fish that have reached high levels in medium and small organizations (cluster 3). Over one-quarter, the movers and shakers, have shown great mobility by switching among different size firms, skipping up or down job levels, and from outside the finance labor market (cluster 4). Of the respondents, 16% started their own firms; two-thirds of these entrepreneurs are grouped in cluster 5. Almost half the careers begin outside the finance labor market, and half of these began in female-dominated occupations.

Forces Creating the Patterns

Although I explicitly clustered the careers on the basis of job level and organization size, they turn out to be patterned by an underlying factor: the degree of career orderliness (cf. Wilensky 1961). Orderliness is shaped

¹¹ I thank an anonymous reviewer for suggesting the labels for the four career types.

by the number and type of employer organizations, environmental uncertainty, geographic restrictions on mobility, and entrepreneurship.

Orderly careers and firm internal labor markets.—Conceptually, the ideal-type of an orderly career advances smoothly along a foreseeable path, such as an ILM (cf. Kerckhoff 1995, p. 337). Interorganizational transitions are rare and not forced by an unexpected merger or bankruptcy. Orderly careers allow long-term planning. In contrast, the ideal-type of a disorderly career shifts between disparate fields and among several different organizations. Contingencies cause unplanned job changes. Geographical ties to a spouse's career can also cause unplanned job shifts or can prevent one from accepting a promotion

Table 2 presents several dimensions of orderliness and disorderliness of careers within clusters. These dimensions indicate that clusters 1, 2, 3, and 4 are on a continuum, with clusters 1 and 2 being the most orderly and cluster 4 the most disorderly. (I discuss the entrepreneurial cluster 5 separately below.)

Working in a small number of organizations and spending most of one's career with one employer are dimensions of career orderliness.¹² The first column of table 2 shows that cluster-1 members worked for a median number of two employers, cluster-2 respondents worked for a median of just one employer, and clusters 3 and 4 worked for a median of three employers during their finance careers. The second column displays the proportion of careers in each cluster with over half their length in one organization. This indicator controls for career length, and it decreases monotonically across clusters 1–4. Almost 80% of cluster-1 respondents and over 70% of cluster-2 respondents spent over half their finance careers with one employer. This percentage falls to 56% of cluster 3 and drops to just 20% of cluster 4.

These indicators suggest the presence of firm ILMs. Since the careers are by definition successful, careers concentrated in one organization have probably advanced up firm job ladders (Althauser 1989; Althauser and Kalleberg 1990). Firm ILMs characterize most of the careers in clusters 1 and 2, about half the careers in cluster 3, and few of the careers in cluster 4.

Disorderliness, turbulence, and career advancement.—A manager in finance could make several strategic organizational changes and still have

¹² All calculations are as of the interview date in 1994 and are right truncated. Careers with high values on the disorderly indicators are already disorderly, regardless of future career moves. Careers with high values on the orderly indicators could conceivably become more disorderly later, especially if faced with severe environmental shifts. The youngest respondents' careers are the most severely affected by right truncation. However, the shortest career is already 14 years long and is probably unlikely to depart drastically from its already established pattern.

TABLE 2
ORDERLINESS AND DISORDERLINESS INDICATORS AFFECTING PROPORTION OF CAREERS IN EACH CLUSTER

Cluster	Median Number of Organizations in Finance Career	Proportion of Careers with over Half Length in One Organization	Proportion of Careers with < 7 Years on Average per Organization	Proportion of Careers Facing Environmental Turbulence	Proportion of Careers Geographically Restricted	Proportion of Careers Beginning outside Finance
1 (19)	2	.79	.32	.21	.21	.26
2 (7)	1	.71	.14	.14	.29	.43
3 (9)	3	.56	.44	.67	.33	.44
4 (15)	3	.20	.60	.73	.47	.66

NOTE.—Ns are given in parentheses. Clusters 1 and 2 are corporate climbers, cluster 3 is big fish in small and medium-sized organizations, and cluster 4 is movers and shakers

an orderly career. But if she changes employers too frequently, her career will look disorderly. Column 3 of table 2 shows the proportion of careers in each cluster that spent an average of six years or less in each organization. Less than a third of the careers in clusters 1 and 2, almost half the cluster-3 careers, and 60% of cluster-4 careers changed firms frequently.

One cause of a disorderly career is turbulence in the organization, industry, or wider economy. Some careers face a particularly high degree of environmental turbulence, including mergers, restructuring, severe competition, and employer bankruptcy. Of the careers in the total sample, 22 (39%) were directly affected by environmental turbulence. Turbulence can have negative effects on careers, such as job loss or demotion. It can also have positive repercussions, such as the creation of new opportunities when companies restructure.

The fourth column of table 2 reports the proportion of careers affected by turbulence. Only five of the 26 respondents in clusters 1 and 2 (21% and 14% respectively) faced environmental turbulence, while the other careers were buffered from extreme uncertainty. In contrast, 11 out of the 15 careers in cluster 4 were affected by turbulence.

Over half of the turbulent careers in the total sample are in financial services. Financial service firms in the sample tend to be smaller than the firms in other industries and, in the 1970s and 1980s, particularly vulnerable to volatile profits, mergers, and acquisitions. Yet surviving firms made extremely high profits in the mid- to late 1980s (Gart 1989). Respondents in financial services undergo more career disruptions but also enjoy higher compensation than do executives in other industries (Top Executive Compensation 1994).

Of the 22 turbulent careers in the sample, 11 had career slopes below the sample median. Six of these were in the lowest quartile. These careers were particularly hurt by turbulence. But nine turbulent careers ascended faster than the median slope, and six were in the highest quartile. In fact, these fast-paced turbulent careers beat the overall advancement rate of 15 of the 19 careers safely ensconced in stable firm ILMs. Although the employment security and promotion opportunity characterizing many ILMs is assumed to benefit employees (Doeringer and Piore 1971), there are circumstances in which turbulence breaks the implicit ILM contract and creates even better opportunities for some workers.

Environmental turbulence affected about three-quarters of the cluster-4 careers, most of which were in the financial services sector. Turbulent careers may have been a norm during the 1980s for financial services managers and bankers of either gender. Respondents in cluster 4 were generally very successful; half had reached the top two job levels (7 or 8) in large organizations. Most of these women used the turbulence to their

advantage and landed on their feet after a merger, downsizing, or bankruptcy.¹³

For example, a former banker interpreted the acquisition of her bank a few years ago as "a signal to myself that it was time to move, to do something more challenging," and she landed a more interesting job in financial consulting. Similarly, a second respondent remarked that "it was a break" that her company was sold in the mid-1980s. She continued, "I didn't plan on it, but it was a fortunate that the company was sold and I had to find another job. . . . I was recruited to become director of investor relations at American Foods. It was a great opportunity. It was a big jump up in pay. I went from a \$1 billion company to a Fortune 500 company."

A third respondent benefited enormously from a mid-1980s merger of her financial services firm with a broker/dealer organization. She was promoted from controller of the financial services firm to the CFO of the newly combined firm. She said: "It was the biggest promotion and the biggest vote of confidence I'd ever received." Although cluster-4 careers appear disorderly in terms of frequent employer change, given the turbulence of the financial services industry, they may be modal careers among successful financial services executives.

Curiously, the monotonic relationship between cluster, the median number of employers, and the proportion of careers facing turbulence breaks down between clusters 1 and 2 (table 2). At first glance, the very large firms in cluster 1 seem to provide less stable environments than the large firms in cluster 2. This surprising pattern may be due to the failure of one very large bank in the 1980s. Three of the four careers experiencing turbulence in cluster 1 (as well as six careers in other clusters) had worked for this one very large organization. Bankruptcies of very large banks are fairly rare, but when they occur, they affect huge numbers of employees. This bankruptcy galvanized six respondents to leave commercial banking for the more lucrative financial services industry, which was just starting to open its doors to women in the early 1980s.

Restrictions on geographical mobility.—Geographical restrictions due to family relationships also shape career patterns. Relocating to accommodate a spouse's career or turning down an opportunity because it would entail relocating one's family can disrupt orderly advancement. Women are more likely than men to have geographical limits on their careers (Markham 1987; Steil and Weltman 1991). Of the sample, 34% reported restrictions in a third or more of their years in finance.

Table 2, column 5 shows that the proportion of careers with geographi-

¹³ Women whose careers were seriously damaged by turbulence may have been selected out of the sample.

cal restrictions increases monotonically by cluster. Only a fifth of the orderly cluster-1 careers faced geographical limitations, while almost half of the disorderly cluster-4 careers were geographically circumscribed. Cluster-4 respondents seem remarkably agile at creating career opportunities in the midst of geographical restrictions and environmental turbulence. Other women may have been less able or less lucky in exploiting limited mobility and may have seen their careers stall; these women would have been selected out of my sample.

Beginning careers outside the finance labor market.—The proportion of careers beginning outside the finance labor market increases monotonically across clusters 1–4 (table 2, col. 6). Only a quarter of cluster-1 careers started out in nonfinance jobs, whereas two-thirds of cluster-4 careers began outside finance. Beginning one's careers outside the finance labor market is another dimension of disorderliness. We will see below that this factor is sensitive to the time in which the finance career began.

Time spent working part-time or out of the labor force.—There is much debate over whether companies should structure career ladders to allow managers to take time off or to work part time. Supporters of these policies argue that they benefit women with children, while opponents fear that they marginalize women in lower-paid tracks. It is interesting to see if women who are now senior managers ever spent time as homemakers or worked part time.

Only one respondent (fig. 1, cluster 4, case 20) spent at least a year as a full-time homemaker, and that was before she began her finance career. Only two respondents worked part-time after launching finance careers. One woman (cluster 3, case 4) works in a financial services firm that she describes as very supportive and that continued to promote her despite her part-time status. She enjoys a rapid career pace in part because the firm did not penalize her for working part-time and in part because the firm is growing. The other part-timer (cluster 3, case 17) says her financial services firm has a history of hostility toward women. She faced a decided lack of support from the firm at the news of her pregnancy and at her request to work four days a week after her daughter was born. Her career pace is just below the sample median.

With very few exceptions, labor force interruptions and part-time work are simply not part of the career tracks of this sample of women at senior levels. Executive finance careers apparently do not easily tolerate spells out of the full-time labor force. Respondents with children repeatedly said that most of their friends who are mothers had left similar jobs to raise their children. The decision to work part-time or take time off is a difficult one, and most women believe that their careers would suffer if they relinquished their full-time commitment to their employers.

Entrepreneurs.—Cluster 5 in figure 1 groups entrepreneurs. Three of

the six cases are *long-term* entrepreneurs; they started their own businesses in the 1970s and early 1980s. The other three cases are *recent* entrepreneurs who started firms in the late 1980s and early 1990s. Three additional women in other clusters are also recent entrepreneurs.¹⁴

The recent and long-term entrepreneurs described different motives for starting their own firms. All three long-term entrepreneurs stated that they founded their own businesses for a combination of two reasons: to get around sex-based hurdles in large organizations and to create the flexibility to accommodate child care or a husband's geographical preference. For example, in 1976, one long-term entrepreneur (cluster 5, case 51) said she was forced to leave a large public accounting firm after marrying a colleague. The firm did not allow married couples both to be on staff and assumed that she rather than her husband would leave. She decided to start her own business at home where she says "she could start a family and still keep active in the profession."

In contrast, the six recent entrepreneurs told me that they disliked large corporate bureaucracies for reasons unconnected with gender-based barriers or family. They founded businesses in the pursuit of autonomy, productive work, and profit.

To illustrate, one recent entrepreneur (cluster 5, case 29) discusses the contrast between her former job in a Fortune 500 company and her new job as cofounder of a small business.

It was appealing to get away from the stupid, fruitless memos and meetings. I'd just had it with the corporate world. No one was looking at the big picture. There were just pathways of paperwork. People wanted things done the way they'd always done them. They didn't care about seeing results. . . . [In my own firm] I haven't written a memo yet. You don't have 10 people posturing and defending their territory. You can see immediate results of anything you do, without going through 20 levels of bureaucracy and red tape. . . . The connection between what I do and the bottom line is so direct here. It's really entrepreneurial. My personal financial success is directly connected to the success of the company, which is directly connected to my work.

This contrast between long-term and recent entrepreneurs underlines the importance of time in women's self-understandings of their careers. Many women starting finance-related firms 15 or more years ago said they were trying to bypass gender-based barriers to their advancement in large companies and to accommodate their families' needs. In contrast, women

¹⁴ Cluster-5 long-term entrepreneurs are cases 39, 51, and 28, and the recent entrepreneurs are cases 25, 9, and 29. Other entrepreneurs are cases 3 and 19 (cluster 1) and case 6 (cluster 2). Most of their careers resemble the other careers found in clusters 1 and 2.

launching finance-related businesses more recently report they are motivated by the desire to escape bureaucracies and to create new business opportunities. Ostensibly similar career steps can have different meanings depending on when the steps occur.

It is interesting that the career slopes of the recent entrepreneurs (median 0.89, mean 0.96) are almost identical to the slopes of the long-term entrepreneurs (median 0.80, mean 0.96) and similar to the career slopes of the sample as a whole (median 0.93, mean 0.98).¹⁵ The career slopes do not support the interpretation that more senior entrepreneurs had actually faced more restricted career opportunities or slower advancement than had new entrepreneurs. Yet the long-term entrepreneurs interpreted career opportunities and barriers differently than did recent ones.

In sum, factors distinguishing clusters include firm ILMs, turbulence, geographic mobility, the boundary separating jobs outside and inside the finance labor market, and entrepreneurship. These factors are interrelated in their effect on career trajectories. Respondents who climb ILMs in one or two large organizations are somewhat insulated from environmental turbulence; these corporate climbers are concentrated in clusters 1 and 2. Cluster 3, the big fish in small and medium-sized organizations, contains careers that have experienced more turbulence and more mobility among firms and have reached fairly high levels in small and medium-sized firms. Cluster 4 groups careers with even more environmental turbulence and more frequent employer change. These characteristics are correlated with the risky financial services industry and with geographic restrictions. Cluster-4 respondents are successful risk takers, the movers and shakers of the sample. Cluster 5 contains the entrepreneurs.

The remainder of the article focuses on female executives' new opportunities, beginning in the 1970s, that crosscut the factors such as firm size, turbulence, and geographic mobility that shaped career types. The first section below discusses industry changes that I assume affected both female and male managers' careers. The following section will then focus on legal and social changes that specifically benefited *female* managers in finance.

CHANGES IN OPPORTUNITY STRUCTURES

Changes in Finance-Related Industries

We have seen that over a third of the sample encountered environmental turbulence; this turbulence particularly affected careers in financial ser-

¹⁵ I measure the career slopes of entrepreneurs up until the point in time when they formed their own businesses.

vices.¹⁶ In addition to financial service firms (including investment banks, brokerage firms, and financial consulting firms), commercial banks, public accounting firms, and other finance-related businesses faced global challenges that eroded profit margins in the 1970s and early 1980s. I expect that the increased competition and volatility during this period would have constricted career opportunities for female and male managers in finance-related fields.¹⁷

The 1970s and early 1980s were plagued by high, volatile interest rates and a sluggish stock market. This was also a period of increasing globalization, deregulation, and competition. Commercial banks lost money on loans made to developing countries hit by high energy prices and high inflation. Investment bankers reeled when deregulation opened up price competition on broker fees. In short, commercial and investment banks saw their profits shrink in the 1970s. Similarly, public accounting faced heightened competition in the 1970s and 1980s, which resulted in mergers of public accounting firms, slowing growth rates and dropping relative salaries. We will see that women's career pace accelerated in the 1970s and early 1980s *in spite of* these problems in commercial banking, financial services, and public accounting.

The 1980s continued to be difficult years for commercial banks, which lost their monopoly on demand deposits to savings and loan associations and ceded corporate loan market share to investment banks. There were an unprecedented number of bank mergers and acquisitions.

Investment banks also faced record numbers of failures, mergers, and acquisitions in the 1980s. But surviving investment banks were highly profitable between 1982 and the 1987 stock market crash. The mid-1980s were years of expanding product innovation, securities sales, globalization, and competition. We will see that these six years of expansion in financial services occurred after the increase in women's career pace had already begun and therefore cannot explain this increase. However, this expansion may have contributed to the acceleration in some women's advancement and also helps account for the timing of women's entrance into financial services. Of the 17 respondents who worked in financial services at some point in their careers, 15 entered this industry in the 1980s.¹⁸

¹⁶ Sources for the industry trends discussed in this and the following two paragraphs are Altman (1987), Rose (1987), Gart (1988, 1989), Taylor (1991), Flynn et al. (1995), Useem (1996), and Stearns and Allan (1996).

¹⁷ A study of male finance-executive careers is beyond the scope of this paper. I am currently conducting research on this population.

¹⁸ The concern that industry expansion created the increase in career pace might apply to corporate law firms, which expanded in the 1970s through the late 1980s (Epstein

Increased Opportunities for Women

This section shows that in spite of economic contractions and volatility in commercial banking, financial services, and public accounting, opportunities for female managers in finance-related fields increased after the early 1970s. This advancement is in large part due to dramatic legal and social changes that dismantled some of the explicit barriers to women's mobility.

Congress passed legislation protecting women's employment rights in the 1960s, but this made little difference to employed women until the early 1970s. For example, Title VII of the Civil Rights Act of 1964 provides the federal basis for protection from employment discrimination based on race, sex, and other criteria. However, the Equal Employment Opportunity Commission (EEOC) initially lacked any intention to protect women's rights and ignored complaints of sexual discrimination (Mills 1994). In response to this disregard, Betty Friedan and two dozen others founded the National Organization of Women (NOW) in 1966 (Ferree and Hess 1985; Freeman 1973). By the early 1970s, women's groups successfully pressured the federal government to enforce the laws against sexual discrimination. The EEOC and the courts gradually insisted that employers hire women into traditionally male occupations (Costain and Costain 1987; Cancian 1981; Kessler-Harris 1994). For example, the EEOC collected data on and filed lawsuits against some banks (Ashenfelter and Hannan 1986; see also Reskin and Hartmann 1986).

Respondents discussed how these changes affected their own careers. Some were in the first sex-integrated cohort of management trainees. Others said they were promoted because of EEOC pressure. For example, one respondent who joined a very large bank in the early 1970s noted that "commercial banks were in the forefront of promoting women; they were under much more EEOC pressure." She elaborated that the increase in women's career opportunities was "an EEOC issue; it was not just a demand issue."

We might therefore expect that respondents' careers will reveal a period effect in the early to mid-1970s. The data reveal distinct shifts in the distributions of three indicators. The indicators are: (a) the number of women entering the finance labor market (including business school) each year, and two subsets of (a): (b) the number leaving nonfinance jobs to enter finance; and (c) the number entering full-time MBA programs. There was a sharp increase in the rates of all three factors from the pre-1970 period to the 1970-73 period. The rate of women entering finance from non-

1993; Curan and Carson 1988). I deleted the seven respondents who worked in law firms from the slope calculations and found virtually identical results.

finance jobs went down slightly after 1973. But the other two indicators rose again from the 1970–73 period to the 1974–80 period. Taken together, the three indicators suggest a shift from the first (1960–69) to the third (1974–80) period, with the second period (1970–73) as a transitional period between them.

National events support the data's picture of a watershed between 1970 and 1973. In 1970, the EEOC finally signaled seriousness in applying Title VII to sexual discrimination by bringing suit against the American Telephone and Telegraph Company (AT&T), the largest private employer of women in the country, for sexual discrimination. A consent decree stipulated affirmative action at AT&T and \$15 million in back pay to female employees (Crothers 1973). In 1973 and 1974, consent decrees were negotiated with several other large employers, including the Bank of America (Ashenfelter and Hannan 1986; see also Reskin and Hartmann 1986).

An exponential increase in NOW membership and in media coverage of the women's movement was also marked in 1970 (Cancian and Ross 1981; Freeman 1987). In 1972, Congress passed the Equal Rights Amendment as well as Title IX of the Higher Education Act, which prohibited sexual discrimination in school admissions. There was a concurrent surge in female enrollment in business and business schools that prepared the way for women's subsequent movement into high-paying, male-dominated, business and professional occupations (Cancian 1981; Shu and Marini 1998). Furthermore, "1970 marked the turning point as massive demonstrations were held across the country on the fiftieth anniversary of women's suffrage. Women who had not been active before marched and picketed. . . . Feminist protests spread through the churches, the professions [and] academic disciplines" (Mueller 1987, p. 97).

By triangulating the distributions of my data indicators with watershed national events, we see that the years 1970–73 mark a distinct period in American women's history. In sum, the first period (1960–69) occurred before the women's movement became widespread and before the EEOC signaled its seriousness in policing sexual discrimination. The second period (1970–73) is a transitional stage marked by a sharp rise in the mass base and activism of the women's movement and by increased media attention. The third period (after 1973) occurred after these changes had begun to be institutionalized.

To capture any impact of this period effect on women's finance careers, I divide the sample into three finance cohorts. Finance cohorts are defined by the year a respondent began her finance career, including professional school. The first finance cohort began finance careers between 1960 and 1969, before the federal government enforced women's rights to equal opportunity in the workplace. The second finance cohort started finance careers during the transitional period of 1970–73. The third finance cohort

TABLE 3

MEDIAN AND RANGE OF CAREER SLOPES IN EACH FINANCE COHORT

	1 (1960-69)	2 (1970-73)	3 (1974-80)
Median66	.94	1.06
Mean69	1.01	1.06
Range46-.95	.71-1.45	.38-2.10
N in each finance cohort	10	11	35

NOTE —Years in which cohorts entered finance labor market are given in parentheses

started finance careers between 1974 and 1980. Note that members of the same finance cohort can be of different ages, as some people started finance careers directly after college, while others first worked in nonfinance jobs.

Career Advancement

The initial hypothesis is that the period in which a woman begins a finance career affects the subsequent shape of her career. To explore this, I first examine the rate of advancement over respondents' total finance careers (excluding spells outside of finance). Then I analyze advancement over finance-career segments during shorter time periods.

Overall career slopes.—The analysis of promotion rates over the total career uses a heuristic measure of career pace, the career "slope." Table 3 shows a distinction between the slow-paced, shallow ascent of the first finance cohort (median slope 0.66, mean 0.69) and the faster career ascent of the second finance cohort (median 0.94, mean 1.01) and third finance cohort (median and mean 1.06). Overall, career slopes increased abruptly between finance cohorts 1 and 2 and remained fairly constant thereafter.¹⁹ One explanation is that women starting finance careers in the second and

¹⁹ A difference of means *t*-test showed that, as expected, the mean slope of the first graduation cohort was significantly lower than the mean second-cohort slope ($P \leq .01$) and lower than the mean third-cohort slope ($P \leq .01$). A one-way ANOVA test gave similar results. Since the distribution of total slopes was skewed to the left, I did a Kruskal-Wallis test, which also showed significant differences among the graduation cohorts ($P \leq .01$). A square-root transformation of the slopes created a more normal distribution. Statistical tests on the transformed slopes gave the same results as the untransformed slopes. Separately calculating the slopes of careers *reaching each job level* shows that *at each level* the slope range also increased between cohorts 1 and 2 but remained similar across cohorts 2 and 3. The issue of right truncation and the career slope is addressed in the section on "the right-truncation problem," below.

third periods advanced more quickly because the EEOC began pressuring companies to promote women in the early to mid-1970s. This interpretation aligns with the historical discussion of the women's movement and Title VII, and it is supported by the interview data presented below.

The next section will show that these cohort differences between overall slopes are partly due to career age effects (seniority). We will also see evidence of a period effect: on average, finance cohorts 2 and 3 have faster advancement than finance cohort 1 across each career stage.

Slopes of career segments.—To more closely examine the interrelated effects of age, period, and cohort, I divided the overall careers into career segments corresponding to particular time periods. Ideally, these periods would be small enough to isolate the period of theoretical interest: 1970–73. However, the division of careers into four-year segments (e.g., 1966–69, 1970–73, 1974–77, etc.) was empirically too small to see mobility. A majority of careers showed no change (a zero slope) during most periods of just four or five years.

Ensuring that the majority of careers in a given period would have been promoted during the period required longer career segments. I compared the finance-cohort slopes across career segments during five overlapping time periods that each lasted about a decade: prior to 1970–75, 1970–80, 1975–85, 1980–90, and 1985–94. Table 4 presents the median slopes of 12 career segments, which are each composed of distinct cohorts across each time period. (The mean slope of each career segment is similar to the median). Although the cells of this table are very small, they reveal period, age, and cohort effects.²⁰

Period effects emerge when we compare the medians of career-segment slopes of the same finance cohort across the different time periods. Career segments 1, 2, 3, 4, and 5 track the career-segment slopes of the first finance cohort over the five time periods. The first finance cohort had the lowest median career segment slope, 0.16, during the earliest period (before 1970–75, career segment 1). The first cohort median career pace increased across the next two time periods, peaked at 0.73 during the 1975–85 period (segment 3), then leveled off and dropped.

In the first decade of the second finance-cohort's careers, 1970–80, careers show a median slope of 0.50 (career segment 6). By 1975–85, the median pace had peaked at 1.09 (segment 7). Thus both the first and second finance cohorts experienced heightened mobility between 1975 and 1985. This is consistent with my argument that legal and social changes of the early 1970s began to dismantle explicit gender barriers to women's advancement in finance. As business schools reckoned with Title IX,

²⁰ One-way ANOVA tests showed statistically significant difference of means among career segment slopes ($P \leq .001$).

TABLE 4
SLOPES OF CAREER SEGMENTS BY FINANCE COHORT*

Career Segment	Finance Cohort	N Careers in Segment	Time Period	Median Slope of Careers in Segment†	Shift in Median Slopes across Career Segments
1	1	10	<1970-75	.16	
2	1	9	1970-80	.45	from segment 1 to 2: .29
3	1	9	1975-85	.73	from segment 2 to 3: .28
4	1	9	1980-90	.73	from segment 3 to 4: 0
5	1	8	1985-94	.40	from segment 4 to 5: -.33
6	2	11	1970-80	.50	
7	2	11	1975-85	1.09	from segment 6 to 7: .59
8	2	10	1980-90	.86	from segment 7 to 8: -.23
9	2	7	1985-94	0	from segment 8 to 9: -.86
10	3	34	1975-85	.48	
11	3	34	1980-90	1.09	from segment 10 to 11: .61
12	3	32	1985-94	.80	from segment 11 to 12: -.29

* Finance cohort 1 members entered finance before 1970, finance cohort 2 entered finance between 1970 and 1973, and finance cohort 3 entered finance between 1974 and 1980. Each career segment excludes any respondents who were entrepreneurs at the starting or ending date of the time period, as I was unable to calculate their career-segment slopes

† Mean career slopes of each segment create the same pattern

women began flooding into MBA programs in the early 1970s. As banks and other financial services firms began responding to the enforcement of Title VII as newly applied to women, their management training programs became sex integrated.

In addition to this period effect, we can also see the inverted U-shaped effects of career age or seniority.²¹ Table 4 shows that all three finance cohorts increase the pace of mobility after the first five years. This acceleration of career pace with career age peaks at midcareer (segment 3 for the first finance cohort, segment 7 for the second cohort, 11 for the third cohort) and then levels off and declines with seniority. This is probably because the hierarchical structure of firms limits opportunities as employees approach the apex of the organizational hierarchy (cf. Stewman and Konda 1983).

Career pace shows the effects of period as well as career age. The first finance cohort starts out more sluggishly (median slope 0.16) than the second and third cohorts start out (median slopes 0.50 and 0.48). Moreover, the first cohort never accelerates as quickly (median slope 0.73, career segment 3) as the second and third finance cohorts (median slope 1.09, segments 7 and 11). Through almost every stage of seniority, median and mean career pace for the first finance cohort lag behind the younger cohorts. First finance-cohort members continue to pay a penalty for gate-crashing finance-related managerial occupations before the doors had officially opened to women.

As discussed earlier, the first and second cohorts both accelerated fastest during the 1975–85 period (career segments 3 and 7). This is due to the combination of period and age effects. These career segments showed the highest mobility because they had the dual advantage of following the early 1970s watershed and being the segments of mature careers.

A competing explanation for third cohort members' rapid career advancement is that they are more valuable employees than women in the earlier cohorts. Since the enforcement of Title VII and Title IX created new openings for women in business schools and organizations, there were more women competing for finance-related jobs in the 1980s than in earlier decades. The third finance cohort is a more selective sample than the earlier cohorts in the sense that its members beat out more female competitors for high-ranking jobs (cf. Kanter 1983).

Yet there are two problems with this explanation. The second finance cohort advances, on average, just as quickly as the presumably more com-

²¹ Recall that career age is different from chronological age, since respondents were of different ages when they began their finance careers. The section on "converging careers," below, considers cohorts as defined by chronological age, specifically by college graduation date.

petitive third cohort. Moreover, the first and, to a lesser extent, the second finance cohorts are selective in another way: they created opportunities for themselves when institutionalized access to managerial finance jobs was almost nonexistent for women. Although the third finance-cohort respondents competed with more women for jobs in the finance labor market, second- and especially first-cohort members had to fight to create those jobs.

Affirmative Action, Period, and Cohort Effects

The new attention to women's employment rights in the early 1970s affected respondents differently, depending on when their finance careers began. First finance-cohort women began finance careers before institutional channels into finance careers were sex-integrated. They had somehow convinced large firms to hire them and had then accumulated several years of experience by the time the EEOC began pressuring large companies to promote more women.²² In the early 1970s, first finance-cohort respondents were thus positioned finally to receive their first significant promotions. Second finance-cohort women, who launched finance careers between 1970 and 1973 found affirmative action helped them gain access to business and law school and to entry-level finance positions.

Older members of the third finance cohort, especially those who first worked in other fields, said that the women's movement influenced their decision to go into finance; some found affirmative action measures important in opening entry-level finance positions. In contrast, younger third-cohort women took access to business school and finance jobs for granted. Most did not expect or perceive any limits to their advancement, although some women encountered sex-based barriers after they had reached senior levels. The interviews illustrate these time-dependent patterns. (All names are pseudonyms.)

First finance-cohort women gained unorthodox entry into finance ILMs in the 1960s and then benefited from affirmative action in the 1970s. Louise Gelb started a clerical job at a very large bank in 1960, made herself indispensable, and then threatened to quit if she were not moved into a finance-related job. Her "back-door" entry into the firm ILM allowed her to benefit from the company's later concerns about affirmative action. In 1974, she was the first woman promoted to assistant vice president in accounting. She discusses this transition: "It was a major career step. The

²² First-cohort women are overrepresented in the first and second clusters, the climbers of ILMs in big firms.

time was ripe for women to be looked at. I was visible in the bank and in the community. Other senior officers were aware of me. It was an opening.”

Similarly, Shari Roberts describes how she got a clerical job in a large manufacturing company in 1961 and fought hard to break into management: “When I first started with the company in 1961, women only did clerical work. . . . In 1966, I threatened to leave. I had three other job offers. I was not going anywhere at my company. Then they promoted me to administrative services managers. They met the other offers, and I stayed. Percent-wise, it was a sizable salary increase.” Once in the ILM, affirmative action finally allowed her to be promoted: “Title VII became important in the early 70s. The company suddenly had an EEOC manager in 1974. In 1974, I was promoted. They looked for women with [college] degrees, and they found two. I was one. In an interview with the treasurer, I asked, ‘Why me?’ He said, ‘If I said it wasn’t because you are a woman, I’d be lying.’ Being female cuts both ways. . . . In the early 70s, women had to be excellent. When the time comes when average women are allowed to achieve in the workforce, we will have progressed a long way.”

Louise Gelb, Shari Roberts, and most other first finance-cohort members found a back-door entrance into a finance ILM. They maintain that the women’s movement had helped open workplace barriers after they had already gained some experience in the finance labor market.

In contrast, second finance-cohort women credit the women’s movement and affirmative action for helping them *enter* the finance labor market in the early 1970s. Many were pioneers in finance positions and benefited from—and actively contributed to—the continuing pressure to promote women throughout the 1970s. Most second finance-cohort members thought the women’s movement had been pivotal in directing their attention toward the finance labor market and in sex-integrating professional schools and management training programs. For instance, Liz Frank, who attended business school from 1972 to 1974, found that “being a female was a plus for getting into graduate school. The business schools were just starting to recruit women.”

Lisa Mignetti became one of the first two female management trainees in a commercial bank in 1970, when the bank began responding to EEOC guidelines. Although legal pressure got her into the company, she had to struggle for advancement. She discusses gender difference in income and promotion pace: “It was the first year companies were under pressure to hire women. . . . I was hired for \$8,200, which I thought was a lot of money. Then I found out that the male management trainees were paid \$8,600. . . . I kept getting put in charge of silly special projects. In 1971, they made me in charge of bank parties. . . . It’s supposed to take two

years [to become an officer]. But because I kept getting stuck in dumb jobs, it took me four years. . . . I complained a lot. . . . Everyone my age has these tales."

Several years later, Mignetti again discovered that the bank was paying her less than her male colleagues. This time she threatened a lawsuit and negotiated a 50% pay raise. Shortly thereafter, she joined a very large manufacturing company and in 1982 became its first female corporate officer. She says, "I was elected the token woman VP. The men at my level gave me a bus token."

Several older third finance-cohort women started finance careers between 1974 and 1980 after working in other fields. They credited the women's movement for directing their attention to finance-related careers. They recognized a changing regime, which gave women new opportunities.

For example, Dorothy Jones graduated from college in 1969, taught elementary school for a few years, and then decided to enroll in business school. She remarks, "All of a sudden there was a lot of demand for women in business. The Civil Rights Act helped. . . . There were only 13 women in the whole MBA program at the time [1974-76]. . . . Women were hired right away. They got multiple offers."

Harriet Simpson is another older third finance-cohort member who held nonfinance jobs before launching a finance-related law career in 1975. She maintains that a law career had not been a structural or a psychological option for her when she graduated from college in 1966.

[In the early 70s,] I started seeing myself as someone who would be working for the rest of my life. I thought about maneuvering myself for the job with the best rewards and the most pleasure. With proper career counseling, I would've been a likely candidate for law school out of college [in 1966]. But no one even suggested it. . . . The feminist movement has been very important. If I had graduated from college four to five years later, I would have gone straight to law school. . . . To the extent I started thinking of myself differently, things never would've occurred to me without the women's movement.

Simpson's remarks support my delineation of a watershed between 1970 and 1974. Not only workplace structures but also women's aspirations changed dramatically in a small amount of time. Changing structures enabled some women to form new understandings of the social world and themselves (cf. Shu and Marini 1998).

Younger third finance-cohort members entered finance between 1974 and 1980 immediately after college. They generally took their admittance into law or business school or into entry-level finance jobs for granted. Many did not believe that the women's movement was relevant to their

personal success.²³ Some third-cohort women said they had never encountered sex-based barriers to advancement; others said they confronted such hurdles only after reaching senior levels.

Cornelia Urbach, who entered a very large accounting firm in 1975, said that initially "the door was wide open for me." However, her smooth advancement stopped after she became a partner in the late 1980s. She remarked, "There is a glass ceiling, big time. I definitely have the potential to develop, but because of politics and because the world of my peers is male dominated, I am pigeonholed and restricted from developing my fullest potential. The glass ceiling shows up at different levels, for different people. I didn't hit it until I became a partner. As long as you are talented, there's opportunity, but only if the men are willing to give it to you. They have a limit to how far they let you go." Urbach finds herself blocked from pursuing certain business opportunities by what she perceives as subtle yet persistent gender discrimination.

Amy Peterson, another third finance-cohort member who entered a very large firm in 1977, also found her advancement stalled when she reached the level of partner. She attributes this to an increasing industry-wide competition and resulting heightened competition for clients among firm partners. She argues that male senior partners tend to be more comfortable mentoring other men (cf. Kanter's [1977] "homosocial reproduction"). "I think the advantage men have is people like being around people that are like them. And so you get your senior partners who can relate much more easily to the men and so there's much more mentoring going on. . . . Maybe if I had been male all along, I would have had mentors that were making sure I was protected. . . . And making sure I was developing smaller clients I could keep [after I made partner]." Peterson's lack of guidance and personal ties to powerful partners left her vulnerable in an increasingly competitive environment.

While Cornelia Urbach and Amy Peterson found their advancement curtailed after reaching a senior position in the firm, other third finance-cohort members have yet to feel any limit to their success or to encounter substantial gender discrimination. Martha Ungvarsky finished college and joined a financial services firm in 1980, where she is now chief financial officer. She says she has never encountered sexism at the firm: "They always treated me the same as everyone else. . . . As long as you can do the job, they'll give you the opportunity."

Ungvarsky believes that expecting discrimination helps create it. Al-

²³ One younger third-cohort member, now a partner in a very large public accounting firm, stated: "The women's movement was not an influence in my life. It was irrelevant, and I did what I wanted to do."

though she acknowledges that her firm may be unusually enlightened, she sees no reason why women with enough "mental toughness" cannot be successful. "If I had expected discrimination, I would have been resentful. I'd have had a different attitude and wouldn't have worked as hard. . . . You do see successful women. How does that happen? It takes a certain mental toughness to deal with it and not let it affect you. I've had people look at my legs during meetings. I just ignore it and go on. . . . I attribute my success to hard work, to a lot of time and effort. Whatever I did, I'd be successful at it."

For Ungvarsky, overcoming sex-based hurdles is a matter of keeping a positive attitude. In contrast, many first and second finance-cohort members fought sex-based barriers with threats to quit and legal action. Given the different historical context in which they launched their careers, first and second finance-cohort members lack Ungvarsky's faith that personal abilities translate easily into career success. The first and second cohorts' aggressive action contributed to changing workplace structures; the altered, less discriminatory structures in turn shaped the third cohort's perspective and sense of identity.

The impact of affirmative action depended on when women launched their finance careers. The next section shows that the legal and social transformations of the early 1970s particularly structured the careers of the youngest female executives, those who finished college after 1973.

Converging Careers

Although almost half the respondents began their work lives outside the finance labor market, this pattern is almost completely restricted to women who graduated from college in the first or second period. The disappearance of female-dominated and other nonfinance work in successful women's finance careers is linked to the early 1970s period effect. We see this if we divide the sample into three college graduation cohorts that left college and began full-time employment, inside or outside the finance labor market, during the first, second, and third periods.

The first college-graduation cohort includes women born between 1934 and 1947 and who finished college between 1956 and 1969 ($N = 24$). The second college-graduation cohort was born between 1948 and 1951 and finished college between 1970 and 1973 ($N = 12$). The third graduation cohort was born between 1952 and 1959 and completed college between 1974 and 1980 ($N = 20$). (A college-graduation cohort is the same as a birth cohort and is distinct from a finance cohort, as finance cohort members may be of different ages.) The proportion of women leaving college to work outside of finance for more than a year declines in each graduation cohort, from 15 out of the 24 first graduation-cohort members (63%) to

seven out of the 12 second graduation-cohort respondents (58%) to just two out of the 20 third graduation-cohort members (10%). By the third period, working outside the finance labor market is no longer part of the pattern of successful women's finance careers.

Figure 2 reorders the career sequences in figure 1 by age and divides them into the three college-graduation cohorts. Vertical lines demarcate the watershed second period, 1970–73. While over half of the first and the second graduation cohorts began in female-dominated or other nonfinance jobs (coded “fe” and “nf”), only two of the third graduation-cohort careers (cases 22 and 4, starred) began outside finance.

For most third-cohort graduation members, the specialization in finance actually began earlier, during college. Of third-cohort members, 75% majored in business, economics, accounting, or a related field. In contrast, only 40% of the first graduation cohort and 33% of the second graduation cohort had college majors in business-related specialties.

Of course, there could exist a group of much younger women or a group of late bloomers who are doing or did other jobs before launching finance careers and may yet reach senior levels. The next section examines the issue of right truncation and potential late bloomers. But preliminarily, it seems that in contrast to older women who might be teachers or social workers before pursuing successful finance careers, younger women may be on an unyielding track that demands specialization in finance immediately after college.²⁴

If the hypothesis that respondents' careers are growing increasingly rigid is to be supported, we should be able to see it in the optimal matching clusters. Consider again figure 1, which shows female finance executives' five distinct career types. Within each of the five clusters, the longer careers at the top are from the first graduation cohort. The middle-length careers that start at some point between the two vertical lines, between 1970 and 1973, are from the second graduation cohort. The short careers at the bottom of each cluster and that start after the second vertical line, after 1973, belong to the third graduation cohort.

If women's careers really are becoming more rigid, we would expect the clusters to be growing more homogeneous: to be coalescing over time. In other words, within each cluster, the distances between careers generated by the optimal matching algorithm should be smaller for the younger respondents.

²⁴ I do not suggest that all women's finance careers now look like men's. My sample analyzed here is restricted to women who have reached high levels. Among full-time managers, women are vastly underrepresented in senior management. And many female managers leave their careers or shift to part-time work in order to care for families, as my related research is investigating.

TABLE 5
INTERSEQUENCE DISTANCES WITHIN GRADUATION COHORTS, WITHIN CLUSTERS

CLUSTER	MEAN INTERSEQUENCE DISTANCES			
	Within Cluster	Among First Graduation-Cohort Careers	Among Second Graduation-Cohort Careers	Among Third Graduation-Cohort Careers
143 (19)	.52 (7)	.61 (4)	.29 (8)
241 (7)	.41 (2)	.45 (3)	.38 (2)
351 (9)	.57 (3)	NA (1)*	.49 (5)
463 (15)	.64 (8)	.66 (3)	.57 (4)

NOTE —Ns are given in parentheses
* The mean intersequence distance could not be calculated because cluster 3 has only one second-cohort career.

I explore whether careers are coalescing by using the intersequence distances calculated by the optimal matching algorithm to find the mean distance within each cluster (see fig. 1). Then I separately calculated the mean distances between each of the three graduation cohorts within each cluster. For example, in cluster 1, the corporate climbers in very large firms, I first calculated the mean distance of all the corporate climbers in very large firms from one another. Then I separately computed the mean intersequence distance among the seven long first-cohort careers at the top, the mean intersequence distance among the four second-cohort careers, and the same among the eight short third-cohort careers that are at the bottom of cluster 1.

To garner support for the rigidity hypothesis, we would need to find that the mean distance among the first graduation-cohort careers and that among the second graduation-cohort careers should be greater than the overall cluster mean, while the mean distance among the third graduation-cohort careers should be less than the cluster mean. These distances are all reported in table 5. The top row of table 5 presents the distances in our first cluster, the corporate climbers in very large firms. The mean intersequence distance among all 19 careers in cluster 1 is 0.43. As predicted by the rigidity hypothesis, the mean intersequence distance among the first graduation cohort (0.52) and that among the second graduation cohort (0.61) are both greater than the mean intersequence distance of the overall cluster (0.43). Again as predicted, the mean intersequence distance among the third graduation cohort is just 0.29, a smaller mean distance than that within cluster 1 overall.

Table 6 summarizes the relationship between the overall cluster mean intersequence distance and the intersequence distances within cohorts in

TABLE 6

SUMMARY OF TABLE 5: INTERSEQUENCE DISTANCES OF GRADUATION COHORTS, WITHIN CLUSTERS

CLUSTER	GRADUATION COHORT*		
	1	2	3
1	GT	GT	LT
2	EQ	GT	LT
3	GT	NA	LT
4	GT	GT	LT

* GT indicates that the graduation cohort's intersequence mean distance is greater than the cluster's mean intersequence distance. LT means that the cohort's intersequence mean distance is less than the cluster's mean intersequence distance. EQ indicates a graduation-cohort intersequence mean distance equal to the cluster's.

clusters. The rigidity hypothesis finds a great deal of support. Within clusters 1, 3, and 4, the older cohorts have mean intersequence mean distances greater than (GT) the overall cluster mean, while the third cohort's mean distance is less than (LT) the overall cluster mean. This pattern only varies in the case of cluster 2, in which the first graduation-cohort intersequence mean distances are actually equal to the overall cluster mean, while the second- and third-cohort distances show the predicted pattern.²⁵

These findings are exploratory. The numbers in the cells of table 5 are small, the sample is nonrandom, the differences between mean distances are not always large, and these careers are right truncated. However, the pattern is striking (and is statistically significant in the larger clusters 1 and 4). My respondents' careers fall into five distinct trajectories, which are shaped by occupational and organizational factors. Within each trajectory, the careers of mid-baby boom women finishing college after 1973 are more similar to one another than are the careers of their older female colleagues. Within the pathways dictated by organizational and economic structures, respondents' finance careers are advancing both more quickly and more narrowly over time.

²⁵ A difference of means *t*-test shows that the predicted patterns are statistically significant in the larger clusters 1 and 4. In cluster 1, graduation cohort 3 has a smaller mean intersequence distance than cohort 1 ($P \leq .001$) and than cohort 2 ($P \leq .001$). Similarly, in cluster 4, graduation cohort 3 has a smaller mean intersequence distance than cohort 1 ($P \leq .10$) and cohort 2 ($P \leq .10$). In table 5, as in the other tables, *N* indicates the number of respondents. But the means and statistical tests are based on the number of intersequence distances in each cell, which equals $(N - 1) + (N - 2) + (N - 3) \dots + (N - N)$. The number of intersequence distances ranges from 28 (graduation cohort 3 of cluster 1 and graduation cohort 1 of cluster 5) to 1 (graduation cohorts 1 and 3 of cluster 2).

THE RIGHT-TRUNCATION PROBLEM

The analyst of real careers of living, working people unavoidably confronts the problem of right truncation. Here the third cohort is the most severely truncated. Moreover, these data, by design, include only high-ranking female finance executives. There could exist some late bloomers the same age as the youngest cohort who have not yet reached a sufficiently high level in finance to be included in the sample. There is no real solution to this problem other than caution about interpretations. Future research should examine whether 10 years from now female finance executives of third graduation-cohort age continue to have more narrowly focused careers than their predecessors or whether their ranks have been diluted by women who began their careers as teachers and social workers and more recently attained finance executive status.

Nonetheless, I made two checks to bolster the argument that female finance careers are becoming more homogeneous over time. First, I quickly looked for evidence regarding the existence of a late-bloomer population. My respondents who attended business school generally went to elite institutions. Older women studying for MBA degrees at elite business schools would constitute a possible source of future late bloomers. To try to see if these women existed in large numbers, I read Web pages, ordered brochures, and interviewed admissions offices at 11 top business schools.²⁶ The current mean age of the MBA student body at enrollment was similar across the schools and averaged 27 years. Other than MIT, the admissions offices gave me no data on mean age by sex. MIT reported that the mean age of female students was 26.5, a year younger than that of male students. I asked the staff of offices that did not provide data on age by sex whether they had an impression whether one sex was, on average, older or had a different age distribution than the other sex. The staff said either that they believed the age distribution for men and women to be similar or that they had "no impression" about this.

Staff members explained that the MBA admissions process favors applicants with business experience. For example, 98% of the incoming University of Chicago MBA class of 1997 had work experience, and the mean years of experience was 4.5. Assuming the average female student's mean age is around 27, she would have finished college and then worked, proba-

²⁶ I investigated the business schools reported by the March 1997 *U.S. News and World Report* as the top 10: Stanford, Harvard, University of Pennsylvania, MIT, University of Chicago, Northwestern, Columbia, Dartmouth, Duke, and University of California, Berkeley. I also looked at New York University, the one business school listed in the top five in finance that was not also on the previous list (Lord 1997). My research excluded executive MBA programs, which are shorter courses of study designed for middle- and upper-level executives.

TABLE 7

INTERSEQUENCE DISTANCES OF GRADUATION COHORTS, WITHIN CLUSTERS, FIRST 19 YEARS OF ALL CAREERS

CLUSTER	MEAN INTERSEQUENCE DISTANCE			
	Within Cluster	Among First Graduation-Cohort Careers	Among Second Graduation-Cohort Careers	Among Third Graduation-Cohort Careers
146 (19)	.54 (7)	.58 (4)	.29 (8)
244 (7)	.52 (2)	.49 (3)	.34 (2)
353 (9)	.68 (3)	NA (1)*	.47 (5)
466 (15)	.57 (8)	.67 (3)	.55 (4)

NOTE —Ns are given in parentheses

* The mean intersequence distance could not be calculated because cluster 3 has only one second-cohort career

bly in business, for a few years before starting her MBA program. This pattern does not resemble my data's first and second graduation-cohort members, almost two-thirds of whom initially pursued careers and sometimes advanced degrees in female-dominated occupations before entering finance. This cursory glance at elite MBA programs does not suggest that they are housing large numbers of late bloomers.

Another check was to equalize the truncation across the three cohorts by artificially shortening each career to include only the first 19 years after college graduation.²⁷ I then reanalyzed the data. The findings were very similar to those concerning the whole careers. For the shortened careers, the mean distances among the first and second graduation cohorts within each cluster were again greater than the overall cluster mean (with one exception), while the mean distance among the third-cohort careers within each cluster was less than that cluster's overall mean. These distances are reported in table 7 and summarized in table 8.²⁸ The first half of female

²⁷ I picked 19 years as a convenient cutoff point for artificial truncation because 19 is the median length of the third graduation cohort's whole careers and half the length of the longest careers in the sample.

²⁸ The only exception to the predicted pattern is cluster 4, graduation cohort 1, which has a smaller intersequence distance than that for cluster 4 as a whole (tables 7 and 8). This is because all but one of the cluster 4, first-cohort respondents had a similar pattern of beginning their work life in female-dominated occupations (fig. 1). Other than that exception, difference of means *t*-tests showed that differences between graduation cohorts 1 and 2 and between cohorts 1 and 3 are statistically significant in each of the three larger clusters: cluster 1 ($P \leq .001$), cluster 3 ($P \leq .01$), and cluster 4 ($P \leq .01$).

TABLE 8

SUMMARY OF TABLE 7: INTERSEQUENCE DISTANCES OF GRADUATION COHORTS, WITHIN CLUSTERS, FIRST 19 YEARS OF ALL CAREERS

CLUSTER	GRADUATION COHORT*		
	1	2	3
1	GT	GT	LT
2	GT	GT	LT
3	GT	NA	LT
4	LT	GT	LT

* GT indicates that the graduation cohort's intersequence mean distance is greater than the cluster's mean intersequence distance. LT means that the cohort's intersequence mean distance is less than the cluster's mean intersequence distance.

finance-executive careers shows evidence of increasing rigidity by the third graduation cohort.

I also reanalyzed the slopes of the artificially truncated careers. (Recall that in contrast to the rigidity analysis, the slope analysis examines *finance* cohorts.) The first 19 years of respondents' careers show the same pattern as the slope data for the whole careers. Slopes of the truncated careers increased abruptly between finance cohorts 1 and 2 and remained fairly constant thereafter (see table 9).

The first half of female finance-executive careers has indeed transformed over the past 30 years. These patterns could conceivably change depending on the possibility of late bloomers. But among currently successful executives, the careers of women who entered finance after 1973 look quite different from the careers of women who entered finance before 1970. Those starting careers after 1973 enjoy quicker promotions and a faster overall career pace than women beginning finance careers in the 1960s. In the third graduation cohort, women's managerial careers resem-

TABLE 9

MEDIAN AND RANGE OF CAREER SLOPES IN EACH FINANCE COHORT, FIRST 19 YEARS OF ALL CAREERS

	1 (1960-69)	2 (1970-73)	3 (1974-80)
Median70	1.0	.93
Mean66	.94	.94
Range00-1.20	.00-1.45	.00-2.50
N in each finance cohort	10	11	35

NOTE.—Years in which cohorts entered finance labor market are given in parentheses

ble the stereotypical successful male managerial career (Mills 1951; Powell 1993). They progress quickly from business-related college majors to business school or entry-level management jobs up to significant executive responsibility. Older respondents had the option of working in other fields before pursuing successful finance careers and faced higher barriers to entering finance. In contrast, women leaving college after 1973 seem to be on a more rigid, unyielding track that demands specialization in finance-related fields during or immediately after college.

CONCLUSION

This article examines one end of the distribution of employed women: senior executives in finance-related fields. This is a useful case for several reasons. It outlines unusually successful career pathways over social structural barriers to advancement. These women with ample resources highlight the effects of social and legal change at their strongest. They were well able to exploit legal changes that were designed to help all employed women.

The article demonstrates the importance of studying intact careers in historical context with methods sensitive to both objective structures and subjective interpretations. Despite female finance executives' rarity and isolation, their careers fall into one of four career types: the corporate climbers, big fish in small- and medium-sized organizations, movers and shakers, and entrepreneurs. These four trajectories are shaped by a constellation of causes: ILMs, geographic mobility, the presence or absence of work histories before finance, and turbulence. A historical event cut across all these factors and shaped their impact on women's careers. The EEOC enforcement of women's employment rights in the early 1970s increased respondents' rates of advancement and altered their expectations about the barriers they would encounter.

The sharp discontinuity in the career pace and patterning of cohorts closely following one another reminds us that change across cohorts is not always gradual. Although social practices usually reproduce social structures with small revisions, historical events can abruptly interrupt social regularities and rearrange structures (Sewell 1996). First and second cohorts participated in this historical event by pushing against structural barriers with lawsuits and threats to quit and by aggressively taking advantage of new opportunities. The altered, less-discriminatory structures in turn shaped the third cohort's perspective. Ironically, many younger third-cohort members were unaware that their mobility was in part due to the creative action of their female predecessors. Swimming with the current, these younger respondents took personal credit for their own rapid progress (see Emirbayer and Mische 1998).

There is some evidence to suggest a paradox: as women experience more freedom in pursuing and succeeding in finance careers, their career pathways have become more rigid. It is extremely rare for currently senior women to have worked part time or to have spent time outside the labor force. Although some mothers of young children say they wish they could work part time or take a leave of absence, they believe their career advancement would never recover if they demonstrated less than total commitment to their careers. Moreover, the youngest women may be on a track that demands specialization in business school or finance-related work immediately after college. Executive women's careers may be growing more coercive as they more closely resemble the template of executive men's careers.

The liberal strand of the feminist movement, with its emphasis on equal opportunity for individuals, has opened up elite finance occupations to some women. Yet the very success of liberal feminism has weakened the movement. Many younger beneficiaries give their own effort full credit for their advancement and deem the women's movement irrelevant.

Even among female finance executives, liberal goals are not fully accomplished. Executive women enjoy prestigious, highly paid work, but they compare themselves not to the average worker but to their predominately male colleagues. As old barriers crumble, many respondents encountered new barriers that they interpret as new forms of sexual discrimination.

Feminists whose concerns extend beyond liberal goals toward a more radical transformation of society will note that the organization of finance work has been virtually unaffected by feminism. Female finance managers have neatly conformed to the organizational demands and rigid timetables of elite careers. Executive women in finance-related fields have transformed themselves rather than the organizations to which they devote most of their waking hours.

APPENDIX

A Simplified Example of Optimal Matching Techniques

Optimal matching techniques help us find typical patterns in sequences of career events. They provide a measure of resemblance between career pairs. These resemblances (or their inverse, distances) can then be scaled or clustered to show patterns and to reveal any common trajectories (see Sankoff and Kruskal 1983; Abbott and Hrycak 1990; Abbott 1995).

Figure A1 examines three hypothetical career paths of job levels only (see table 1 for job codes). This example ignores organization sizes. The sequences begin at college graduation. Each code represents one year in time.

A	ps	ps	4	4	5	5	5	5	6	6	6	6	6	7	7	7
B	ps	ps	4	4	5	5	5	5	5	5	5	5	5	6	6	en
C	fe	fe	fe	fe	fe	4	4	4	4	4	5	5	5	5	5	5

FIG. A1

By inspection, we see that career A looks more similar to B than it does to C. A and B each begin with professional school followed by two years at job level 4, and so on. C is longer and begins with 5 years in a female-dominated occupation. C then spends six years at level 4, and so on.

The standard general algorithm for alignment is the Needleman-Wunsch algorithm. It measures the distance between A and B by determining how difficult it is to turn A into B, or vice versa. It does so by calculating "substitution" costs, the cost of turning a mismatched event of B into A's event. The algorithm also uses "insertion" (or "deletion") costs. These measure the cost of inserting blank years into one sequence (or deleting years from the other sequence).

In figure A2, let "I" indicate a perfect, costless match. Let "T" stand for insertion costs and "S" for substitution costs.

A.	ps	ps	4	4	5	5	5	5								
B	ps	ps	4	4	5	5	5	5	5	5	5	5	5	6	6	en

FIG. A2

The first eight elements of A and B are costless matches. The next eight events are mismatched and require either substitutions or insertions. To transform A into B, the metric moved part of A's string over to the right to make the two "6s" in B match up with A.

This alignment uses four substitutions and four insertions. The analyst can vary the actual cost of each substitution depending upon the elements being replaced, but we will ignore that here. (Table A1 lists the varied substitution costs used in this study.) The analyst also decides the ratio of substitution to insertion costs based on theoretical and practical criteria. For example, if an insertion cost is set to one-half a substitution cost, then the alignment would cost $4 \text{ sub} + (4 \text{ ins} \times .5) = 6$. In other words, the distance between A and B is 6 units.

Now we measure the distance between careers A and C. The alignment of A and C in figure A3 takes six substitutions and 10 insertions. The cost then is $6 \text{ subs} + (10 \text{ ins} \times .5) = 11$.

A		S	S	I	I	I	I	I	I							
	ps	ps								4	4	5	5	5	5	6
C.	fe	fe	fe	fe	fe	4	4	4	4	4	4	5	5	5	5	6

FIG. A3

TABLE A1
SUBSTITUTION COST MATRIX FOR OPTIMAL MATCHING ANALYSIS*

	nf	fe	ps	4S	4M	4L	4V	5S	5M	5L	5V	6S	6M	6L	6V	7S	7M	7L	7V	8S	8M	8L	8V	en
nf	.																							
fe	71	.																						
ps		84	85																					
4S			87	87	78																			
4M																								
4L				71	76																			
4V				85	87	74	78																	
5S																								
5M																								
5L																								
5V																								
6S																								
6M																								
6L																								
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7S																								
7M																								
7L																								
7V																								
8S																								
8M																								
8L																								
8V																								
en																								

* This matrix lists the cost for substituting a row for a column element in the alignment of two sequences. These substitution costs, plus the .48 cost of each insertion or deletion of an element in one sequence are added to determine the distance between two sequences. The diagonal substitution costs, which indicate the substitution of one element with an identical element, are zero. Job-level and organization-size codes are presented in table 1

A and B are only six units apart, while A and C are eleven units apart. The distances between sequences can be scaled or clustered. See figure 1 for a cluster analysis of career sequences from my data.

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The Categorical Imperative: Securities Analysts and the Illegitimacy Discount¹

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This article explores the social processes that produce penalties for illegitimate role performance. It is proposed that such penalties are illuminated in markets that are significantly mediated by product critics. In particular, it is argued that failure to gain reviews by the critics who specialize in a product's intended category reflects confusion over the product's identity and that such illegitimacy should depress demand. The validity of this assertion is tested among public American firms in the stock market over the years 1985–94. It is shown that the stock price of an American firm was discounted to the extent that the firm was not covered by the securities analysts who specialized in its industries. This analysis holds implications for the study of role conformity in both market and nonmarket settings and adds sociological insight to the recent "behavioral" critique of the prevailing "efficient-market" perspective on capital markets.

Consider how an impostor is exposed. (White 1970, p. 5)

INTRODUCTION

A circular dynamic governs much of social life. Actors interpret one another's actions by comparing them with accepted role performances. Similarly, social objects are evaluated via legitimate categories. To confer meaning and give order, such systems of classification must have integrity. Thus, new roles and types emerge with difficulty as actors face pressure to

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demonstrate that they and the objects they produce conform to recognized types. In general, actors accede to this categorical imperative. Existing structures are thereby reproduced.

These ideas are familiar from observations of premodern cultures (e.g., Durkheim 1915; Douglas 1966; Berger and Luckman 1966) and are responsible for the image of stasis that we frequently ascribe to such societies. It is thus interesting to note that the idea that actors are constrained by accepted models represents an important but underrecognized thread that runs through much thinking on modern organizations and markets.

This insight is clearly at the heart of the neoinstitutional perspective on organizations (Meyer and Rowan 1977; DiMaggio and Powell 1983). The answer to the question of "Why there is such . . . homogeneity to organizational forms and practices?" (DiMaggio and Powell 1983, p. 148) is that organizations that do not meet institutionalized expectations for how they should look and act are viewed as illegitimate. The threat of being denied legitimate standing in turn induces organizations to adopt accepted procedures. Organizational variety decreases accordingly.

While generally not described in such language, this process forms a central feature of market behavior as well. In particular, White's image of production markets as self-reproducing role structures (White 1981a, 1981b, 1988; Leifer 1985; Leifer and White 1987) hinges on producers' continuing conformity with recognized "schedules" of cost-quality niches. Attempts to deviate from a niche invite sharp discipline as they threaten the acts of cross-product comparison that sustain the market. The quite different intellectual tradition of marketing theory focuses on similar issues using the framework of product categories. This literature suggests that a seller must offer products that conform to accepted types lest such offerings be screened out of consideration as incomparable to others (e.g., Shocker et al. 1991; Urban, Weinberg, and Hauser 1996). Thus, in interorganizational relations, and in markets more generally, unclassifiable actors and objects suffer social penalties because they threaten reigning interpretive frameworks.

While the constraining impact of accepted role structures on individual behavior seems quite evident, two important deficiencies in previous research limit our understanding of the processes involved. First, rather than demonstrate that defying classification invites penalties, scholars tend to point to the homogeneity of practice and take this as evidence that defection is punished. Researchers have described processes of conformity with legitimate models in a wide variety of market (e.g., Leifer and White 1987; Davis 1991; Burt 1992, chap. 6; Haunschild 1993, 1994; Han 1994; Greve 1995, 1996) and nonmarket (e.g., Tolbert and Zucker 1983; Galaskiewicz and Burt 1991; Edelman 1992; Dobbin et al. 1993; Sutton

et al. 1994) settings. However, evidence of negative consequences caused by illegitimacy is scant.²

A related weakness in existing theory concerns an inattention to the audience responsible for conferring legitimacy on actors and objects. The expectations of critical interactants and observers discipline actors to play accepted roles. However, such expectations are typically thought to be either inaccessible or irrelevant. For example, neoinstitutionalists study the adoption of legitimate models but rarely examine the actors who hold such models. Similarly, consumer expectations in mass markets are generally thought to be unobservable. Thus, White stresses that the central dynamic in production markets consists of mutual monitoring among sellers rather than reaction to an amorphous mass of buyers (e.g., White 1981*b*). Feedback from the audience is indeed indirect in many contexts; however, as audience expectations are at least partially responsible for illegitimacy costs, they must be included in our understanding of how such penalties emerge.

The present analysis addresses these weaknesses by examining a "mediated market," one in which third parties act as critics as they shape market patterns through product recommendations and endorsements. In industries where they exert significant influence, such critics, who may not even take part in the flow of exchange, replace consumers as the primary audience that determines the fate of products (Hirsch 1972, 1975). Indeed, relative to markets dominated by anonymous and fleeting transactions with consumers, durable and concrete relations with critics increase seller sensitivity to audience response. Further, the relative visibility and stability of seller-critic relations make mediated markets particularly useful settings for studying the social processes that underlie illegitimacy costs.

In particular, two observations about such markets motivate the present analysis. First, encoded in a critical review is an acknowledgment on the part of the reviewer that the product is legitimate. Second, critics often specialize by product category. In the context of such a division of labor, a critical review confers a very specific kind of legitimacy. It signals membership in an accepted product category. Thus, in the aggregate, a product's position in the network of reviews linking critics to the products they critique indicates its degree of legitimacy. For a product that is pro-

² An exception is Hannan and Carroll's (1992) theory of density dependence. However, measurement of legitimacy in such studies is indirect and aggregated such that penalties suffered by individual firms are unobservable (Zucker 1989). Other notable studies have shown harmful effects of nonconformity (e.g., Miller and Chen 1996) and failure to gain accreditation by government agencies (Singh, Tucker, and House 1986; Baum and Oliver 1991, 1992). However, such research cannot distinguish between the impact of the legitimacy and the efficiency of the organizations in question.

moted within a particular category, the degree to which it fails in attracting reviews from relevant critics indicates its susceptibility to illegitimacy costs. When the pattern of reviews suggests a mismatch between a seller's view of its offering and the identity attributed to it by critics, demand for the product should be weak. All other factors being equal, such a product should command a lower price.

This article presents a general approach to understanding illegitimacy costs and proposes that mediated markets afford a useful context in which to study such penalties. In addition, the present research aims to extend sociological theories of economic markets by testing for illegitimacy costs in the stock market. The stock market displays the described features of mediated markets in the sense that firms correspond to products, industries to product categories, and securities analysts to product critics. Based on the theoretical framework sketched above, I claim that failure to attract coverage from the analysts who specialize in a firm's industries causes the firm's equity to trade at a discount. This argument runs counter to the predominant scholarly approach to financial markets, which rejects the possibility of such discrepancies between price and value. By contrast, I contend that the considerable uncertainty inherent in valuation, which is compounded by the social nature of investing, gives special urgency to the need for legitimacy. Thus, the present analysis constitutes a joint test of two claims: that illegitimacy is costly and that financial markets are sensitive to the pressures for legitimate role performance characteristic of other market and nonmarket settings.

THE CANDIDATE-AUDIENCE INTERFACE

Consider a very simple social situation: an interface between two classes of actors (cf. White 1981*b*). The first set of actors, whom I term "candidates," seeks entry into relations with members of the second class, whom I call "the audience." Candidates present the audience with different "offers" in an attempt to win their favor. There is a fundamental asymmetry in the interface. Candidates seek relations with audience members, and the latter select those to whom they will grant these privileges.

Figure 1 explicates the two-stage process by which candidates compete to form relations with the audience members. The latter seek to assess the relative worth of the offers presented by the former. However, evaluation requires calibration of offers against one another. Offers that do not exhibit certain common characteristics may not be readily compared to others and are thus difficult to evaluate. Such offers stand outside the field of comparison and are ignored as so many oranges in a competition among apples. It is this inattention that constitutes the cost of illegitimacy. Fur-

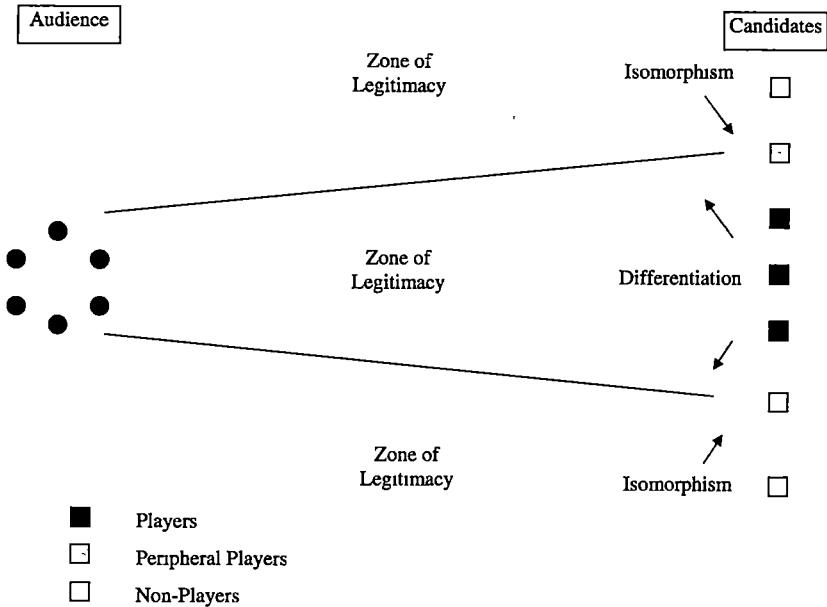


FIG. 1.—The candidate-audience interface

ther, the prospect of such illegitimacy leads candidates to demonstrate their comparability with standard offerings. The aggregate result is that all players share certain basic characteristics.

The second stage of competition is more familiar. Audiences observe the offers made by legitimate players and choose the one that seems most attractive. Players, in turn, vie with one another to promote their offers to audiences. Each player tries to differentiate its offer from those advanced by its peers and establish its relative desirability. Thus, differentiation works hand in hand with isomorphism (cf. Porac and Thomas 1990; Baum and Haveman 1997). Gaining the favor of an audience requires conformity with the audience's minimal criteria for what offers should look like and differentiation from all other legitimate offers.

Note that two assumptions underlie this discussion. First, candidates depend on positive response from the audience. To the extent that candidates are insensitive to such reaction, illegitimacy is irrelevant and there should be no tendency toward conformity. Indeed, when the tables are turned, that is, when audience members depend on a candidate, it may in fact be advantageous to present an incoherent, rather than a coherent identity (Padgett and Ansell 1993). Second, while conforming to audience expectations is generally wise, the greatest returns likely flow to those

who innovate by creating new categories and corresponding interfaces (Schumpeter [1934] 1983). Nevertheless, the vast majority of innovations fail in part because of the difficulties they face in achieving legitimacy (Stinchcombe 1965; Hannan and Carroll 1992).

LINKS TO EXISTING THEORY

The proposed framework bridges seemingly disparate bodies of theory. In particular, the first stage of competition reflects the need for legitimacy described by neoinstitutionalists (Meyer and Rowan 1977; DiMaggio and Powell 1983) and the sharp discipline faced by niche defectors in White's market model (see esp. White 1981*b*, 524 n. 4). In both theories, conformity at the microlevel ensures coherence at the macrolevel. Further, the present perspective highlights two aspects of these theories that are generally left implicit: the presence of an audience confronting focal actors and the competition among such actors for the favor of this audience. Without an audience, legitimacy loses its value and, indeed, its meaning. Similarly, when the audience does not select among alternative candidates, this eliminates competitive tensions among focal actors, and any pressure for conformity dissolves.

The proposed perspective suggests connections between sociological models of organizations and markets and the models of consumer decision making and market structure that prevail in the marketing literature. Drawing on cognitive psychological models of decision making, marketing theorists see consumers as selecting products in two phases (see Shocker et al. 1991; Urban et al. 1996 for review). First, they eliminate all options that do not meet minimal criteria of acceptability (cf. Payne 1976). Next, consumers compare among members of their "consideration sets" and select a final choice. The implications for sellers are clear. "First, the (product) must be positioned so that consumers do not eliminate it through outright categorization. Second, it must have the attributes that lead to its being . . . preferred, given that it is not eliminated" in the first stage (Urban et al. 1996, p. 57). That is, sellers must engage in isomorphism so as to gain membership in a recognized product category and differentiation from other members in that category.

Note that consideration sets impose significant constraints on sellers because, rather than originating in individual tastes, they are generally extracted from publicly discussed product categories (Urban et al. 1993; Bronnenberg and Vanhonacker 1996). Indeed, while the proposed framework bears surface resemblance to economic models that portray actors as laboring to reduce the cost of gathering information (e.g., Stigler 1961; Williamson 1975; Raff and Temin 1991; Aghion and Tirole 1995), it differs from such models in two critical respects (but see Zwiebel 1995). First,

deviation from idealized rational choice is not simply a matter of compensating for a lack of full information but the enactment of two distinct stages of choice—categorization and comparison. Consumers first screen out illegitimate options, and only then do they perform something akin to rational choice among legitimate alternatives. Second, the screen is a *social screen*, not designed by the actor but external to her, given in the categories that comprise market structure. Products that deviate from accepted categories are penalized not simply because they raise information costs for consumers but because the social boundaries that divide product classes limit the consideration of such offerings.

Thus, the proposed framework understands illegitimacy costs and tendencies toward role conformity as twin aspects of a general situation of social confusion. Audience members employ categories to interpret the offers set before them. The threat of illegitimacy consists in the possibility that a candidate will not be readily classified and will therefore be ignored as unintelligible. Conformity ensues. Indeed, the categorical imperative is operative wherever there are meaningful categories. Whether candidate-audience interaction consists of suitors vying for potential mates, parties appealing to voters, or firms seeking investors, offerings that do not fit existing categories are pressured to conform.

THE REVIEW NETWORK

A focus on mediated markets helps illuminate such pressures. As described above, a weakness of previous research on organizational and product isomorphism lies in its failure to account for the audience whose expectations drive such conformity. Whereas interested observers are thought to hold certain models of form and practice and to discipline those who do not adhere to them, these audiences are missing from analysis and are largely ignored in theory. In particular, consumers are generally considered an unobservable mass whose role in product classification is effectively irrelevant (e.g., Porac and Thomas 1990; Reger and Huff 1993; Lant and Baum 1995). Indeed, some scholars make the irrelevance of consumers a central feature of analysis. "Rather than dream about buyers," writes White, "firms watch their competitors" (White 1988, p. 238). Similarly, Burt argues that social contagion consists primarily of mutual monitoring among structurally equivalent rivals rather than exposure to similar sets of third parties (Burt 1987). In sum, existing theory supposes that audience influence on isomorphism is either negligible or unobservable.

However, such a perspective is less tenable where buyer-seller transactions are significantly mediated by a visible and enduring public of critics who act as the primary audience for product offerings. In such markets, the vast array of consumer reactions to a product becomes concentrated

in a relatively small set of reviews, whose great influence commands seller attention. Rather than projecting broad appeals to consumers, sellers in such markets promote their wares directly to critics and employ a wide variety of tactics to co-opt their power (Hirsch 1975; cf. Porter 1976, chap. 2; Shrum 1996).

Indeed, the behavior of critics is visible not only to sellers but to scholars as well. Critical reviews form a two-mode network (Wasserman and Faust 1994, pp. 291–343) in which critics send reviews to product offerings. This network structure has important implications where critics specialize by product type. In the aggregate, analysis of the network may reveal the market's product categories and the degree of proximity among them. Further, the egocentric network of reviews to any individual product registers its degree of legitimacy as a member of a category. That is, the decision by a critic to review a product implies a judgment regarding how that offering should be classified. The egocentric network of reviews that a product attracts—and does not attract—indicates the general perception of its market identity.

The analysis of such egocentric networks provides a basis for assessing the harm suffered by illegitimate offerings. When promoting a product, sellers generally appeal to a given product category. A product that does not gain legitimacy via reviews by the critics who specialize in its intended category should face greater difficulty generating demand. As a general proposition, I expect that:

PROPOSITION.—*Ceteris paribus, a product experiences weaker demand to the extent that it does not attract reviews from the critics who specialize in the category in which it is marketed.* The crux of the challenge for sellers lies in a tension between self-concept and social identity. As with any social role, occupancy depends less on the actor's beliefs about his own identity than on a relevant audience's attributions (Baker and Faulkner 1991). Sellers may become players only when recognized as such by critics. Thus, sellers must gain acceptance for their view of their product's identity. Failure to gain recognition as a player lowers a product's chance of success.

THEORETICAL SCOPE

It is important to recognize that the stated proposition should be operative only in markets with two basic characteristics. First, the market must possess certain structural features: a recognized set of product categories and an influential class of critics who specialize by category. While certain markets—for example, pharmaceuticals or academic publishing—may display these characteristics, others—for example, the paper clip industry—may not.

Second, the present perspective applies only to markets where consumers face significant difficulty evaluating products. Indeed, this feature relates to the first in that valuation problems are a necessary—though insufficient—condition for the emergence of the described structures.³ If the worth of a product is clear, questions of classification, the behavior of critics (cf. Hirsch 1972; Biglaiser 1993), and the nature of marketing activity should all be irrelevant (cf. Nelson 1974). Such ambiguity is greatest in the case of “social” goods, those for which the value to a given consumer depends heavily on their worth to other consumers. Products that are consumed largely for their role as social markers are the most familiar examples (Veblen [1899] 1953; Douglas and Isherwood [1979] 1996). Other markets for social goods include real estate or education, where the benefit derived depends on others’ consumption of similar resources (cf. Hirsch 1976), or technology, where the need for compatibility generates “network externalities” (e.g., Farrell and Saloner 1985). Critics play a crucial role in such markets by providing guides to current and future tastes. Indeed, the preliminary stage of critical evaluation involves reaching a consensus on proper product classification (DiMaggio 1982; Smith 1989, pp. 28–31).

Thus, the stated proposition should be valid only where audiences face significant valuation challenges. Indeed, the structural features relevant to the present perspective—a well-defined product category system and an influential set of critics who specialize by product category—should not emerge where value is clear. Therefore, an analysis of the proposition posed above—that products suffer when they are not certified by critics as members of their intended product categories—serves both to test its general validity and to indicate the nature of the market under study. If products are indeed penalized in the described fashion, then the market must be one in which value is ambiguous.

APPLICATION: THE STOCK MARKET

In light of these scope conditions, the contemporary American corporate equity or stock market recommends itself as a compelling context in which to apply the present perspective. First, this market possesses the structural characteristics relevant to the stated hypothesis. In particular, industries are the product categories by which corporate equity shares are classified and securities analysts, who divide their labor by industry, are the relevant product critics. Second, the applicability of the proposed theory to a financial market has important implications for our understanding of how

³ Other important conditions are that purchases be relatively large and infrequent and that the market be of significant scale.

such markets work. Evidence that analyst certification of a firm's membership in an industry influences its value would imply that value is unclear in financial markets, a claim that challenges prevailing academic wisdom regarding the nature of financial valuation.

Structural Features

The product categories of the stock market are quite clear. While there are many ways to classify equities (e.g., large vs. small capitalization; domestic vs. foreign), the principal categorization groups firms by industry. This classificatory scheme runs through public discussion of the stock market and is evident in newspaper tables, academic research (King 1963; Boudoukh, Richardson, and Whitelaw 1994; Firth 1996), and corporate self-presentations (e.g., Pfizer 1994, n. 20; McDonnell Douglas 1994, n. 14). Indeed, the principal standards of value, ratios of price to performance variables, are generally treated as industry-specific measures (e.g., Waxler 1997; King 1963; see below).

Further, as represented by the "sell-side" securities analysts who work for investment houses and research firms, corporate securities markets possess a well-established field of product critics.⁴ Certain analysts follow general trends in financial markets and the economy. Most analysts, however, track the performance of specific sets of firms and produce two principal products: forecasts of these firms' future earnings and advice that clients buy, sell, or hold their shares in the stocks of these firms (Kleinfeld 1985; Balog 1991). Indeed, while securities analysts are by no means the only sources of influence on share prices, such public and semipublic pronouncements on the value of firms distinguish analysts as critics of corporate equity in a manner akin to critics in other industries.

The analyst's unique status as market critic may be seen in three principal ways. First, along with large institutional investors, analysts represent the principal target for investor relations campaigns, whereby firms attempt actively to shape investor opinion (Useem 1993, 1996). Thus, Rao and Sivakumar (1999) demonstrate that increased attention from securities analysts brings about an intensification of such marketing activity. Further evidence that analysts represent the front line for investor relations efforts may be seen in the frequent visits by managers to analyst associations (Francis, Philbrick, and Hanna 1996) and in the conference calls and meetings with analysts in which significant corporate announcements are generally made. In such settings, the generally diffuse and anon-

⁴ Such analysts are also known as stock analysts, equity analysts, or equity researchers. See Burk (1988, chap. 2) and Zuckerman (1997, chap. 4) for discussions of the history of this quasi profession.

ymous relationship between a corporation and its investors becomes a more concrete and enduring affair (Useem 1996, pp. 72–77). Such sessions reflect the belief among managers—often borne out (Francis et al. 1996)—that actively promoting a firm to analysts is critical in ensuring that the market interprets corporate actions in a favorable manner.

Second, analysts serve as “surrogate investors” (cf. Hirsch 1972) in that their recommendations and forecasts significantly affect investor appetite for a firm’s shares.⁵ Indeed, while analysts often disagree amongst themselves on a firm’s prospects (Kandel and Pearson 1995), certain currents of opinion, especially when voiced by prominent analysts, significantly influence prices (e.g., Stickel 1985, 1992; Womack 1996). Analysts’ earnings forecasts are perhaps even more consequential than their stock picks. The primary question posed when a firm releases its quarterly earnings is whether it met analyst projections (see e.g., Marcial 1995; Blanton 1996). Consequently, a major focus of investor relations activity is the management of analyst expectations, so that the firm does not suffer the consequences of “negative earnings surprises” (e.g., Ip 1997; Lowenstein 1997).

Finally, securities analysts’ industry-based division of labor distinguishes analysts from other influential actors in financial markets. Just as physicians serve as gatekeepers for the drugs designed to treat their specialties, analysts tend to specialize by industry (see e.g., *Nelson’s Directory of Investment Research* 1998). Thus, in addition to indicating position in the economy, industry boundaries reflect divisions among stock market product categories as well as the professional specialties of securities analysts. Divisions among industry specialties are reinforced by public rankings, which evaluate analysts within industries. (See, e.g., “All-Star Analysts 1997 Survey,” *Wall Street Journal*, June 19, 1997, sec. R, p. 1, col. 1; and “25th All-American Research Team,” *Institutional Investor*, November 1996, p. 121.) Analysts compete intensely for position in such rankings both for intraprofessional status and for the increases in compensation granted to analysts of high rank (Eccles and Crane 1988, pp. 152–53). In sum, analysts act as stock market critics, and the analysts who specialize in a particular industry represent the principal critics for the stock issued by firms in that industry.

⁵ Like most critics, analysts are often charged with softening their views because of their dependence on corporate managers for access to information and their desire not to damage the house’s investment banking relationships (Hayward and Boeker 1998). Nevertheless, in their bid to satisfy their “buy-side” clients, analysts engage in an array of rhetorical subtleties to indicate more or less positive views on a firm and, in some instances, voice explicitly negative opinions (e.g., Lyons 1969).

Valuation in Financial Markets

As argued above, the structural characteristics associated with the corporate equity market should prevail only where investors face significant valuation difficulties. In particular, such structures are especially likely to emerge in markets for social goods—that is, where desire for a product depends on others' demand for the same good.

Note, however, that the dominant academic perspective on capital markets, “efficient-markets” theory, views the value of financial assets to be quite certain.⁶ According to the most prominent form of this position, a financial asset's price incorporates all past and future public information and is therefore the best estimate of its value.⁷ In particular, a price represents the future stream of dividends that will flow from a share of stock adjusted by discounts for time (dividends received far into the future are worth less) and risk (high-risk investments must yield higher return since investors must be compensated for taking on that risk).⁸ Future events are thought to generate current prices through the practice of what may be called “value arbitrage,” profiting by exploiting the discrepancy between an asset's price and its true value. Efficient-market theorists conjecture that, while many investors are poorly informed or ill-equipped to interpret the information at their disposal, the great rewards available to those who collect and correctly interpret present clues to future events guarantee that certain investors will undertake such efforts and succeed at them. These investors will value assets correctly and earn a profit by buying them when they are undervalued and selling them when they are overvalued. Moreover, the success of such “smart-money” investors should attract imitators, and the process should continue until the gap

⁶ Following Knight (1921), uncertainty should here be distinguished from risk—the assignment of known probabilities to outcomes. Note as well the interplay between uncertainty and ambiguity in the current context. March (1994, pp. 178–79) defines ambiguity as confusion over classification while uncertainty refers to the opacity of future events. The two issues become intertwined in the present context where classification is the first step in anticipating financial returns.

⁷ This represents the “semi-strong” version of the efficient-markets hypothesis (see Fama 1976). The “weak” version states that prices incorporate only past public information, and the “strong” version states that prices include all public information as well as all private information.

⁸ Note that dividends are considered to be theoretical—those earnings that a firm *could* disperse—rather than actual dividends. Indeed, given the taxation on dividends, investors are thought to prefer to receive yield in the form of capital gains—that is, the firm should use profits to buy back its own shares, lowering their supply and thereby raising their price (Miller and Modigliani 1961). The fact that investors have historically placed higher value on shares with greater dividend yield (Baskin and Miranti 1997) and continue to do so in contemporary markets (Shefrin and Statman [1984] 1993) challenges this aspect of efficient-markets theory.

between price and value is eliminated. Indeed, the constant search for such price-value discrepancies ensures that such gaps are competed out of existence (Friedman 1953; Modigliani and Miller 1958; Fama 1965*a*, 1965*b*; Samuelson 1965).

Thus, efficient-markets theory portrays financial products as unaffected by the uncertainty characteristic of the goods to which the present argument should apply. Indeed, this perspective predicts the disappearance of securities analysts (e.g., Fama 1965*b*) and the failure of managerial attempts to influence the value of their firms (e.g., Modigliani and Miller 1958; Miller and Modigliani 1961).⁹ That is, the stock market should not display the structures typical of markets in which value is unclear. Indeed, an implication of this perspective is that the current thesis is inapplicable to financial markets. As price is the best estimate of value in such markets, success in gaining acceptance as a member of a financial market category should have no impact on its price.

However, recent advances in finance theory suggest a more limited sense of efficiency. Two developments are particularly relevant to present concerns. First, a stream of research known as "behavioral finance" has documented the existence of "anomalies" in which stock prices have been shown to follow predictable patterns that should theoretically be arbitrated away (see Thaler [1993] for review). In particular, several researchers have shown that prices reflect certain cognitive biases such as short-term underreaction and long-term overreaction to information (see e.g., DeBondt and Thaler 1990; Jegadeesh and Titman 1993). Second, important limitations to arbitrage are now widely accepted. Empirically, such limits have been suggested by the failure of risk-based explanations of the excess returns associated with certain firm characteristics (see e.g., Fama and French 1992, 1996). Theoretically, several scholars have pointed out that, even if they are able to discern the correct price, arbitrageurs are often unable to bear the uncertainty inherent in deviating from prevailing investor opinion and not knowing how soon it will be "corrected" (De

⁹ Jensen and Meckling (1976, pp. 354–55) explain the anomalous existence of securities analysts by arguing that their direct monitoring of managers helps reduce the agency costs inherent in public corporations. While this explanation is not inconsistent with the present perspective, it ignores the analysts' provision of investment advice, which, though seemingly adding nothing to public information, has been shown to predict stock prices (e.g., Stickel 1985, 1992; Womack 1996). Furthermore, note that certain analysts perform no monitoring function whatsoever. In particular, the continued presence of "chartists" and technical analysts, who predict future prices from past price movements and market conditions, stands as a glaring challenge even to the "weak" version of efficient-markets theory, which contends that prices incorporate all information about past price movements and that price trends are thus a "random walk" (Bachelier [1900] 1964; Cowles 1933; Working 1934; Kendall 1953; Osborne 1959; Roberts 1959).

Long et al. [1990] 1993; Shleifer and Vishny 1997; cf. Keynes [1936] 1960, p. 157). Together, this work suggests that markets are largely efficient but that such efficiency is limited by the existence of certain cognitive conditions coupled with the inability of arbitrageurs to close the gaps between price and value that such conditions produce.

The limitations on market efficiency run deeper still. In particular, note that underlying the hypothesis of efficient markets is a selection process whereby correct methods of valuation outperform and thereby drive out incorrect ones. This assumes a closed system whereby investors repeatedly encounter the same or highly similar problems of valuation and can thereby adjudicate among competing techniques by observing their relative performance (cf. Winter 1986; Spotton 1997). However, this assumption appears suspect when we consider the fact that the economy continuously undergoes a vast amount of change along a wide variety of dimensions—for example, the introduction of new products, the proliferation of new competitive strategies, or the heightening of foreign competition. That is, financial valuation takes place not in a closed system but in one that sustains repeated exogenous shocks that resist easy interpretation. Investors must repeatedly manage the uncertainty generated by events that defy the categories of existing models (cf. Baker 1984; Podolny 1993, 1994; Haunschild 1994). Indeed, the very question of whether a new era or “paradigm” has been reached is a perennial issue in the financial community (e.g., Fisher 1996, p. 196; Henry 1997), a question about which every investor must theorize.

Further, such uncertainty is exacerbated by the fact that, like other social products, the value of an asset to a given investor depends to a great extent on how others view that asset. In particular, to the extent that an investor is sensitive to capital gains and losses rather than yield, she bases her purchases and sales at least implicitly on her belief that other investors will follow suit (Keynes 1960). As a result, financial market participants must closely monitor changes in prevailing theories of valuation regardless of their own views (Shiller [1984] 1993, 1990). Indeed, as professional investors are typically evaluated by their short-term relative performance, they may be especially responsive to one another’s beliefs (Keynes 1960; Scharfstein and Stein 1990; Friedman [1984] 1993).

These considerations do not suggest that markets are inefficient in the sense that available information is not incorporated in prices. Rather, the upshot of recent advances in finance research and the sociological approach advanced here is that access to information is insufficient for price to equal its theoretical value. Regardless of its availability, information must be decoded. However, the various cognitive limits on information processing as well as the inherent unpredictability of the economic future hinder interpretative projects. That such interpretation is a social enter-

prise, carried out with an eye to how others will come to view the same information, complicates matters further. Finally, that arbitrageurs face these and additional limitations suggests that their participation does not ensure market efficiency in the classical sense.

Indeed, as argued above, it is telling that the stock market possesses the structural trappings of markets in which value is unclear. In particular, a successful application of the proposition posed above to the corporate equities market would validate the view that financial-market efficiency is limited by the uncertainty—and, in particular, the social uncertainty—that inheres in efforts at valuation.

Application of the Proposition

Thus, the application of the stated proposition to the stock market is straightforward. In particular, a firm's position in the network of analyst coverage establishes its market identity. When this identity fails to match the firm's self-definition, the firm's stock performance should be impaired. In particular, define "coverage mismatch" as obtaining to the extent that a firm that does business in industry i is not covered by the analysts who specialize in i . Such a condition indicates that the firm has failed in its efforts to manage its market identity. In particular, its claim that it merits comparison with the full-fledged members of i has been rejected. Such illegitimacy increases investor reluctance to purchase the firm's shares. Thus, we may say the following:

HYPOTHESIS.—*Ceteris paribus, the greater the coverage mismatch suffered by a firm, the lower its stock price.*

DATA

I use three data sources: *Standard & Poors Compustat Industrial Annual* and *Industry Segment* files, *Zacks Historical Database*, and the *Center for the Study of Security Prices (CRSP)* database. Each of these data sets covers virtually every public corporation listed on American stock exchanges, including firms that ceased existence during or after this period, and may be linked to one another through common identifying variables. *Compustat* data include a large amount of financial information on publicly traded companies compiled primarily from quarterly and annual reports filed with the Securities and Exchange Commission (SEC). The *Industrial Annual* data set covers firm-level information, while the *Industry Segment* files include data on key variables for up to 10 industry segments or aggregated business lines (see, e.g., Davis, Dickmann, and Tinsley 1994). The *CRSP* database is an archive of daily stock market prices.

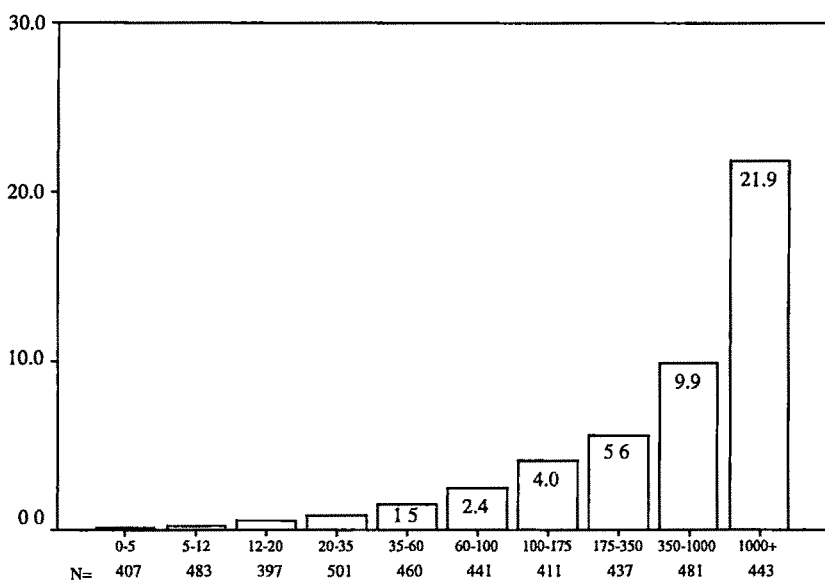
Finally, the *Zacks* data consist of dated earnings forecasts made by securities analysts. As the last database is less familiar, I describe it in detail.

Since the 1970s, several firms including *Zacks* have been collecting analysts' forecasts of corporate earnings. These data are useful for present purposes because every published earnings forecast registers a relationship between an analyst and a firm. Thus, ignoring the content of these forecasts and merely documenting their existence, one can trace the network of reviews between products and critics in the stock market. In particular, comparing the nature of coverage obtained by a firm and its industrial participation allows for the measurement of a firm's degree of mismatch with its stock market identity.

Zacks data do not contain the full set of earnings estimates made by sell-side analysts. However, there are strong reasons to consider these data as "missing at random" (Little and Rubin 1987) in that the pattern of missing data is unrelated to firm characteristics. First, as they are used in rankings for which analysts and their employers compete intensely, brokerage firms are highly motivated to publicize these forecasts. Second, as these forecasts are published by brokerage houses rather than the corporations being analyzed, there seems to be no basis for concern that the firms influence the pattern of missing data. Largely as a result of these considerations, financial economists and accounting researchers tend to treat these data as if the published forecasts reflect the complete range of information available to market participants at a particular point in time (see Givoly and Lakonishok 1984; Schipper 1991).

Furthermore, these data are largely complete in terms of the most prominent analysts, those who constitute the most significant element of the audience for corporate behavior. Of the 361 analysts who were ranked in 1985 by *Institutional Investor* as either first, second, third, or a runner-up in their coverage of particular industries, 292 or 81% are included among the 1,803 analysts whose forecasts were included in the *Zacks* data for that year. Further, 56 of the 69 missing analysts work for two large brokerage firms, each of which had a policy of not publishing their analysts' forecasts. Excluding analysts who work for these firms, forecasts made by almost 98% of these high-ranking analysts appear in the 1985 data.

Finally, the level of coverage in the *Zacks* data follows expected patterns. In particular, the number of analysts that follow a firm is highly correlated with its size, as indicated in figure 2 (cf. Bhushan 1989). In addition, analyst specialization by industry is evident in the *Zacks* data, as illustrated by figure 3. This graph shows the standard deviation above and below the mean proportion of firms covered by an analyst for the top



Market Value

FIG. 2.—Mean analyst coverage by market value, 1985

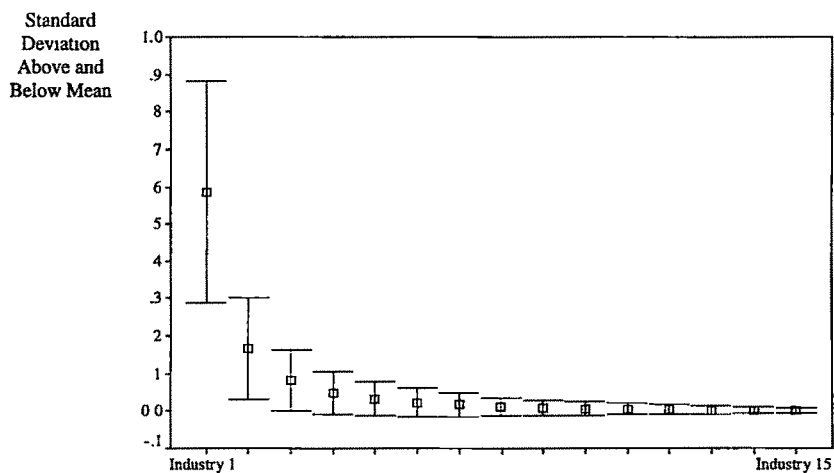


FIG. 3.—Analyst specialization by industry, 1985: proportion of analyst attention given to top 15 three-digit industries.

15 three-digit industries that included any firms followed by that analyst.¹⁰ We see that, on average, analysts devoted 57% of their coverage to a single three-digit industry and that these proportions drop steeply such that very few analysts follow firms in more than a handful of industries.

ANALYTICAL FRAMEWORK

I test for the presence of an “illegitimacy discount” using an approach pioneered by Berger and Ofek in their study of the impact of diversification on firm value (Berger and Ofek 1995; cf. LeBaron and Speidell 1987; see also Berger and Ofek 1996; Denis, Denis, and Sarin. 1997). Three measures of “excess value” or the discrepancy between an imputed value of a firm and its actual value are constructed (Berger and Ofek 1995, pp. 60–61). The basis for such imputations begins with a calculation, for every single-segment firm, of its ratio of total capital (the market value of a firm’s common stock—the number of shares outstanding multiplied by the share price at year’s end—plus the book value of its debt) to its sales, assets, and EBIT (earnings before interest and taxes).¹¹ Next, median ratios by industry are computed, using the most detailed SIC class that has at least five firms with valid data and at least \$20 million in sales. An imputed value for the firm is then:

$$I(V) = \sum_{i=1}^n AI_i^* (Ind_i(V/AI)_{mf}), \quad (1)$$

where $I(V)$ is the estimated value of the firm; AI_i is segment i ’s sales, assets, or EBIT; $Ind_i(V/AI)_{mf}$ is the median ratio of total firm capital to sales, assets, or EBIT in segment i ’s corresponding industry; V is total capital; and n is the number of segments reported by the firm.

This formulation applies valuation standards from single-segment firms to the full set of firms, which do business in one or more industry segments. For a given segment, the imputed value reflects its sales, assets, or profits multiplied by the median ratio of total capital to that variable for the corresponding industry. The predicted value for a diversified firm is the sum of these segment values. *Ceteris paribus*, a given firm should match these ratios for each of the industries in which it does business. Thus,

¹⁰ As below, an analyst is coded as covering a firm when he publishes at least one forecast for a firm in the relevant year.

¹¹ While the present argument concerns firms’ stock prices, corporate debt is included in such calculations because the worth of a firm should reflect its value to all claimants—lenders and bondholders, as well as shareholders. However, virtually the same results obtain when debt is removed from such calculations.

when a firm's participation in its full profile of segments is viewed favorably by investors, the firm's market value will exceed the sum of the segment-level imputed values. This discrepancy or "excess value" is measured as a log ratio: $\ln(V/I(V))$. Note that, as public firms follow different fiscal calendars, excess value is computed at the end of a firm's fiscal year. Relevant share price data come from the *CRSP* database.

Calculation of excess value assumes that summed segment-level data on sales, assets, and EBIT equal firm-level data on such variables. However, discrepancies may occur due to accounting decisions left to managerial discretion (see Lichtenberg 1991). As do Berger and Ofek (1995, pp. 60–61), I correct these discrepancies in the following manner. First, the 5% of firms for which the sales-based discrepancy is greater than 1% are treated as missing on excess value variables. Discrepancies are more common in the case of assets (72% of cases) and profits (93%). To correct for this, the 5% of cases where the assets-based discrepancy and the 13% of cases where the EBIT-based discrepancy exceeds 25% are treated as missing on the respective variables. For the 67% of the assets-based and 80% of the EBIT-based discrepancies that are within 25% of the total, the segment variables for each are recalculated by multiplying the segment's proportion of the segment sum for that variable by the firm's total. A final correction is performed for firms with negative EBIT. To avoid giving such segments negative values, the EBIT plus depreciation is used (EBITD). When EBITD is negative as well, the sales ratio is used instead (Berger and Ofek 1995, pp. 61–62).

VARIABLES IN THE ANALYSIS

Coverage Mismatch

The stated hypothesis concerns the relative frequency with which the analysts who specialize in a given industry follow the firms who participate in that industry. Calculation of "coverage mismatch" requires three kinds of information: the industry or industries with which a firm claims affiliation, the identities of the analysts who cover the firm, and analyst industry specialties. Measurement of the first and second issues is straightforward. SEC filings represented in the *Industry Segment* file indicate the SIC codes of the industries in which firms claim participation. An analyst is coded as covering a firm when he publishes at least one earnings forecast for that firm during the relevant fiscal year. The identification of analyst industry specialties is somewhat more complicated due to the fact that such specialties are not given in the *Zacks* data. Furthermore, while such publications as *Nelson's Directory of Investment Research*, a professional directory, give industry specialties, industries are not assigned in terms of the SIC

TABLE 1
MINIMAL PROPORTIONS FOR ESTABLISHING ANALYST COVERAGE OF AN INDUSTRY

No. of Firms with Highest Proportion of Sales in the Industry	Minimal Proportion for Analyst Coverage	No. of Relevant Three-Digit Industries, 1986
1-3	1	165
4-580	30
6-1060	36
11-1550	9
16-2040	9
21-2530	5
26+20	3

codes given in firms' SEC filings. However, firms' identity claims and analyst attributions of identity must be comparable for coverage mismatch to be meaningful.

Thus, industry specialism must be defined on the basis of an analyst's observed tendency to cover firms that affiliate with a particular SIC code.¹² In particular, an analyst is coded as specializing in an industry when he follows at least a minimal proportion of all industry-based firms that receive any analyst coverage.¹³ To control for industry size, this proportion varies based on the number of covered firms in the industry, as shown in table 1. Note that each of these conditions has been submitted to sensitivity tests, which indicate that the measurement of coverage mismatch is robust across alternative criteria for establishing analyst industry specialization. In addition, informal comparisons of the industry specialties generated by this procedure with those listed in *Nelson's Directory of Investment Research* reveal broad agreement.

Having designated industry specialists, I calculate coverage mismatch as follows. First, define c_{if} for a firm that participates in industry i as the

¹² For these calculations, multisegment firms are assigned to their primary industry on the basis of sales. Absent this assumption, the pattern of diversification would influence the observed pattern of analyst specialization. However, this assumption proves to have little impact in that assignment of analysts to industry specialties is substantially the same when multisegment firms are removed from these calculations. Note further that this assumption is not applied to the calculation of coverage mismatch.

¹³ Using the number of firms in an industry that receive coverage as the denominator controls for interindustry differences in the likelihood of attracting coverage. An alternative denominator would be the number of firms covered by the analyst in question. Results change little when such a measure is used.

number of specialists in industry i who follow the firm. Then, coverage mismatch is measured:

$$cm_{fi} = 1 - (c_{fi}/\max(c_{gi})), \quad (2)$$

where c_{gi} is the number of industry specialists in industry i covering firm g and $\max(c_{gi})$ is the maximum taken over all firms in industry i . While a score of zero indicates that the firm has attracted the greatest number of analysts who specialize in industry i to cover the firm, a score of 1 means that it has attracted none of these industry specialists. Using the maximum, rather than the total, number of industry specialists as the denominator standardizes the criterion across industries.

For single-segment firms, coverage mismatch is as above. For diversified firms, a firm-level measure of coverage mismatch may be generated by taking the average of the segment-level scores weighted by the size of the segments:

$$cm_f = \sum_{s=1}^S w_s^* cm_{fs}, \quad (3)$$

where S refers to the number of segments reported by firm f and w_{fs} refers to the proportion of total sales—or assets, if sales data are unavailable—that the segment represents.

A final issue in making such calculations concerns the appropriate level of industry aggregation. In the analyses presented below, I consider industries at the three-digit SIC level. The three-digit level is chosen largely because this middle range of aggregation gives a more useful rendering of the analyst coverage structure. By contrast, aggregation at the two-digit level produces a highly skewed distribution in that a small number of industries are associated with a disproportionate share of firms and analysts. Conversely, aggregation at the four-digit level generates many industries, each of which tends to have a small number of firms and analysts associated with it. The three-digit level provides a useful middle ground between these extremes. Nevertheless, analyses performed at the two- and four-digit levels generate results that, while slightly weaker, are highly consistent with those that emerge at the three-digit level.

An Illustration: Restaurants and Diversification

Table 2 illustrates the calculation of coverage mismatch in SIC code 581, "eating and drinking places" or restaurants, in 1985. The 20 public American operating companies with the greatest amount of sales in this industry are presented. In addition, summary statistics are presented on these firms, the 37 industry participants that received any analyst following, and the

TABLE 2

COVERAGE MISMATCH FOR 20 LARGEST PARTICIPANTS IN RESTAURANT INDUSTRY, 1985*

Company	Sales, Restaurant Industry†	Total Firm Sales†	No. of Industry Segments	Analysts Covering Firm	Restaurant Specialists		Segment-Level Coverage		Firm-Level Coverage	
					Covering Firm	Specialists	Coverage Mismatch	Coverage Mismatch	Coverage Mismatch	Coverage Mismatch
TGI Friday's, Inc.	311.82	311.82	1	8	6	6	.6			.60
El Torito Restaurants, Inc.	318.85	318.85	1	6	2	2		.87		.87
Karcher Carl Enterprises, Inc.	322.75	322.75	1	12	6	6		.60		.60
Collins Foods Intl., Inc.	345.39	535.67	2	8	2	2		.87		.90
Vicorp Restaurants, Inc.	379.90	379.90	1	10	7	7		.53		.53
Carson Pirie Scott & Co.	444.85	1,301.89	3	1	0	0		1.00		1.00
Hershey Foods Corp.	471.49	1,996.15	3	13	0	0		1.00		.37
Ponderosa, Inc.	480.77	480.77	1	8	4	4		.73		.73
Jerrico, Inc.	516.48	516.48	1	9	7	7		.53		.53
Shoney's, Inc.	540.57	540.57	1	18	10	10		.33		.33
Church's Fried Chicken, Inc.	543.68	543.68	1	15	10	10		.33		.33
Ralston Purina Co.	564.50	5,863.90	4	29	2	2		.87		.75
General Mills, Inc.	1,051.00	4,586.60	3	30	0	0		1.00		.36
Wendy's International, Inc.	1,100.11	1,127.50	1	23	14	14		.07		.07
Saga Corp.	1,254.80	1,254.83	1	9	6	6		.60		.60
Transworld Corp.	1,448.63	2,151.55	2	11	2	2		.87		.59
Pepsico, Inc.	2,081.10	8,056.65	3	29	2	2		.87		.51
Marriott Corp.	2,343.30	4,241.70	2	14	7	7		.53		.24
Pillsbury Co.	2,663.20	5,847.89	2	28	4	4		.73		.43
McDonalds Corp.	3,694.75	3,694.75	1	30	14	14		.07		.07
Averages for 20 largest industry participants	1,043.90	2,203.70	1.75	15.55	5.25	5.25		.65		.53
Averages for 54 smaller firms with any coverage	73.49	139.58	1.42	4.98	2.87	2.87		.81		.76
Averages for 51 smaller firms with no coverage	25.90	54.245	1.55	0	0	0		1.00		1.00

* Twenty largest public American operating companies in terms of their reported sales in SIC code 581, "eating and drinking places"

† Numbers are given in millions

36 firms that received no analyst attention. On the basis of the calculations described above, 10 analysts were identified as specializing in this industry in that they published earnings forecasts for at least 20% of the 57 firms that received any coverage. Among all participants in restaurants, Chi-Chi's, Inc., which ranked twenty-first in industry sales, was followed by all industry specialists, resulting in a coverage mismatch score of 0.0. By contrast, Pepsico, a company that participated in this industry at a level nearly 20 times that of Chi-Chi's, was followed by only two industry specialists. That its firm-level coverage mismatch score was as high as 0.51 stems from the fact that it was followed by the maximum number of beverage industry specialists.

The contrast between these firms illustrates an important feature of the analyst division of labor. While analysts specialize by industry, they are responsible for individual firms. As a result, diversified firms present a classificatory challenge. By their very nature, such corporations embody the issues raised by any product that deviates from an existing competitive frame: "To which industry does such a firm belong?" "Which analyst should cover it?" "To what should it be compared?" Indeed, the tension between diversification and analyst coverage patterns suggests an alternative explanation for the "diversification discount," the penalty suffered by firms that do business in multiple industries. Conventional accounts suggest that this discount, which led to a significant wave of "dediversification" during the 1980s and 1990s (Davis et al. 1994; Lang and Stultz 1994), reflects the inefficiency of the conglomerate form (e.g., Porter 1987; Jensen 1988, 1993; Berger and Ofek 1995). However, while such firms may indeed be less efficient, the newspaper excerpt presented in figure 4 illustrates the very different problem that, by straddling multiple industries and corresponding analyst specialties, diversified firms hinder efforts at cross-product comparison. Indeed, diversified firms are significantly more likely to divest segments with high coverage mismatch (Zuckerman 1998).

Control Variables

I include in analysis the four variables considered by Berger and Ofek (1995, pp. 50–51) to affect firm value: the log of the firm's assets, a measure of firm size; the EBIT to sales ratio, an indicator of firm profitability; the ratio of capital expenditure to sales, which represents growth opportunities; and the extent of a firm's diversification. I measure the latter with a sales-based Herfindahl index, which tends toward zero as a firm's sales are spread out among many segments. However, note that other measures of diversification, such as the number of segments and a measure of intersegment similarity, display substantially the same patterns of association with excess value (Zuckerman 1997, chap. 5). In addition, following

Why Is Indresco's Stock Lagging?

It's not that Indresco is a financial loser. It's just difficult to describe what Indresco does.

The hodgepodge of businesses it owns bothers Wall Street.
Financial analysts can't categorize Indresco.

"Indresco's crisis is defining what it is: Are they an underground mining equipment manufacturer? A refractories company?" says [Tobias] Levkovich, [a] Smith Barney analyst.

Some analysts are calling for Indresco to narrow its focus by shedding businesses, such as the air tools and mining equipment units.

J. L. Jackson, CEO: "We're not going to get out of some business *so we can make some analyst happy, so he can compare us* with just the steel business or just the refractories business."

Dallas Morning News March 14, 1995

FIG. 4.—Newspaper excerpt illustrating the valuation challenges faced by diversified firms (emphasis added).

Denis et al. (1997), I include the R&D expenditure to sales ratio as an additional measure of growth prospects.¹⁴

The number of analysts who follow the firm represents an important control factor as well. First, as has been shown in the case of critics in other markets (Shrum 1996; Eliashberg and Shugan 1997), it may be that the sheer magnitude of analyst attention, rather than the specialization of their coverage, affects firm value. In addition, several scholars suggest that the opposite pattern may occur: analysts respond to increases (decreases) in a firm's stock price by increasing (decreasing) their coverage (Bhushan 1989; McNichols and O'Brien 1997). Thus, to the extent that low coverage mismatch reflects a large analyst following and the latter is

¹⁴ They also consider a firm's advertising/sales and its debt/equity ratios and show them to be unrelated to excess value.

generated by a low market value, including the number of analysts takes this reciprocal effect into account.

ANALYSIS

I explore the relationship between coverage mismatch and excess value over the years 1985–94. As do Berger and Ofek (1995, p. 43), I exclude firms that had any segments in financial services industries (SIC codes between 6,000 and 6,999) because many such corporations do not report their earnings before interest and taxes (EBIT). In addition, I restrict the study population to American operating companies that are listed on major exchanges. This excludes foreign firms that are listed in American stock markets—and which thus file data with the SEC—as well as subsidiaries, LBOs, and private or rarely traded firms, which sometimes appear in *Compustat* as well.¹⁵

Table 3 gives descriptive statistics and a correlation matrix for the variables used in analysis. Several patterns are worthy of note. First, while the sales and assets-based excess value scores are highly correlated, the EBIT-based measure is only moderately correlated with the others. This corresponds with generally weaker associations between EBIT-based excess value and all other variables (Berger and Ofek 1995, pp. 48, 51) and may reflect the measurement difficulties described above. Accordingly, Denis et al. (1997) disregard the EBIT-based measure. Finally, note that the R&D intensity measure is missing for many firm years. Accordingly, I compare models that include this variable with those that exclude it.

As I consider multiple observations of the same firms, I model this association using fixed-effects regression analyses (e.g., Hannan and Young 1977).¹⁶ Thus, coefficients represent within-firm differences across years. Random-effects models, which make more liberal assumptions about the nature of serial correlation, produce virtually the same results. Tables 5–6 present the fixed-effects results for the sales, assets, and EBIT-based measures respectively.

Models 1 and 2 assess the impact of the control variables. We see that the patterns differ little across tables. However, somewhat discrepant re-

¹⁵ Note as well that, unlike Berger and Ofek (1995, p. 61), I include firms with extreme values on the excess value scores—those for which the imputed value is more than four times greater or smaller than its actual market value. Berger and Ofek do not explain this exclusion. Indeed, that there seem to be no clear breaks in the distribution of the excess value variables, and that the variance explained actually increases slightly when firms with extreme values are included, suggests that the method works equally well for these cases.

¹⁶ In particular, I estimate the models using the “xtreg” procedure in *Stata 5.0* (Stata Corporation, College Station, Tex.).

TABLE 3
SUMMARY STATISTICS AND CORRELATIONS

ID	Variable	Valid <i>N</i> (Firm-Years)	Mean	SD	Minimum	Maximum	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
[1]	Excess value— <i>sales based</i> ...	27,597	-.13	1.04	-16.31	4.20	.								
[2]	Excess value— <i>assets based</i>	26,346	-.12	.96	-16.16	3.71	90								
[3]	Excess value— <i>EBIT based</i>	24,652	.12	1.02	-16.27	9.70	.61	.59							
[4]	Log of assets	31,602	5.12	1.69	.06	11.43	.14	.04	.00						
[5]	EBIT/Sales	31,602	.07	.14	-3.92	.78	.25	.26	-.02	.21					
[6]	Capital expenditure/Sales	31,116	.09	.19	.00	8.31	.13	.05	.10	.12	.03				
[7]	Diversification (Herfindahl) ..	31,600	.92	.14	.34	1.00	.13	.15	.15	.31	.00	.03			
[8]	R&D/Sales	15,945	.05	.09	.00	3.41	.13	.07	.14	-.06	-.35	.16	.13		
[9]	No. of analysts	31,602	6.26	8.33	0	62	.26	.22	.09	.71	.20	.07	-.16	.13	.
[10]	Coverage mismatch	31,526	.73	.35	.00	1.00	-.22	-.19	-.07	.50	-.16	-.06	-.02	.00	-.59

sults appear in table 6. In particular, while the log of analyst coverage has a strong negative association with the sales and assets-based excess value, its impact with the EBIT-based measure is much weaker. Further, while profitability (EBIT/sales) has the expected large positive effect on excess value in tables 4 and 5, this variable has no effect in the case of the EBIT-based measure of excess value. These surprising results suggest that we treat results based on this dependent variable with some caution.

Models 3 and 4 introduce coverage mismatch as a covariate. The results from each table strongly support the stated hypothesis. That is, corporations that succeed in attracting recognition from the analysts who specialize in their industries enjoy greater financial market success. Firms that fail to reduce their level of coverage mismatch trade at a discount. This effect is significant even when controlling for a host of factors that affect valuations, including the sheer size of a firm's analyst following.

It is useful to get a sense of the magnitude of this effect. Consider once again the case of PepsiCo in 1985, a diversified firm that received little or no coverage from analysts who specialized in its non-core-industry segments. PepsiCo's total capital in 1985 was \$6.57 billion and its sales-based imputed value was \$4.3 billion, generating a sales-based excess value of .42. According to model 2 in table 4, were PepsiCo to lower its coverage mismatch from 0.51 to 0—the level attained in its core segment of "Beverages," its excess value score would increase by $(.51) * (.136)$ or .07 to .49. Exponentiating this number and multiplying it by the imputed value generates a potential total capital of \$7.04 billion, indicating that PepsiCo's heightened level of coverage mismatch reduces its value by \$472 million, a discount of 7.2%. Slightly smaller but quite large estimates of the discount are obtained for the assets and EBIT-based measures respectively. It seems clear then that changes in coverage mismatch can amount to vast sums of money. Indeed, arbitrageurs compete to find price discrepancies that are much smaller than this one. In sum, a firm that does not succeed in cultivating an analyst review network befitting the desired corporate identity suffers a significant devaluation on Wall Street.

Possible Spuriousness

Two challenges may be raised to this conclusion. First, perhaps the set of control variables included does not exhaust the range of possible factors that underlie the association between coverage mismatch and excess value. In particular, efficient-markets theory would insist that there must be information about the firms' future prospects that impacts both analyst coverage patterns and excess value, resulting in a spurious association between these variables.

It seems doubtful that information about firm's future earnings could

TABLE 4

MODELS OF EXCESS VALUE BASED ON SALES-RATIO, 1985-94

VARIABLE	CONTROL VARIABLES		INCLUDING COVERAGE MISMATCH		INCLUDING EBIT/ SALES ($t + 1$) (Model 5)	CHANGE MODEL: INCLUDING EXCESS VALUE ($t - 1$) (Model 6)
	Model 1	Model 2	Model 3	Model 4		
Log of assets	-.105 (.016)**	-.101 (.012)**	-.108 (.016)**	-.105 (.012)**	-.121 (.012)**	-.072 (.013)
EBIT/sales	1.955 (.083)**	1.296 (.049)**	1.951 (.083)**	1.286 (.049)**		1.185 (.052)**
Capital expenditure/sales	1.633 (.111)**	.595 (.025)**	1.612 (.110)**	.593 (.034)**	.553 (.036)**	.873 (.087)**
Diversification (Herfindahl)	1.173 (.127)**	1.154 (.088)**	1.141 (.128)**	1.119 (.088)**	1.172 (.088)**	.460 (.064)**
Number of analysts	.016 (.002)**	.022 (.002)**	.013 (.002)**	.019 (.002)**	.021 (.002)**	.011 (.002)*
R&D expenditure/sales	1.433 (.163)**		1.430 (.163)**			
Coverage mismatch			-.153 (.034)**	-.136 (.025)**	-.129 (.025)**	-.093 (.024)**
EBIT/sales ($t + 1$)					.493 (.049)**	
Excess value ($t - 1$)						.339 (.007)**
Intercept	-1.053 (.142)**	-.954 (.104)**	-.874 (.147)**	-.782 (.108)**	-.681 (.109)**	-.673 (.110)**
Firm differences (F -statistic)	6.91**	6.74**	6.82**	6.70**	7.21**	2.20**
No. of firms	2,921	5,479	2,918	5,470	5,024	4,687
Total N	14,006	27,217	13,937	27,144	25,230	23,170
Model DF	11,079	21,733	11,048	21,668	20,200	18,476
R^2	.145	.111	.152	.115	.082	.512

NOTE—SEs are in parentheses.

* $P \leq .05$ ** $P \leq .01$

TABLE 5
MODELS OF EXCESS VALUE BASED ON ASSETS-RATIO, 1985-94

VARIABLE	CONTROL VARIABLES		INCLUDING COVERAGE MISMATCH		INCLUDING EBIT/SALES ($t + 1$)		CHANGE MODEL INCLUDING EXCESS VALUE ($t - 1$)	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 5	Model 6
Log of assets	-.249 (.015)***	-.270 (.012)***	-.251 (.075)***	-.274 (.012)***	-.277 (.012)***	-.211 (.012)***		
EBIT/sales	2.313 (.079)***	1.506 (.047)***	2.308 (.079)***	1.498 (.047)***		1.311 (.050)***		
Capital expenditure/sales821 (.100)***	.322 (.032)***	.809 (.100)***	.320 (.032)***	.265 (.034)***	.106 (.037)***		
Diversification (Herfindahl)	1.025 (.131)***	1.040 (.091)***	.997 (.132)*	1.009 (.091)***	1.030 (.091)***	.656 (.093)***		
Number of analysts020 (.002)***	.027 (.002)***	.018 (.002)***	.025 (.002)***	.027 (.002)***	.015 (.002)***		
R&D expenditure/sales	1.051 (.1553)***		1.049 (.153)***					
Coverage mismatch			-.110 (.032)***	-.121 (.023)***	-.112 (.024)***	-.093 (.024)***		
EBIT/sales ($t + 1$)647 (.047)***			
Excess value ($t - 1$)								
Intercept	-.197 (.145)	-.020 (.106)	-.066 (.150)	.133 (.110)***	.210 (.110)*	.318 (.007)***		
Firm differences (F -statistic)	6.474**	6.15***	6.36***	6.09 ^{DOWN}	6.50***	.229 (.113)**		
No of firms	2,858	5,405	2,855	5,396	4,952	2.24***		
Total N	13,274	25,981	13,241	25,908	24,060	4,577		
Model DF	10,410	20,571	10,379	20,506	19,102	21,656		
R^2096	.064	.101	.067	.048	17,092		
						.366		

NOTE.—SEs are in parentheses
* $P \leq .10$.
** $P \leq .05$
*** $P \leq .01$.

TABLE 6
MODELS OF EXCESS VALUE BASED ON EBIT-RATIO, 1985-94

VARIABLE	CONTROL VARIABLES		INCLUDING COVERAGE MISMATCH		INCLUDING EBIT/ SALES ($t + 1$)		CHANGE MODEL INCLUDING EXCESS VALUE ($t - 1$)	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 6	Model 6
Log of assets	-.027 (.023)	-.007 (.017)	-.029 (.023)	-.009 (.017)	-.055 (.017)***		.014 (.019)	
EBIT/sales	-.014 (.144)	.092 (.065)	-.010 (.014)	.088 (.065)			.129 (.078)*	
Capital expenditure/sales	1.158 (.145)***	.643 (.049)***	1.153 (.145)***	.642 (.049)***	.525 (.052)***		.705 (.066)***	
Diversification (Herfindahl) ..	1.058 (.252)**	1.214 (.153)***	1.039 (.253)***	1.195 (.153)***	1.183 (.151)***		.923 (.182)***	
Number of analysts007 (.003)**	.010 (.002)***	.005 (.003)	.009 (.002)***	.010 (.002)***		.010 (.002)***	
R&D expenditure/sales417 (.230)*		.418 (.230)*					
Coverage mismatch ..								
EBIT/sales ($t + 1$)			-.105 (.050)**	-.068 (.033)**	-.054 (.033)*		-.077 (.036)**	
Excess value ($t - 1$) ..					-.341 (.065)***			
Intercept	-.853 (.255)***	-1.127 (.170)***	-.734 (.273)***	-1.038 (.175)***	-.752 (.174)***		.062 (.008)***	
Firm differences (F -statistic) ..	2.64***	2.93***	2.64***	2.93***	3.07***		1.94***	
No. of firms	2,758	5,220	2,755	5,211	4,777		4,306	
Total N	12,494	24,321	12,461	24,248	22,540		19,864	
Model DF	9,730	19,096	9,699	19,039	17,757		15,551	
R^2049	.034	.050	.034	.041		.073	

NOTE.—SEs are in parentheses

* $P \leq 10$

** $P \leq 05$

*** $P \leq .01$

affect coverage mismatch net of its impact on the size of analyst coverage. Nevertheless, in an attempt to address this possibility, model 5 in tables 4–6 replaces EBIT/sales with EBIT/sales of the following year. If information about earnings prospects is governing analyst coverage, inclusion of this variable should attenuate the effect of coverage mismatch. However, we see that there is little difference in the effect of coverage mismatch across models 4 and 5. Indeed, EBIT/sales at year $t + 1$ has a *weaker* effect on excess value than does EBIT/sales at year t . This finding supports the notion that stock prices reflect not a firm's earnings prospects but the prevailing interpretation of such prospects based on past profits and other factors.

A second objection represents the opposite concern. Rather than reflections of future profits, perhaps analyst coverage patterns represent past price movements. That is, as has been found for the magnitude of analyst coverage (Bhushan 1989; McNichols and O'Brien 1997), stock prices may influence coverage mismatch rather than vice versa. Again, this conjecture seems doubtful as it is highly unlikely that excess value could influence coverage mismatch net of its association with the size of a firm's analyst following. This would require that, as a result of poor financial market performance, the analysts who cover a firm's industries are replaced by analysts who follow other industries. It is difficult to explain how and why such a process would occur, especially on a systematic basis.

Nevertheless, to explore the possibility of reciprocity, model 2 is reestimated as a change model, which is presented in model 6. For this equation, excess value in the prior year is included as an additional predictor of excess value in a given year. With lagged excess value variable as a regressor, the other variables now predict yearly change in excess value rather than its level in a given year. Note that this framework makes the very conservative assumption that coverage mismatch and the various control variables have no impact on excess value except on a year-to-year basis. Nevertheless, results from model 6 in each table indicate that coverage mismatch has a strong effect on excess value even when reciprocity is taken into account in the described fashion. For all three dependent variables, the lagged variable has a strong association with the subsequent year's excess value. However, the coefficient for coverage mismatch—as well as the other covariates—remains quite significant. It would seem that the impact of coverage mismatch on excess value is strong and is not due to reverse causation.

CONCLUSIONS

The preceding analysis demonstrates clear support for the proposed theory. For a product to compete in any market, it must be viewed by the

relevant buying public as a player in the product categories in which it seeks to compete. In the case of mediated markets, this requires reviews from the critics who follow those categories. In the specific instance of the stock market, a firm seeks coverage from the analysts who follow the industries in which it participates. I have shown that success or failure at gaining such recognition has a significant impact on a firm's fate in financial markets. All other things held equal, firms that cultivate an ego-centric network of reviews to securities analysts that reflects its industrial participation are more highly valued than those that do not.

This analysis offers lessons for several fields of study: research on role and product conformity, the social structure of markets, network theory, and theories of capital markets. I discuss implications for each of these areas in turn.

Pressures toward "Offer" Conformity

I address two weaknesses in previous research: a failure to validate the presumption that actors are punished for deviating from accepted roles and a neglect of the audience that metes out such punishment. The proposed framework fills in these holes in a manner that facilitates the integration of several seemingly disconnected lines of theory. It allows for an understanding of homogenous practice in a wide variety of settings, collapsing the wall thought to separate market and nonmarket isomorphism. As illustrated above, perspectives as seemingly diverse as marketing theory, neoinstitutionalism, and White's market model emerge as commentaries on the same social dynamic. Further, by highlighting what these theories leave implicit or ignore—the role of the audience in disciplining deviation from accepted roles—the proposed framework lends itself to an analysis of effects that had previously been merely assumed or even ignored.

The Significance of Market Mediation

A focus on the audience pays great dividends when we shift our view of market structure from buyer-seller to critic-seller interaction. Apart from the work of a few scholars (Hirsch 1972, 1975; Shrum 1996; Eliashberg and Shugan 1997; see also Rao 1998), the role of critics and other third parties in shaping consumption patterns has been overlooked. However, appreciating the significance of such intermediaries is necessary for moving beyond the neoclassical image of market transactions as diffuse, ephemeral, and anonymous and thereby appreciating the enduring relationships that frequently govern such exchange. Indeed, Granovetter's (1985) analysis of the social embeddedness of market exchange attends to

the social context for such transactions but ignores their structure. Further, his framework would seem inapplicable to mass markets, which do appear to consist of arms-length encounters between anonymous actors. Accordingly, even structural sociologists such as White concede that consumers are essentially unobservable and irrelevant to sellers.

The present analysis joins with such sociologists as Burt (1982, 1983, 1992; cf. Burt, Christman, and Kilburn 1980), Baker (1984), Gerlach (1992), and Podolny (1993, 1994) in attending to the structure of economic relationships in addition to the manner of their embeddedness. By focusing on seller-critic relationships, I provide insight into the structuration of mass markets, which may be missed by looking at seller-buyer relationships alone. In markets where seller-buyer relations would seem to be fleeting and anonymous, fixing analytic attention on seller-critic relationships reveals highly structured and enduring patterns of interaction between actors who are acutely aware of one another's existence. These relationships do more than embed market transactions; they shape such exchange and give a market its character.

Network Theory

A distinctive aspect of the analysis presented here is that I derive network effects not from an actor's structural position but from how that position relates to the ideal-typical one implied by a desired identity. The prevailing stress of research on the effects of networks on individual experience concerns either the advantages that are derived from occupancy of a particular social position (e.g., Burt 1992; Podolny 1993) or the role of networks in the diffusion of attitudes and behavior (e.g., Laumann 1973; Burt 1987). In each of these modes of analysis, networks are taken as given, and an actor's network position confers her social identity.

By contrast, I focus on the mismatch between actual and idealized networks. I argue that an actor may hold a self-concept that differs from that given in a network of attributions and that this disjuncture has implications for future behavior. What most distinguishes the analyst coverage structure from the networks generally studied by network analysts is that it is possible to specify from the outset what the structure should look like. In particular, knowing a firm's industrial participation generates clear expectations for how the firm should relate to analysts. Accordingly, rather than the content or structure of an actor's ties, the causal spark in my analysis lies in actor's deviation from expected tie patterns.

The Uncertainty of Financial Valuation

Finally, the application of the present perspective to a financial market speaks to the very nature of such markets. As argued above, failure to

gain certification by the critics who cover a product category can matter only in contexts where the relevant structural features are in place and where the value of products is subject to significant uncertainty. Indeed, such structures exist only where products are of uncertain value and, in particular, where buyers are sensitive to the purchasing decisions of others.

Armies of interpreters and prognosticators are present on Wall Street because they fill an important social purpose: they help investors make sense of the dizzying array of possible investments. No such investment has a clear value, and the struggle to anticipate future prices never ends. Similarly, such an environment spawns investor relations, the marketing of corporate equity. Corporate executives promote their financial products to investors and the intermediaries who influence investor opinion. Indeed, this particular product-critic interface lends itself to the dynamics explored in the present analysis; as in other product markets, firms that do not gain membership in accepted categories are punished.

Thus, the present article builds on recent research in behavioral finance, which suggests that cognitive limitations can induce a gap between price and value and that the risks of arbitrage keeps such gaps open. I have added to this critique, arguing that, as the value of a financial asset is inherently uncertain, valuation is necessarily an interpretive exercise. Further, while responses to uncertain conditions generally induce processes of social comparison (Festinger 1954), social contagion is especially likely where, as with financial assets, such conditions are determined by others' actions. Thus, share prices reflect the theories of value that have gained currency among prevalent market players, and their constant flux reflects continuing theoretical debate (Shiller [1990] 1993). Indeed, rather than discontinuous breaks in the normal workings of financial markets, the bubbles and crashes that periodically occur indicate that a particular theory of valuation has gained widespread acceptance.¹⁷

Further, note that the very industry-based category structure analyzed in this article is itself contingent on the prevalence of a particular theory of value. There is nothing inherent or timeless about this structure. Rather, it reflects the dominance of the "pragmatic" theory of valuation (Burk 1988; cf. Babson 1967), which has dominated since the 1930s and which spawned the profession of security analysis, whose specialization by industry reflects the timing of its birth. While earlier theories tended to value stocks on a firm's assets (see Burk 1988) or its dividend disbursements

¹⁷ Frankel and Froot's (1990) argument that the dollar bubble of the mid-1980s reflected a shift in the prevalent theory of valuation from a "fundamentalist" to a "chartist" perspective illustrates this nicely

(see Baskin and Miranti 1997), the pragmatic perspective prices firms on their earnings power and stresses that industries discriminate firms on this basis (e.g., Graham and Dodd [1934] 1940). However, the possibility of alternative theories of value and rival classificatory schemes persists, as indicated by the initial welcome received by the conglomerate firm in the 1960s (e.g., Shleifer and Vishny 1991). Such challenges to prevailing theories of value illustrate the fact that financial markets are necessarily open, rather than closed systems, and that value is fundamentally indeterminate.

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Where Do Interorganizational Networks Come From?¹

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Organizations enter alliances with each other to access critical resources, but they rely on information from the network of prior alliances to determine with whom to cooperate. These new alliances modify the existing network, prompting an endogenous dynamic between organizational action and network structure that drives the emergence of interorganizational networks. Testing these ideas on alliances formed in three industries over nine years, this research shows that the probability of a new alliance between specific organizations increases with their interdependence and also with their prior mutual alliances, common third parties, and joint centrality in the alliance network. The differentiation of the emerging network structure, however, mitigates the effect of interdependence and enhances the effect of joint centrality on new alliance formation.

INTRODUCTION

Sociologists have made considerable progress in explaining why organizations behave as they do in terms of their embeddedness in social networks (Granovetter 1985, 1992; Swedberg 1994; Powell and Smith-Doerr 1994), but they have seldom examined how those networks originated. With few exceptions, largely limited to the research on interlocking directorates

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(e.g., Useem 1984; Palmer, Friedland, and Singh 1986; Mizruchi and Stearns 1988) or to thick historical accounts of the development of particular interorganizational networks (e.g., Stern 1979), organizational sociologists have typically viewed network formation as driven by exogenous factors, such as the distribution of technological resources or the social structure of resource dependence (Pfeffer and Salancik 1978; Burt 1983). In this view, organizations create ties to manage uncertain environments and to satisfy their resource needs; consequently, they enter ties with other organizations that have resources and capabilities that can help them cope with these exogenous constraints.

The exogenous approach to tie formation provides a good explanation of the factors that influence the propensity of organizations to enter ties, but it overlooks the difficulty they may face in determining with whom to enter such ties. This difficulty, which results from the challenges associated with obtaining information about the competencies, needs, and reliability of potential partners (Van de Ven 1976; Stinchcombe 1990), is especially vivid in the case of interorganizational strategic alliances. Alliances are a novel form of voluntary interorganizational cooperation that involves significant exchange, sharing, or codevelopment and thus results in some form of enduring commitment between the partners. While strategic alliances can be a means to manage environmental uncertainty, there is also considerable uncertainty associated with entering those cooperative ties. Imperfect information about potential partners raises search costs and the risk of exposure to opportunistic behavior (Gulati 1995a; Gulati and Singh 1999). Thus, while exogenous factors may suffice to determine whether an organization should enter alliances, they may not provide enough cues to decide with whom to build those ties. Where do organizations find those cues? And how do the particular cues they use shape the formation of interorganizational networks?

These are the two questions addressed in this article. We propose that to reduce the search costs and to alleviate the risk of opportunism associated with strategic alliances, organizations tend to create stable, preferential relationships characterized by trust and rich exchange of information with specific partners (Dore 1983; Powell 1990). Over time, these "embedded" relationships (Granovetter 1985) accumulate into a network that becomes a growing repository of information on the availability, competencies, and reliability of prospective partners (Kogut, Shan, and Walker 1992; Gulati 1995b; Powell, Kogut, and Smith-Doerr 1996). The more the emerging network internalizes information about potential partners, the more organizations resort to that network for cues on their future alliance decisions, which are thus more likely to be embedded in the emerging network. These new embedded alliances, in turn, further increase the informational value of the network, enhancing its effect on subsequent alli-

ance formation. In this iterative process, new partnerships modify the previous alliance network, which then shapes the formation of future cooperative ties. Thus, we model the emergence of alliance networks as a dynamic process driven by exogenous interdependencies that prompt organizations to seek cooperation and by endogenous network embeddedness mechanisms that help them determine with whom to build partnerships. Interorganizational networks are thus the evolutionary products of embedded organizational action in which new alliances are increasingly embedded in the very same network that has shaped the organizational decisions to form those alliances.

We develop a model by specifying the mechanisms through which the existing alliance network enables organizations to decide with whom to build new alliances, and we discuss how the newly created ties can increase the informational content of the same alliance network, enhancing its potential to shape future partnerships. In theoretical terms, this is akin to specifying the mechanisms through which social structures shape organizational action and the mechanisms through which this action subsequently affects social structures (Wippler and Lindenberg 1987; Gargiulo 1998). We test this model using longitudinal data on interorganizational strategic alliances in a sample of American, European, and Japanese business organizations in three different industries over a 20-year period. The quantitative data collection and the empirical analysis for this study were preceded by extensive interviews with managers involved in alliance decisions at a variety of organizations. We conducted exploratory, open-ended field interviews with 153 managers actively involved in alliance decisions in 11 large multinational corporations. This fieldwork enabled us to ground our claims about the role of the existing alliance network as a source of information for organizational decision makers, as well as to identify some of the mechanisms through which they tap that information.

INTERORGANIZATIONAL STRATEGIC ALLIANCES

Strategic alliances are a vivid example of voluntary cooperation in which organizations combine resources to cope with the uncertainty created by environmental forces beyond their direct control. These alliances are organized through a variety of contractual arrangements, ranging from equity joint ventures to arm's-length contracts (Harrigan 1986; Gulati 1995a; Gulati and Singh 1999). Partly as a response to the growing uncertainty that characterizes the international business arena, the number of interorganizational alliances has grown at an unprecedented rate in the last 15 years, across a wide array of industries and both within and across geographical boundaries. Empirical evidence suggests that the number of interorganizational alliances prior to 1980 was very small, but there has

been a virtual explosion since that time (e.g., Hergert and Morris 1988). The rapid growth of such ties provides a unique context in which to study the emergence and the evolution of an interorganizational network from the early stages of its development to the period in which alliances became a more established form of cooperation among firms (Gulati 1998).

Despite their explosive growth, strategic alliances are associated with a variety of risks and pitfalls that result in considerable uncertainty about the decision to enter such ties. This is further compounded in the global setting, with disparate firms from a wide range of national origins, in which a good number of these alliances take place (Kogut 1988; Doz 1996). This uncertainty stems from two main sources. First, organizations have difficulty in obtaining information about the competencies and needs of potential partners. This knowledge is essential to assess the adequacy of a potential partnership if both organizations are to derive benefits from the alliance. An organization that knows about the competencies and needs of a potential partner is in a better position to assess whether the alliance can simultaneously serve its own needs and its partner's needs. Yet organizational needs and capabilities are multifaceted and ambiguous. Accurate information on needs and capabilities of other organizations may be difficult to obtain before an alliance is initiated. In most cases, it may require access to confidential information that would not be revealed outside an established partnership. Such a paucity of information is even more significant between firms from different geographic origins.

The second source of uncertainty that affects strategic alliances stems from the paucity of information about the reliability of the potential partners, whose behavior is a key factor in the success of an alliance (Gulati 1995*a*, 1995*b*). Such behavioral uncertainty is intrinsic to voluntary cooperation; indeed, it plays a central role in Coase's ([1937] 1952) theory of the firm and in the transaction-cost perspective (Williamson 1985). Organizations entering alliances face considerable moral hazard concerns because of the unpredictability of the behavior of partners and the likely costs to an organization from opportunistic behavior by a partner, if it occurs (Kogut 1988; Doz, Hamel, and Prahalad 1989). A partner organization may either free ride by limiting its contributions to an alliance or may simply behave opportunistically, taking advantage of the close relationship to use resources or information in ways that may damage the partner's interests. In addition, rapid and unpredictable changes in the environment may lead to changes in an organization's needs and its orientation toward ongoing partnerships (MacIntyre 1981).

The paucity of reliable information about the capabilities, the needs, and the behavior of potential partners creates a significant informational hurdle for organizations that consider entering strategic alliances. Yet the

explosive growth of strategic alliances suggests that organizations are able to overcome such hurdles and enter alliances. How do they do it? And what consequences does their behavior have for the social context in which new strategic alliances take place?

THE FORMATION OF STRATEGIC ALLIANCES

Interdependence

Interdependence is the most common explanation for the formation of interorganizational cooperative ties such as strategic alliances. A long stream of research suggests that organizations enter ties with other organizations in response to the challenges posed by the interdependencies that shape their common environment (e.g., Aiken and Hage 1968; Pfeffer and Salancik 1978; Aldrich 1979; Galaskiewicz 1982; Burt 1983). Broadly defined, environmental dependence encompasses two sets of considerations: resource procurement and uncertainty reduction (Galaskiewicz 1985). Organizations build cooperative ties to access capabilities and resources that are essential to pursue their goals but that are at least in part under the control of other organizations in their environment. Interorganizational cooperation is thus a means by which organizations manage their dependence on other organizations in their environment and attempt to mitigate the uncertainty generated by that dependence. Oliver (1990) reviewed the literature on such exogenous drivers of interorganizational relations and presented six broad categories of environmental contingencies that stimulate such ties. One of these types of contingencies—necessity—prompts ties mandated by legal or regulatory requirements, but the other categories—asymmetry, reciprocity, efficiency, stability, and legitimacy—lead to cooperative ties that organizations voluntarily initiate to address specific needs resulting from their external interdependence.

Strategic alliances are an important form of voluntary cooperative interorganizational ties. Organizations build alliances for a variety of reasons, including the need to share the costs and risks of technology development or large-scale projects, to develop existing markets or penetrate new ones, and to pursue resource specialization strategies (Mariti and Smiley 1983). Such objectives make organizations interdependent with other organizations that may have the capabilities and the resources to assist them in meeting their specific needs. Other things being equal, the higher the interdependence between two organizations, the higher their incentive to combine their resources and capabilities through an alliance. Building on the insights of this research tradition, we expect tie formation between organizations to be a function of the level of interdependence between them. Thus:

HYPOTHESIS 1.—*The probability of a new alliance between two organizations increases with the level of interdependence between those organizations.*

Interdependence may be a necessary condition for organizations to enter alliances. In most cases, however, interdependence may not be sufficient to account for the formation of an alliance between two specific firms. Indeed, not all opportunities for cooperation between interdependent organizations actually materialize in alliances. An organization confronted with the need to build an alliance to cope with an uncertain environment faces another type of uncertainty resulting from the identification of an appropriate alliance partner. Such uncertainty stems from the paucity of information about the true capabilities, the needs, and the behavior of potential alliance partners. While interdependence may help an organization to orient the search for an adequate alliance partner, it cannot offer sufficient cues to determine with whom it should build such an alliance. This has not posed major difficulty for studies conducted at aggregate levels of analysis, which typically predict the formation of alliances across industries as a function of the intensity of the transactions between those industries (Pfeffer and Nowak 1976a, 1976b; Berg and Friedman 1980; Duncan 1982; Burt 1983). Yet, this approach masks the considerable heterogeneity of available information on prospective partners across organizations, which may influence the formation of ties between specific organizations without necessarily affecting aggregate industry trends. Although resource-dependence perspectives recognize an "enactment" process that mediates between environmental demands and organizational action (Pfeffer and Salancik 1978, p. 71), most of this research implicitly assumes that decision makers have adequately identified the sources of environmental uncertainty as well as the partners that would help their organizations to reduce that uncertainty. While this assumption is tenable at aggregate levels of analysis, it is difficult to sustain when examining alliances between specific pairs of organizations.

Interorganizational Embeddedness

If interdependence alone cannot offer sufficient cues for organizations to cooperate with one another, how do they decide with whom to build strategic alliances? Building on a growing body of research (see Powell and Smith-Doerr [1994] and Gulati [1998] for a review) and on our own fieldwork, we argue that organizations address the potential hazards associated with building alliances by relying on information provided by existing interorganizational networks. We propose that organizational decision makers that play a crucial role in the formation of new strategic alliances rely on the network of past partnerships to guide their future

alliance decisions. The creation of new ties, in turn, contributes to the subsequent development of that same network, enhancing its capacity to shape subsequent alliance decisions.

Although rooted in classical sociological theory, the idea that economic action is embedded in social networks was revitalized by Granovetter (1985) in his manifesto for a new economic sociology. According to Granovetter (1985, p. 490), the microfoundations of embedded economic action rest on "the widespread preference for transacting with individuals of known reputation," for resorting to "trusted informants" who have dealt with a potential partner and found this partner trustworthy, or, even better, for relying on "information from one's own past dealings with that person." A similarly rich exchange of information occurs across organizational boundaries (Dore 1983; Eccles 1981; Powell 1990; Romo and Schwartz 1995). Personal relationships among key individuals have played a crucial role in producing trust between organizations in Japanese industrial groups (Lincoln, Gerlach, and Ahmadjian 1996) and in contractual relationships (Macaulay 1963; Bradach and Eccles 1989). Closer to our concerns, personal ties are important for the formation and success of strategic alliances (Ring and Van de Ven 1992; Doz 1996). Beneath the formalities of contractual agreements, multiple informal interpersonal relationships emerge across organizational boundaries, which facilitates the active exchange of information and the production of trust that foster interorganizational cooperation (Gulati 1995*a*; Walker, Kogut, and Shan 1997; Zaheer, McEvily, and Perrone 1998).

Most organizations are embedded in a variety of interorganizational networks, such as board interlocks, trade associations, and research and development ventures. Scholars have suggested that participation in such social networks can be influential in providing actors with access to timely information and referrals to other actors in the network (Burt 1992). At the interorganizational level, the network of prior alliances has been identified as one such network that is an important source of information and referrals for organizations (Kogut, Shan, and Walker 1992; Gulati 1995*b*). This insight was strongly confirmed by managers in our own fieldwork who highlighted the importance of the network of prior alliances as a source of trustworthy information about the availability, capabilities, and reliability of potential partners. In the words of one of the managers interviewed, "Our network of [prior alliance] partners is an active source of information for us about new deals [alliances]. We are in constant dialogue with many of our partners, and this allows us to find many new opportunities with them and also with other organizations out there."

The information that flows through the alliance network is not only trustworthy but is also timely. This, as another manager put it, is critical for entering strategic alliances: "In our business, timing is everything. And

so, even for alliances to happen, the confluence of circumstances have to be at the right time. We and our prospective partner must know about each other's needs and identify an opportunity for an alliance together in a timely manner. . . . Our partners from past alliances are one of our most important sources of timely information about alliance opportunities out there, both with them and with other firms with whom they are acquainted."

Existing network research and insights from our own fieldwork suggest that timely, relevant information on the competencies, needs, and reliability of potential partners originates from organizations' previous direct alliances, from their indirect alliance ties through third parties, or from the reputation that results from the potential partner's position in the network of preexisting alliances. Each of these sources of information is related to specific network mechanisms that shape the creation of new embedded interorganizational ties. We refer to these mechanisms as relational, structural, and positional embeddedness, respectively.

Relational embeddedness highlights the effects of cohesive ties between social actors on subsequent cooperation between those actors. Cohesive ties play a prominent explanatory role in classical sociological analysis of social solidarity and cooperation (e.g., Durkheim [1893] 1933, [1897] 1951). Prior cohesive ties between two organizations provide channels through which each partner can learn about the competencies and the reliability of the other. Cohesiveness amplifies trust and diminishes the uncertainty associated with future partnerships (Podolny 1994; Burt and Knez 1995; Gulati 1995a). Cohesive ties may also prompt organizations to become aware of new opportunities for cooperation that would be difficult to identify outside of a close relationship. This facet of cohesive relationships was emphasized by one of the strategic-alliance managers we interviewed: "They [our partners] are familiar with many of our projects from their very inception, and if there is potential for an alliance we discuss it. Likewise, we learn about many of their product goals very early on, and we actively explore alliance opportunities with them." Thus, a history of cooperation can become a unique source of information about the partner's capabilities and reliability and increases the probability of the two organizations forming new alliances with each other. As a consequence:

HYPOTHESIS 2.—*The probability of a new alliance between two organizations increases with the number of prior direct alliances between those organizations.*

Structural embeddedness captures the impact of the structure of relations around actors on their tendency to cooperate with one another (Granovetter 1992). The frame of reference shifts from the dyad to the triad, while the focus of analysis shifts from direct communication between

actors to indirect channels for information and reputation effects.² Organizations tied to a common partner can utilize reliable information about each other from that partner (Baker 1990; Gulati 1995b). When two organizations share common ties, it can also indicate that both are regarded as suitable and trustworthy by the same organizations. Also, sharing common ties with a potential partner may signal that the partner can cooperate with the same kind of organizations with which the focal organization has been cooperating. Finally, common third-party ties can create a reputational lock-in whereby good behavior is ensured through a concern for local reputation. Any bad behavior by either partner may be reported to common partners, which serves as an effective deterrent for both (Raub and Weessie 1990; Burt and Knez 1995).

Referrals, and their associated reputation effects, were explicitly mentioned in several of our field interviews as an important mechanism through which their organizations learned about reliable partners. In the words of one of the managers, "In some cases we realize that perhaps our skills do not really match for a project, and our partner may refer us to another organization about whom we were unaware. . . . An important aspect of this referral business is of course about vouching for the reliability of that organization. Thus, if one of our long-standing partners suggests one of their own partners as a good fit for our needs, we usually consider it very seriously." Thus:

² Although there is no explicit mention by Granovetter (1992), the notion of structural embeddedness is related to network models of structural equivalence, according to which two actors equally tied to the same third parties are "structurally equivalent" (Lorrain and White 1971; White, Boorman, and Breiger 1976; Burt 1976). There has been considerable debate about whether structural equivalence operates through the indirect effect of cohesive ties to common third parties (Alba and Kadushin 1976; Alba and Moore 1983; Friedkin 1984) or through "symbolic role playing" and competition between equivalent actors (Burt 1982, 1987). The essence of the debate is whether the mechanisms behind the impact of indirect ties on behavior are substantially different from those behind the effect of direct ties. Yet, as Borgatti and Everett (1994, p. 28–29) have suggested, the notion of proximity is an inseparable part of structural equivalence. In a similar vein, Mizruchi (1993, p. 280) suggested that deciding whether the effects of structural equivalence on behavior operate through the similar socializing pressures of common third parties or through symbolic role playing is practically impossible without knowing the motives that underlie the actors' behavior. It is worth noting, however, that a mechanism that stresses competition between structurally equivalent actors (e.g., Burt 1987) would predict a smaller probability of cooperation between actors tied to the same third parties, whereas a mechanism that stresses the increased trust and information between those with common third-party ties predicts a greater probability of cooperation between the actors (e.g., Coleman 1990). Our focus on the increased trust and information effects of third-party ties is congruent with our prediction that shared partners increase the probability of cooperation between organizations.

HYPOTHESIS 3.—*The probability of a new alliance between two organizations increases with the number of prior indirect alliances between those organizations.*

Positional embeddedness captures the impact of the positions organizations occupy in the overall structure of the alliance network on their decisions about new cooperative ties. Positional embeddedness is rooted in network models of equivalence and centrality that capture the “roles” actors occupy in a system, irrespective of the specific alters involved in playing those roles (Winship and Mandel 1983; Faust 1988; Borgatti and Everett 1994). As a mechanism that influences alliance formation, positional embeddedness goes beyond proximate direct and indirect ties and highlights the informational benefits that ensue for organizations from particular positions in the network. The position an organization occupies in the emerging network can influence its ability to access fine-grained information about potential partners as well as its visibility and attractiveness for other organizations throughout the network, even if it is not directly or indirectly tied to them. The information advantages resulting from network centrality have been a recurrent theme in network analysis (see Freeman [1979] for a review). Social cognition studies also suggest that central actors have a more accurate representation of the existing network (Krackhardt 1990). Central organizations have a larger “intelligence web” through which they can learn about collaborative opportunities, hence lowering their level of uncertainty about partnerships (Gulati 1999; Powell et al. 1996). Therefore, the more central an organization’s network position, the more likely it is to have better information about a larger pool of potential partners in the network.

The information advantages from centrality in networks are complemented by the higher visibility of central organizations, which enhances their attractiveness to potential partners. Because network centrality is a direct function of organizations’ involvement in strategic alliances, it can also be a signal of their willingness, experience, and ability to enter such partnerships. The signaling property of network positions is particularly important in uncertain environments, because it introduces systemic reputational differences among organizations that extend beyond their immediate circle of direct and indirect ties (Podolny 1993; Han 1994; Podolny and Stuart 1995).³ The information benefits that result from occupying a

³ Network position often has been associated with the more traditional sociological concepts of “role” and “status” (Lorrain and White 1971; Burt 1982; Faust 1988). The notion of “role” typically evokes a relatively defined set of expected behaviors toward types of other actors, whereas “status” refers to a series of observable characteristics associated with a particular role (Linton 1936; Merton 1957; Nadel 1957). Network theory suggests that because an actor’s (organization’s) role and status are ultimately

prominent network position were recognized by a manager we interviewed who reflected on the attractiveness of his firm as alliance partner: "Through our vastly successful technology partnerships program, we have built ourselves a reputation in the industry for being an effective and reliable alliance partner. Today, we are pursued by other firms to enter alliances much more frequently than we pursue potential partners." If central firms have greater access to information and higher visibility than other organizations, then, other things being equal, interorganizational ties should be more common between organizations that occupy central positions in the emerging interorganizational network. Thus:

HYPOTHESIS 4.—*The probability of a new alliance between two organizations increases with the combined alliance-network centrality of those organizations.*

Organizations may seek to enhance their own visibility and attractiveness as potential partners by forming new ties with central players in the alliance network. Since the network position of an organization's partners enhances its own access to information and attractiveness to future partners, it will have a tendency to seek central partners. Central organizations, however, may not have an incentive to accept peripheral players, since they may add little to (or, worse, may damage) their own attractiveness. Furthermore, if network position is a signal of unobserved attributes that determine an organization's attractiveness as a potential alliance partner, peripheral organizations may be perceived by others to have little to offer substantively. This does not preclude the possibility that peripheral organizations may at times enter alliances with central firms. Special circumstances such as those resulting from the need to master a new technology may prompt a central organization to cooperate with a peripheral one that controls such a technology, but we expect that the probability of cooperation will increase with the similarity in alliance-network centrality between the potential partners. Therefore:

HYPOTHESIS 5.—*The probability of a new alliance between two organizations increases with the similarity in alliance-network centrality between those organizations.*

The prediction of this hypothesis corresponds to the tendency toward "structural homophily" that exist under conditions of uncertainty (Podolny 1994; Popielarz and McPherson 1995).

THE ENDOGENOUS DYNAMIC OF ALLIANCE NETWORKS

In building new alliances, organizations also contribute to the formation of the network structure that shapes future partnerships. Observed over

based on its affiliations and patterns of interaction, they can, and should be, gauged from the position the actor occupies in the networks defining the social system.

time, this dynamic between embedded organizational action and the network structure that results from that action propels the progressive *structural differentiation* of the interorganizational network. We define structural differentiation as an emergent systemic property that captures the extent to which actors (organizations) come to occupy an identifiable set of network positions, each of them characterized by a distinctive relational profile. Because the position an organization occupies in an alliance network is a signal of its willingness, experience, and ability to enter partnerships, the higher the structural differentiation of an emerging network, the easier it is for organizations to distinguish among other organizations in terms of their relational profiles and the more the network structure acts as a repository of valuable information on potential alliance partners.

This discussion suggests a linear relationship between the level of structural differentiation of the emerging alliance network and the extent of information contained in that network. There is, however, an important caveat. While a network in which all or most organizations have a similar relational profile would offer little guidance to a decision maker, the opposite extreme, a network in which each organization has a truly unique relational profile, may be equally uninformative. This is particularly significant for the information that originates in the position an organization occupies in the structure of the alliance network. If every organization were to occupy a unique structural position, it would be impossible to infer the behavior of any particular organization from the expected behavior of other organizations that occupy that position in the system. The underlying social structure would offer little guidance to organizations seeking an alliance partner, since every potential partner would be unique from a network standpoint. As a result, the relationship between the structural differentiation of a network and the information available to the actors in that network may level off and eventually even become negative as the accumulation of new ties further increases the level of structural differentiation in the network beyond a critical level. Studies of mature social structures suggest that the structural differentiation of most real systems may not display a continuous increase over time. Instead, mature structures typically display a set of stable, self-reproducing positions occupied by actors with similar network profiles (White 1981; Burt 1988). In such structures, the level of structural differentiation remains practically constant over time. Barring exogenous shocks, the structural differentiation of alliance networks may similarly taper off as the social structure of the interorganizational network reaches a mature state.

The effects of structural differentiation are conceptually distinct from the legitimating effects typically associated with growing network density (Hannan and Freeman 1989; Scott 1995). Although structural differentiation is likely to grow with the number of ties in the network, it is distinct

insofar as it depends on the specific distribution of those new ties, not merely on their number. The density of ties in a network may provide organizations with information about the pervasiveness of a new form of cooperation, thus helping them to address concerns on the legitimacy of this course of action, but it offers no guidance as to which specific organizations could be worthy partners. Thus, while network density affects the availability of information in a system (Blau 1977), it does not shed light on potential differences in effective access to that information, nor on how the pattern of ties may themselves provide information.

We expect that the structural differentiation of the emerging alliance network will influence new alliance formation both directly and through its interaction with some of the mechanisms that drive alliance formation. At the system level, the additional information introduced by the progressive structural differentiation of an emerging network lowers the level of systemic uncertainty faced by organizations, which directly affects the propensity of organizations to enter new ties. Thus:

HYPOTHESIS 6.—*The probability of a new alliance between any two organizations increases with the level of structural differentiation in the interorganizational network.*

While structural differentiation focuses on system-level information, exogenous resource concerns and network embeddedness focus on the more proximate level of organizations. Given the shared focus of these factors on information availability, we expect that an increase in the extent of structural differentiation is likely to moderate the relative influence of interdependence and embeddedness factors on the creation of new ties. In early periods, when a network is relatively undifferentiated and thus likely to contain limited information about potential partners, organizations may still be prompted to cooperate by exogenous pressures that influence their interests. As a consequence, exogenous factors are likely to be the primary driver of tie formation in the early stages of a network, but the growing differentiation of a network enables it to channel increasing amounts of information about potential partners. As structural differentiation increases, exogenous factors are likely to have a diminishing influence on the formation of new ties. Thus, we expect the structural differentiation of the network to have a negative moderating effect on the influence of exogenous factors on tie formation:

HYPOTHESIS 7.—*The effect of interdependence on the formation of new alliances between organizations decreases with the level of structural differentiation of the interorganizational network.*

We also expect the structural differentiation of the network to moderate the influence of embeddedness on tie formation, although not all embeddedness mechanisms are likely to be moderated by the growing differentiation of the network. The information organizations can obtain through

previous direct dealings with other organizations (relational embeddedness) or from common third-party alliances (structural embeddedness) is readily available to a decision maker, and thus it is not necessarily dependent on the larger network in which these dyadic or triadic relations exist. Access to such information depends on the ability of proximate ties to act as conduits of fine-grained information about the competencies and cooperative behavior of other organizations, a property that is not contingent on the stage of development of the entire network. Therefore, the impact of relational- and structural-embeddedness mechanisms is not necessarily contingent on the level of structural differentiation in the overall network.⁴

While the information that results from prior ties to a prospective partner or from common third parties is immediately available to organizations, this is not the case with the information contained in the position their potential partners occupy in the emerging alliance network. The effectiveness of an organization's network position as a signal of unobservable qualities of this organization depends on the development of the overall network in which the varying involvement of organizations in partnerships becomes apparent. The relative scarcity of ties at early stages of the network makes these differences far from apparent. The increase in structural differentiation corresponds to an increase in differences in alliance involvement across organizations, which alters their relative visibility in the overall network. Thus, the informational value of the position of organizations in a social network is likely to be contingent on the level of structural differentiation of that network. We therefore expect the effect of organizations' positional embeddedness on tie formation to increase with the level of structural differentiation of the network:

HYPOTHESIS 8.—*The effect of positional embeddedness on the formation of new alliances between organizations increases with the level of structural differentiation of the interorganizational network.*

Figure 1 summarizes our dynamic model of network formation and highlights the empirically testable predictions of the model. The solid

⁴ Our reluctance to suggest that the effects of relational or structural embeddedness are contingent on structural differentiation does not rule out alternative mechanisms through which the growth of the network may alter the effect of these factors and perhaps lead to an empirically observable relationship. The sheer growth in network density could enhance the legitimacy of partnerships, thus making organizations more eager to build ties. Insofar as we expect organizations to prefer embedded ties, the likelihood of entering new ties with previous partners or with common third parties may increase with the growing density of the network. Since density is a likely correlate of structural differentiation, one may still observe a growing impact of relational embeddedness as differentiation increases, but this effect is likely to be spurious from the standpoint of our model.

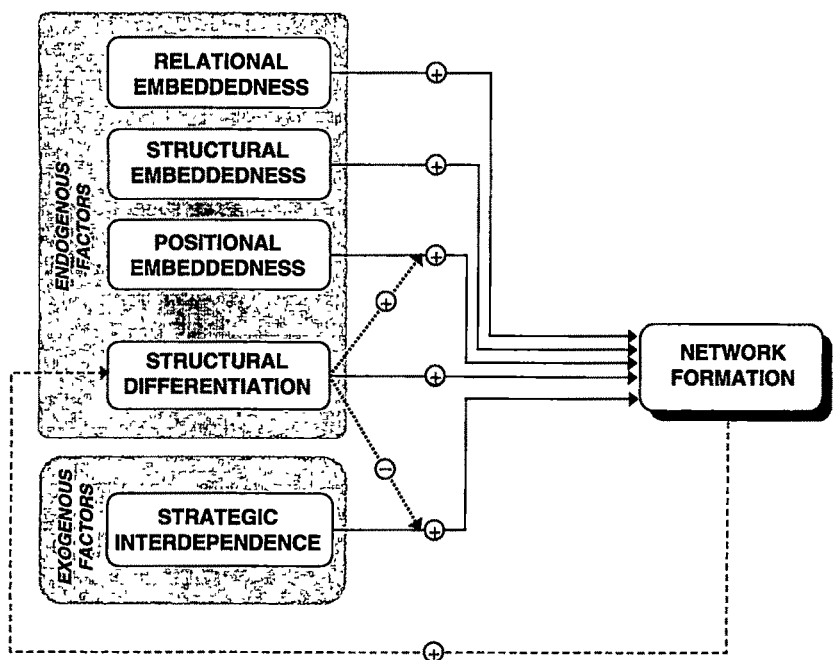


FIG. 1.—The endogenous dynamic of interorganizational networks

arrows represent the direct effects of the key variables on network formation (strategic interdependence, relational, structural, and positional embeddedness, and structural differentiation). The dotted arrows from structural differentiation to the arrows for the direct effects of interdependence and positional embeddedness capture the moderation effect of structural differentiation on the impact of those mechanisms on tie formation. The plus and minus signs indicate a strengthening or weakening of influence in the direction of the arrows. Our expectation is that the greater the structural differentiation of the emerging network, the stronger the effects of positional embeddedness and the weaker the effect of strategic interdependence. The dashed arrow from network formation to structural differentiation indicates the dynamic connection between action and structure.

METHODS

Sample

We tested our model using longitudinal data on strategic alliances in a sample of American, European, and Japanese organizations in three in-

dustries over a 20-year period. We collected data on a sample of 166 organizations in new materials, industrial automation, and automotive products. We selected a panel of 50–60 of the largest publicly traded organizations within each sector, estimating an organization's size from its sales in that sector as reported in various industry sources. We also checked with multiple industry experts to ensure that our panels included all prominent competitors in the sectors. This design led to the inclusion of 62 organizations in new materials, 52 in automotive products, and 52 in industrial automation. Of these organizations, 54 were American, 66 were Japanese, and 46 were European.

For each organization, we collected financial data for each year between 1980 and 1989 from *Worldscope*, which provides detailed information about prominent organizations in a wide range of sectors. For organizations not reported in *Worldscope*, data were obtained from *COMPUSTAT* for U.S. organizations, *Nikkei* for Japanese organizations, and *Disclosure* for European organizations. For a number of Japanese organizations, data were also obtained from *Daewoo Investor's Research Guide*.⁵ We also collected information for each organization from numerous industry-specific trade journals about the subsegment of its industry within which it had expertise. To make sure that these classifications were correctly recorded, we cross-checked these with multiple experts from each of the industries.

Information on the alliances formed in the three panels of organizations was derived from a much larger and more comprehensive data set that includes information on over 2,400 alliances formed by American, European, and Japanese organizations in the three focal sectors for 1970–89. More than half the data came from the Cooperative Agreements and Technology Indicators (CATI) database collected by researchers at the University of Maastricht. We collected additional alliance data from numerous other sources, including industry reports and industry-specific ar-

⁵ For a few organizations, financial data were available for only some years. The gaps typically resulted from the fact that *Worldscope* reports organization data in five-year continuous segments and omits some organizations from some volumes. One alternative for dealing with this problem would have been to use the "available-case method," including only cases with the variables of interest in the analysis. Although such an approach is straightforward, it poses a number of problems, including variability in the sample base as the variables included in models change. Furthermore, it makes little sense to exclude entire cases simply because a single variable is missing. We thus chose to estimate the missing data using a time-trend-based imputation (Little and Rubin 1987). This procedure took into account the fact that the financial outcome for an organization is the result of its own past actions as well as broad trends within its industry. We retained a dummy variable indicating imputation and later compared the results obtained with and without imputed values.

ticles reporting alliances. For the automotive industry, these included *Automotive News*, *Ward's Automotive Reports*, *U.S. Auto Industry Report*, *Motor Industry of Japan*, and the *Japanese Auto Manufacturers Forum*; for the industrial automation sector, *Managing Automation* (1988–89); for the new materials sector, reports from the Office of Technology Assessment and the Organization for Economic Cooperation and Development were used; and for all sectors, we used Predicast's *Funk and Scott Index of Corporate Change*. In all instances, we recorded only alliances that had actually been formed and excluded reports of probable alliances. To our knowledge, these are the most comprehensive data on alliances within each focal sector in both depth and duration of coverage.

The Structure of Interorganizational Alliance Networks

We analyzed the networks in each industry in our sample to explore the structure of the alliance networks and visually examined the emergence of structurally differentiated positions to assess the structural patterns that would clarify and illustrate the differentiation process depicted in the theoretical model. We examined the network structure resulting from the cumulative alliance activity of the organizations within each industry by conducting separate analyses of each industry network in the penultimate period of the study (1988). Each of the three networks included all interorganizational alliances announced in that industry in the previous five years (1983–88). The strength of the ties between two organizations in the network corresponded to the strongest alliance between these organizations in the period, where strength is measured on a seven-point Guttman scale (Contractor and Lorange 1988; Nohria and Garcia-Pont 1991). We used the concept of "role equivalence" to identify classes of organizations or "positions" in the network and their relationship. Role equivalence captures similarities in the organizations' pattern of involvement in alliances, even when this involvement may be with different partners. Each role-equivalent position refers to sets of actors involved in similar types of relations but not necessarily with the same "alters." While structural-equivalence models focus on relations with specific actors (Lorrain and White 1971), role-equivalence models focus on the pattern of relationships among actors and are more adequate to capture status/role sets in a network (Winship and Mandel 1983; Faust 1988; Borgatti and Everett 1994). Two actors are structurally equivalent if they have similar relationships with similar alters, while two actors are role equivalent when they are involved in similar types of relationships with others actors. For instance, two managers leading separate divisions are not structurally equivalent because they have different subordinates; however, they are role equivalent.

lent, since they have a similar type of relationship with these subordinates.⁶

To identify role-equivalent positions in the interorganizational networks, we used an approach developed by Hummel and Sodeur (1987) and by Burt (1990). Building on the triad-census idea introduced by Holland and Leinhardt (1970), this technique identifies role equivalence in terms of similarity in the actor's triad patterns. The larger the extent to which two actors are involved in similar triads, the more they are role equivalent (see Van Rossem [1996] for an example of this application).⁷ Role equivalence is measured as the euclidean distance between the vectors that capture the triad pattern of each actor. Two organizations that had identical triad patterns would be separated by zero euclidean distances and would be perfectly role equivalent, regardless of the specific organizations with which they built their alliances.

We computed role-equivalence measures using the general-purpose network analysis software Structure 4.2 (Burt 1991).⁸ Role-equivalent blocks were identified by cluster analysis of the matrix of euclidean distances between the organizations' triad patterns. Finally, we also performed metric multidimensional-scaling analyses of the proximity matrices of the industry networks, in which proximity was defined by the strength of the alliance between organizations.

Figure 2 presents density tables based on role-equivalent partitions of the three industry networks, along with spatial maps of each industry

⁶ Several network analytical concepts—and algorithms—have been proposed to capture this abstract form of equivalence, including automorphic, regular, positional, and role equivalence. For a comprehensive reviews of these concepts, see Mizruchi (1993), Wasserman and Faust (1994), and Borgatti and Everett (1994).

⁷ In a symmetric network, a focal organization, or "ego," can be involved in six different triads with two other organizations, or "alters." These triads are T1, in which all three parties are disconnected—also known as the "null triad"; T2, in which ego is connected to only one of two disconnected alters; T3, in which ego is connected to two disconnected alters; T4, in which ego faces two connected alters but has no connection to either; T5, in which ego is connected to one of two connected alters; and T6, in which all three actors are connected. T1 and T4 define an isolated role, while T3 is typical of central roles. The isolated triad (T1), however, has a disproportionately high frequency in sparse networks such as the ones analyzed here. To eliminate potential biases that stem from this dominance, we excluded the null triad from the census (Van Rossem 1996). The isolated role is thus purely defined by T2.

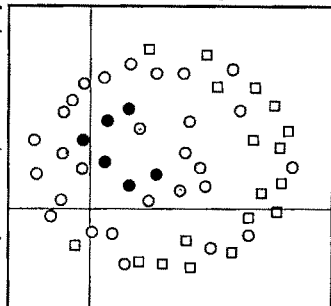
⁸ An alternative approach would have been to use a regular equivalence algorithm, such as the one included in UCINET IV. The algorithm, however, has posed computational and interpretative difficulties when applied to symmetric networks (Doreian 1987, 1988; Borgatti 1988). The triad-census approach is computationally simpler and has an intuitive appeal; it is similar to the original Winship and Mandel's (1983) model if role equivalence is defined by direct and two-step ties only (Burt 1990).

Automotive

Position	1	2	3	4	Centr
● 1	2.800	2.106	.873	.000	.918
⊙ 2	2.106	1.545	.064	.000	.652
○ 3	.873	.064	.118	.000	.155
□ 4	.000	.000	.000	.000	.000
Mean	1.020	.572	.153	.000	
N	6	11	17	18	
Rel	99.29	90.92	95.99	100.00	

● Center ○ Periphery
 ⊙ Semi-periphery □ Isolates

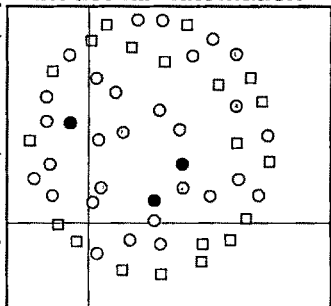
Automotive



Industrial Automation

Position	1	2	3	4	Centr
● 1	2.000	.933	.571	.000	.591
⊙ 2	.933	.448	.157	.000	.230
○ 3	.571	.157	.000	.000	.067
□ 4	.000	.000	.000	.000	.000
Mean	.510	.221	.080	.000	
N	3	15	14	20	
Rel	99.97	89.79	99.90	100.0	

Industrial Automation



New Materials

Position	1	2	3	4	Centr
● 1	3.300	.866	.344	.000	.689
⊙ 2	.866	.418	.103	.000	.308
○ 3	.344	.103	.072	.000	.041
□ 4	.000	.000	.000	.000	.000
Mean	.521	.192	.072	.000	
N	5	14	18	25	
Rel	98.06	86.36	99.89	100.0	

New Materials

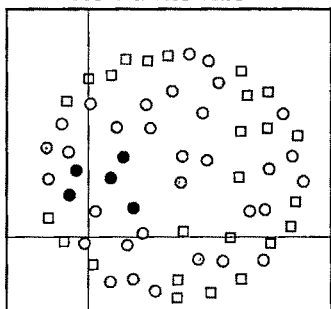


FIG. 2.—Metric MDS maps and density tables of the interorganizational network structures, 1989. Proximities in MDS maps are defined by the strength of the alliances; positions are occupied by role-equivalent organizations. Density is the mean strength of alliances between occupants of the respective position(s), measured on a seven-point Guttman scale. Mean density is the value for the whole industry network and equals .289 for automotive, .115 for industrial automation, and .106 for new materials. Centr. equals the mean eigenvector centrality within the position. Rel. equals the percentage of variance in euclidean distances explained by the first principal component of the position submatrices.

based on the first two dimensions of a metric multidimensional-scaling analysis of the observed alliance networks. For all three industries, the analysis clearly revealed four distinctive positions of role-equivalent organizations, one of which is occupied by "isolates"—organizations that did not enter any alliance in the five years prior to 1989. Figures in the main diagonal cells represent the average strength of an alliance between any two organizations occupying the same structural position; figures in off-diagonal cells represent the average strength of alliances between organizations in the respective two positions. Since we measured alliance strength on a seven-point Guttman scale, figures can vary from zero—when there is no alliance between any two organizations occupying the respective positions—to seven—if all organizations in the position(s) were tied to one another through the strongest alliances. Because alliances are symmetric ties, density tables are symmetric along the main diagonal. The tables also report average eigenvector centrality scores for the organizations in each position.⁹ We tested the homogeneity of the resulting positions with a principal component analysis of the submatrices of euclidean distances between position occupants. In all but one of the cases, the first principal component explained 90% or more of the observed variance in distances within the position, which suggested that the positions were occupied by organizations that were strongly role equivalent.

An inspection of the three density tables and spatial maps revealed a similar structural pattern across the three industries. First, the same number of positions adequately described all three interorganizational structures. The homogeneity of these positions was also consistent across industries. This suggested that all three industries experienced a similar pattern of structural differentiation during our observation period and limits potential concerns associated with industry differences.

Second, the figures in the cells revealed a uniform core-periphery pattern. In all three industries, organizations in the central position or "core" (position 1, represented by the black dots in the spatial maps) built strong alliances with one another and somewhat weaker, but still significant, alliances with organizations in the "semiperiphery" (position 2, gray dots) and the "periphery" (position 3, empty dots). In addition, all structures contain a considerable number of "isolates" (position 4, empty squares). The average alliance between the core and semiperiphery was stronger than that between the core and the periphery. The mean strength of intra-position alliances for organizations in the semiperiphery and periphery—displayed in the respective cell of the main diagonal—was always smaller

⁹ Centrality scores are the average eigenvector centrality (Bonacich 1987), normalized by industry to vary between 0 and 1. Central organizations are involved in alliances with partners who are in turn involved in many alliances.

than the mean strength of alliances between those organizations and the ones in the core—displayed in the first row (or column) of the tables. A similar pattern existed for the alliances between the semiperiphery and the periphery: except for the automotive industry, these alliances were stronger than the alliances between organizations in the periphery. Thus, periphery and semiperiphery organizations were more likely to build ties with organizations in the center than with organizations occupying their same positions. Core organizations, in turn, were more likely to build alliances with other core organizations than with either semiperiphery or periphery organizations. This pattern is typical of core-periphery structures (Van Rossem 1996).

Third, there was no evidence of isolated factions within the alliance networks. An inspection of the density tables and the accompanying spatial maps showed no indication of dense clusters of cohesive organizations with little interaction outside the cluster. Further, structural analysis using various clique detection routines also failed to identify isolated cohesive subgroups. Although all three networks displayed a relatively cohesive core of varying size, its occupants were also heavily involved in building alliances with organizations in the semiperiphery and—to a lesser extent—with those in the periphery.

The emergence of a core-periphery structure, such as the one revealed by our positional analysis, is consistent with the mechanisms in our endogenous embeddedness model. Organizations may originally differ in their propensity to build ties because of variability in exogenous pressures or organization-specific characteristics, yet the logic of relational and structural embeddedness amplify even a small initial variance in alliance activity, eventually creating appreciable differences among organizations. Relational embeddedness suggests that the more active organizations should have better information on potential partners, since they have access to a larger number of previously trusted partners. Structural embeddedness expands this pool to the partners' partners, which are likely to increase exponentially with the number of direct ties. Since having information on a larger pool of comparatively trustworthy potential partners increases the probability of entering new ties, the more active organizations have increasing comparative informational advantages over less active ones, which increases their likelihood of entering new partnerships (Gulati 1999). The differential involvement in partnerships eventually affects the visibility of the most active organizations beyond the circle defined by their direct and indirect ties, prompting further observable differences among organizations in the emerging network. These differences create conditions that can boost the influence of positional embeddedness on new alliance formation, because they make it easier for organizations to recognize central players in the emerging alliance network.

Variables

Alliance formation.—Our dependent variable is the formation of a new alliance between two organizations in a given observation year. Since the unit of analysis was the dyad, for each panel we listed all possible dyads within each sector, discarding reverse-ordered dyads to avoid double counting.¹⁰ These data were then used to construct an event history for each dyad, with a record for each dyad for each year studied (1981–89). For each dyad-year record, we coded a dichotomous dependent variable that indicated whether the pair of organizations entered an alliance in the given year. The resulting data structure is best characterized as a cross-sectional time-series panel in which the units are unique dyads. Each record included the state of the dependent variable, indicating the formation of an alliance in that period, along with time-varying and time-constant covariates characterizing the dyad.

Such a broad definition of the risk set, which included all possible dyads for the sample of firms in each industry, was considered essential to uncovering unbiased results. Including many dyads that never enter an alliance can, of course, lead to its own set of biases, but we had no observable criteria to determine a priori which dyads were likely to enter alliances and which were not. To address this issue and test the robustness of the results, we conducted the analysis with two additional risk sets that were more restrictive. The first set included only dyads in which at least one member had already entered one or more alliances. The second and most restrictive definition of the risk set included dyads in which both members had entered at least one other alliance. The results obtained with different sets were comparable. The results reported here are based on the complete risk set.

Interdependence.—This variable measures the extent to which organizations may need each other to access critical resources and capabilities. Prior research (Nohria and Garcia-Pont 1991; Shan and Hamilton 1991) and our own extensive fieldwork suggest that interdependence between organizations in these industries resulted primarily from the quest for complementary capabilities and resources, but identifying and measuring this complementarity is not an easy task. Complementarity between two organizations can arise when (a) there is a gap between the specific capabilities controlled by each organization and those they need to pursue their strategy and (b) this gap can be filled at least partially by accessing the

¹⁰ Hallinan and Sorensen (1985) used a similar dyadic approach in examining the effects of ability groups in classrooms on the patterns of student friendships formed. Fernandez (1991) examined the effects of informal and formal ties on leadership relations within organizations using such an approach. Both studies, however, used cross-sectional data.

capabilities controlled by the other organization while being able to offer something of value in return. Organizational needs and capabilities, however, are multifaceted and ambiguous; assessing them across a large number of organizations poses a formidable measurement problem. In addition, an index of complementarity for all possible pairs of organizations requires measuring the extent to which the capabilities of one organization can “complement” the capabilities of every other organization in the industry. We therefore used several approaches to assess complementary capabilities and resources that could create interdependence between organizations and conducted a statistical analysis to capture any unobserved affinity between organizations not adequately accounted for with the variables included in the analyses.

The measure of interdependence reported in this article is based on two key dimensions that drive complementarity between firms in our global setting: national origin and industry subsegment. National origin captures the geographical clustering of capabilities in the global economy. Regional contexts circumscribe important sets of unique organizational capabilities and resources, which resulted from specific historical and institutional processes (Porter 1990; Hamilton and Biggart 1988). In addition, interdependence across different geographical regions can result from the need to gain access to markets in those regions. Organizations from different regions are therefore more likely to have complementary capabilities and to benefit from strategic alliances with each other (Shan and Hamilton 1991). We captured these regional differences by grouping organizations in three categories—American, Japanese, and European—which correspond to the three major global markets, as well as to three relatively distinct historical and institutional settings.

Industry subsegment captures complementarity across different technological “niches” within an industry. Each of these niches corresponds to clusters of firms that share specific sets of capabilities and resources. Firms in different niches are more likely to have complementary capabilities that can make them interdependent and lead to alliances between them (Nohria and Garcia-Pont 1991). Building on this insight, we identified broad subsegments that define distinctive clusters of organizations within each industry. We identified two subsegments in the new materials sector (ceramics and polymer composites), four in industrial automation (discrete automation, process automation, software, and robotics), and two in the automotive sector (automobile assemblers and suppliers).

We measure interdependence between any two organizations within an industry as the normalized euclidean distance between those organizations, computed from the matrix that captures the national origin and industry subsegment of each firm and computed to capture the absence of overlap of activities. The greater the normalized euclidean distance

between two organizations, the more likely they are to possess complementary capabilities and resources, and the higher their interdependence. To check the validity of this measure, we performed a cluster analysis of the euclidean-distance matrix for each industry and identified clusters of organizations with similar national and technological profile. We identified seven distinct clusters in the new materials sector and nine clusters each in the industrial automation and automotive sectors.¹¹ The composition of those clusters was then checked with recent studies of similar industries (Nohria and Garcia-Pont 1991), as well as with a classification of the same firms in discrete strategic clusters by a panel of industry experts. The high convergence (80%) between the groups formed by the experts and those obtained from clustering the matrix of euclidean distances between the firms validates the use of these distances as a measure of interdependence. To further assess the similarity between the groups obtained through cluster analysis and the continuous measure used in this study, we constructed a dummy variable coded "1" if the organizations in a dyad belonged to different clusters and "0" if they belonged to the same cluster. The results obtained using this variable were similar to the ones based on the continuous measure of interdependence.

We also tried to account for interdependence between organizations in our sample by considering a series of firm-specific attributes—such as size and financial performance—that capture resource availability and constraints that typically influence the propensity of organizations to enter alliances. We discuss these attributes below as control variables. Finally, we tried to capture any residual interdependence with a statistical model that controls for unobserved factors that might affect the likelihood of alliances between specific firms. Details of this follow in the next section.

Network measures.—To compute our network measures, we constructed adjacency matrices representing the relationships between the organizations in each industry for each year. We included all alliance activity among industry panel members for the previous five years. One concern with such a design is left-censoring, which is an issue because many of the sample organizations existed prior to the start of the alliance observation period in 1981. Additional alliance data were collected for the alliance activity of this sample of organizations for 11 years, dating back to 1970 to minimize left-censorship effects. These data confirm that alliance

¹¹ Our partitioning of industries created clusters of organizations that are akin to the concept of "strategic groups" (for reviews, see Thomas and Venkatraman 1988; Barney and Hoskisson 1990; Reger and Huff 1993). Our clusters are closest to Porter's (1979) original definition of a strategic group as a set of organizations within an industry that are similar to one another in one or more strategic dimensions, such as skills, resources, goals, and historical development.

activity was negligible until 1980, when there was an explosion of alliances (Hergert and Morris 1988).

We made a number of choices in constructing these matrices about the treatment of different types of alliances, the accumulation of multiple ties by the same partners, and the past alliances that should be included. These choices were all tested against alternatives to ensure the robustness of our findings. First, alliances were weighted by their strength, as represented by their formal governance structure, using a seven-point scale (Contractor and Lorange 1988; Nohria and Garcia-Pont 1991), and the results were checked against a simple dichotomous measure. Second, to take into account the cumulative history of alliances between organizations, we used a Guttman scale that captures the score of the strongest alliance formed by the two organizations, checking the results against simple additive scores and normalized additive scores. Third, we used a moving window of five years of prior alliances, based on research suggesting that the normal life span for most alliances is usually no more than five years (Kogut 1988). We checked the results against networks that included all previous alliances dating back to 1970 in the construction of the networks.

Relational embeddedness.—This construct indicates the extent to which a pair of organizations (dyad) had direct contact with each other in the past. For our longitudinal panel of pairs of organizations for 1981–89, we operationalized relational embeddedness as the number of alliances dyad members had entered with each other in the previous five years.

Structural embeddedness.—This construct indicates the extent to which a given pair of organizations shared common partners from past ties. For each dyad-year record, we computed the number of partners shared by the two organizations in a dyad as a result of their alliances in the previous five years. To differentiate structural embeddedness from relational embeddedness, we set common ties to zero if the members of a dyad sharing common ties had entered at least one previous direct alliance with one another (cf. Mizruchi 1992, p. 126).

Positional embeddedness.—This construct indicates the extent to which the organizations in a dyad occupy similar or different network positions. We first computed a measure for the position of each organization and then used those as inputs to compute dyadic values. We measured the position of an organization in the emerging network of alliances using the Bonacich (1987) eigenvector measure of network centrality, a choice that is consistent with prior efforts to capture the position or role of an organization in a relational network (Mizruchi 1993; Podolny 1994). Using this measure, the most central organizations are those linked to many organizations, which are in turn linked to several other organizations. We computed the eigenvector measure of the network centrality of each organization for each year and expressed the scores relative to the most central

organization in the network for that particular year ($C_{\max} = 1$). To capture the joint centrality of the dyad, we computed the geometric mean of the centrality scores for each member of the dyad and then normalized it by the industry maximum (Mizruchi 1993). The larger the score, the more the two organizations occupied a central role in the network. To capture the similarity in centrality in a dyad, we computed the ratio of the smaller to the larger centrality score of the two organizations. The closer this ratio was to 1.0, the more similar were the two organizations' positions in the network.

Structural differentiation.—Our indicator for this construct reflects the nature of the differentiation that characterizes the specific interorganizational systems under investigation. Our positional analysis revealed the emergence of a center-periphery structure in all the three industry networks. At the system level, the emergence of such a structure is parallel to an increase in network centralization. Thus, an indicator that captured the extent of network centralization could adequately represent the type of differentiation observed in our networks. For each observation year, we measured the structural differentiation of the network as the level of network centralization in that year. Following Wasserman and Faust (1994), we measured network centralization as the standard deviation of the eigenvector centrality scores of the organizations in the industry for that year. Because we normalized eigenvector centrality scores by the highest centrality in each industry and year, our measure captured the *relative* internal differentiation of the system for each industry and in each given observation year. The changes over time in the measure of structural differentiation captures the emergence of the center-periphery structure that characterizes the industry networks. The average standard deviation of centrality across the three industries displayed a linear monotonic increase over time, ranging from .15 in t_0 (1981) to .41 in t_8 (1989). These aggregate figures adequately represent the pattern observed in each of the three industries and further highlight the progressive centralization of the networks as alliances accumulate. We tested the hypothesized moderating influence of structural differentiation on interdependence and embeddedness with interaction terms that were constructed using the product-term approach (Jaccard, Turrissi, and Wan 1990).

Control Variables.—We included as controls a number of variables known or expected to affect the alliance activity of organizations. These are network density, time, sector, organization-level effects, and a set of financial measures capturing organizational differences in some key resources.

An alternative interpretation for the endogenous embeddedness dynamic proposed here is a density-dependence argument linking the number of previous alliances to the legitimacy of this new form of business relationship. Ecological density-dependence arguments claim that there is an initial positive impact of density on founding rates of organizations

via the effect of density on the legitimacy of the new organizational form (Hannan and Freeman 1989, p. 132). Applied to this context, it would suggest that the growth in alliances may be the result of a bandwagon effect (Venkatraman, Loh, and Koh 1994). Thus, one could argue that structural differentiation might simply be capturing the progressive legitimization of alliances as a valid form of interorganizational cooperation, rather than the informational effects proposed in our model. If this were the case, the growth of alliances would be driven by the effects of density-dependent legitimization rather than by the increase in the availability of information captured by our notion of structural differentiation.

To account for this alternative explanation, we included a measure called *alliance density*, defined as the cumulative number of alliances within the industry in the previous five years, divided by the total number of possible alliances in the system. If the effect of structural differentiation is only capturing density-driven legitimacy, the inclusion of alliance density should make the effect of structural differentiation insignificant, thus bringing into question the validity of our claims. The endogenous network dynamic model, however, does not preclude a legitimization effect, because density-dependence and structural-differentiation effects need not be mutually exclusive.

To control for unobserved temporal factors that may influence alliance formation, we included dummy variables for each year. Such factors could include a progressive legitimization not accounted for by the simple accumulation of alliances—or unspecified events that may alter the likelihood of new alliances. For simplicity of presentation, we then reestimated these effects using a single variable, time, which ranges from “0” to “8” (with the default year being 1981) and assumes linearity in the effect of time. We observed no differences in the results based on the alternative controls for time. We also controlled for sector differences with two dummy variables, labeled new materials and industrial automation, using the automotive sector as the default sector.

Unobservable organization-level effects were captured by two variables indicating the prior alliance experience of each partnering organization in each dyad (Heckman and Borjas 1980). We computed a measure for each organization that captured the total number of alliances it had previously entered in the past five years. These variables, labeled alliance history, firm 1 and firm 2, capture the possibility of repetitive momentum in individual organizations’ alliance activities, as well as unobserved factors affecting each organization’s proclivity to form partnerships. It is worth noting that these measures are akin to the network analysis notion of “degree centrality,” which is defined as the number of ties in which an actor is involved (Freeman 1979).

We also included a series of financial measures to capture the differ-

ences across the organizations in a number of key dimensions. Insofar as differences in the control of financial resources may result in complementarities that lead to strategic alliances, these controls also can be discussed as an additional way of capturing possible sources of interdependence between organizations (Ghemawat, Porter, and Rawlinson 1986; Barley, Freeman, and Hybels 1992). For each such dimension, we computed a ratio of the smaller to the larger organization. In this way, we controlled for relative differences in financial resources and performance that may have influenced the likelihood of alliance formation between the two organizations. The first dimension was size, measured as total sales in the industry; the second was performance, captured as return on assets normalized to the industry mean (a common measure of performance in managerial research); the third was liquidity. Organizations frequently enter alliances to share the costs of new projects, particularly those involving large resource outlays and risks. In this context, relative liquidity, which reflects the short-term resources available to an organization, is important. We used the "quick ratio"—defined as current assets minus inventory, divided by current liabilities—to measure liquidity (Dooley 1969). Last, we examined solvency differences across the two organizations in each dyad. We used an organization's relative-debt profile within its industry, measured as the total amount of long-term debt divided by the organization's current assets.

We also examined whether each of the above financial attributes for each organization in the dyad separately influenced alliance formation. Thus, for each organization in a dyad, we introduced separate variables indicating the size, performance, liquidity, and solvency of each organization (eight variables). These variables had no effect on our main results and so, in the interest of parsimony, we omitted them from our final analysis.

Table 1 lists the variables, their summary definitions, and their predicted effect on the probability of alliance formation, while table 2 displays descriptive statistics and a correlation matrix for all the variables in the analysis.

Model

We modeled alliance formation using the following dynamic panel model, in which a variable's positive coefficients indicate that it promotes alliance formation:¹²

¹² It is important to note that this approach is distinct from that using the class of models known as network effects or endogenous feedback, which is familiar to network analysts (Marsden and Friedkin 1993). The postulated network effects here result from a lagged network of cumulative prior ties until the previous year, rather than being linked to network elements in the same period.

TABLE 1
DEFINITIONS AND PREDICTED SIGNS OF VARIABLES

Variable	Definition	Prediction
Alliance	Whether two firms formed an alliance in a given year	Dependent variable
Interdependence	Normalized euclidean distance score capturing the absence of overlap of activities between firms	+
Structural differentiation	SD of the normalized prominence scores among firms in the industry	+
Repeated ties	Number of prior alliances between the firms in prior five years	+
Common ties	Number of common partners shared by previously unconnected firms in prior five years	+
Joint centrality	Geometric mean of multiple of centrality of both firms normalized by industry maximum	+
Centrality ratio	Ratio of centrality of lesser to greater value	+
Network density	Ratio of cumulative alliances in an industry divided by the number of possible alliances	+
Time	Year value for each record, ranging from zero to eight	NP
New materials	"1" if firms are in the new materials sector (default: automotive)	NP
Industrial automation	"1" if firms are in the industrial automation sector (default: automotive)	NP
Alliance history, firm 1	Number of prior alliances entered by firm 1 in the dyad	NP
Alliance history, firm 2	Number of prior alliances entered by firm 2 in the dyad	NP
Size	Ratio of sales of smaller to larger partner	NP
Performance	Ratio of performance (ROA) of lesser to greater firm value	NP
Liquidity	Ratio of liquidity (quick ratio) of lesser to greater firm value	NP
Solvency	Ratio of solvency (long-term debt) of lesser to greater firm value	NP

NOTE.—NP = no prediction.

TABLE 2
DESCRIPTIVE STATISTICS AND CORRELATION MATRIX

Variables	Mean	SD	Low	High	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
Alliance21	.09	0	1.00																	
Interdependence	1.42	.28	0	2.47	.16																
Structural differentiation	.29	.08	.15	.41	.24	.00															
Repeated ties	.12	.40	0	5.00	.19	.10	.36														
Common ties	.56	1.21	0	9.00	.15	.03	.19	.32													
Joint centrality	.21	.32	0	1.00	.16	.07	.11	.25	.38												
Centrality ratio	.46	.21	0	1.00	.21	.01	-.02	.12	-.06	.26											
Network density	.16	.09	.06	.22	.16	-.04	.43	.30	.21	.08	.04										
Time	5.72	1.98	0	8.00	.02	.11	.38	.06	.08	.13	-.11	.49									
New materials	.44	.49	0	1.00	.00	.04	.19	-.05	-.12	-.10	.03	.21	.00								
Industrial automation	.23	.42	0	1.00	-.01	.01	-.22	-.03	-.08	-.14	-.13	.10	.00	-.24							
Alliance history, firm 1	3.00	2.79	0	16	.12	.09	.07	.21	.32	.35	.00	.03	.32	.03	-.06						
Alliance history, firm 2	3.13	2.80	0	16	.10	.13	.12	.18	.27	.42	.01	.18	.35	-.05	-.13	.14					
Size27	.25	.09	.94	.02	-.21	.01	.05	.06	-.03	.24	.02	.01	-.03	.00	-.01	-.05				
Performance35	.29	.13	.90	.00	-.09	-.03	.00	.00	.00	.13	.11	-.01	.00	-.01	.00	.00	.00			
Liquidity24	.19	.07	.88	.01	-.02	.00	.02	.03	.00	.07	-.06	.04	.00	.013	.03	.00	.38	.17		
Solvency64	.22	.02	.80	.00	-.05	.09	.01	.03	.00	.16	.00	-.03	.05	-.05	.04	.05	.05	.00	.08	

$$p_{ij}(t) = \Phi(a + bx_{ij} + cy_{ij}(t - 1) + u_{ij}),$$

where $p_{ij}(t)$ is the probability at time (t) of the announcement of an alliance between organizations i and j ; x_{ij} is a time-constant vector of covariates characterizing organizations i and j ; $y_{ij}(t - 1)$ is a time-varying vector of covariates characterizing organizations i and j ; u_{ij} is unobserved time-constant effects not captured by the independent variables; Φ is the normal cumulative distribution function.

We employed a random-effects panel probit model that accounts for unobserved heterogeneity and was implemented here using LIMDEP 6.0 (Butler and Moffitt 1982). Details about our choice of model and the necessity for accounting for unobserved heterogeneity are provided in the appendix.

One concern with analyzing dyadic data is possible interdependence across observations (Lincoln 1984; Mizruchi 1989). To ensure the robustness of our results, we employed a procedure similar to the Multivariate Regression Quadratic Assignment Procedure (MRQAP), routinely used by researchers studying dyads (Krackardt 1987, 1988; Manley 1992; Mizruchi 1992). The percentage of frequency with which the results in the random-sample simulations exceeded the original estimates was far less than 5% in all instances, which attests to the robustness of our probit estimates. Details of these tests are reported in the appendix.

RESULTS

Table 3 presents probit estimates for the effects of factors influencing the formation of new ties between organizations. The coefficients indicate how a change in an independent variable in the previous year affects the probability of two organizations forming a new alliance during the current year.

Model 1 presents a baseline containing an array of control variables. These include the density of alliances in the sector, time, dummy variables for industrial sectors, and controls for each organization's previous alliance experience (labeled alliance history) as well as their similarity in a series of financial indicators. The density of alliances in the prior time period has a positive impact on new alliance formation, which suggests possible legitimization effects. The introduction of alliance density in the model makes the effect of time nonsignificant, suggesting that most linear time-related factors are captured by cumulative industry density. There was a significant improvement in the chi-square statistic once we introduced alliance density, which further suggests that the density of the network may mediate the influence of time on alliance formation. In separate

TABLE 3
RANDOM-EFFECTS PANEL PROBIT ESTIMATES

Variable	1	2	3	4	5	6	7	8	9	10
Constant	1.33* (.24)	1.09* (.21)	58 (.49)	52 (.48)	.50 (.48)	52 (.47)	52 (.47)	.43 (.34)	37 (.35)	.39 (.37)
Interdependence		1.15* (.39)	91* (.30)	.69* (.21)	.66* (.20)	.59* (.17)	.57* (.17)	.52* (.16)	.56* (.17)	.58* (.18)
Structural differentiation			3.78* (1.02)	3.51* (1.01)	3.03* (.91)	2.93* (.88)	2.88* (.89)	2.46* (.74)	2.39* (.70)	2.08* (.63)
Repeated ties				1.38* (.20)	.73* (.15)	.61* (.14)	.60* (.14)	.43* (.12)	.48* (.13)	.44* (.12)
Common ties					1.26* (.14)	1.18* (.20)	1.03* (.21)	.64* (.18)	.94* (.20)	.98* (.22)
Joint centrality69* (.10)	.48* (.13)	.42* (.13)	.29* (.07)	.38* (.13)
Centrality ratio08 (.05)	.06 (.05)	.07 (.05)	.18 (.10)
Structural differentiation × interdependence								-1.79* (.40)		
Structural differentiation × joint centrality									1.38* (.22)	...
Structural differentiation × centrality ratio										1.08* (.20)

Network density	16*	.15*	.11	.10	.11	.11	.10	.09	.12	.12
	(.03)	(.03)	(.06)	(.06)	(.06)	(.07)	(.07)	(.07)	(.08)	(.08)
Time07	.06	.03	.03	.03	.00	.00	.01	.01	.03
	(.05)	(.05)	(.05)	(.04)	(.04)	(.04)	(.05)	(.03)	(.03)	(.02)
New materials	-.23	-.23	-.20	-.21	-.19	-.16	-.15	-.16	-.15	-.17
	(.15)	(.14)	(.14)	(.14)	(.11)	(.12)	(.12)	(.10)	(.10)	(.12)
Industrial automation03	.06	.05	.05	.04	.02	.06	.06	.02	.04
	(.02)	(.05)	(.04)	(.05)	(.05)	(.05)	(.05)	(.04)	(.04)	(.06)
Alliance history, firm 142	.31	.29	.23	.18	.15	.10	.10	.12	.08
	(.27)	(.23)	(.23)	(.21)	(.20)	(.21)	(.16)	(.16)	(.15)	(.14)
Alliance history, firm 211	.24	.19	.18	.17	.14	.17	.16	.15	.16
	(.07)	(.15)	(.14)	(.16)	(.16)	(.16)	(.16)	(.16)	(.15)	(.15)
Size	-.52*	-.47*	-.39*	-.35*	-.32*	-.31*	-.25*	-.20*	-.18*	-.23*
	(.12)	(.12)	(.11)	(.11)	(.10)	(.10)	(.05)	(.05)	(.05)	(.06)
Performance	-.15*	-.09	-.05	-.03	-.03	-.00	-.02	-.02	-.03	-.05
	(.03)	(.05)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)
Liquidity	-.34	-.30	-.27	-.26	-.20	-.18	-.17	-.21	-.19	-.16
	(.19)	(.18)	(.19)	(.18)	(.14)	(.13)	(.11)	(.10)	(.10)	(.12)
Solvency10	.08	.07	.06	.05	.06	.05	.05	.06	.05
	(.12)	(.11)	(.10)	(.11)	(.11)	(.11)	(.10)	(.10)	(.10)	(.10)
Rho44	.43	.36	.35	.32	.30	.29	.24	.21	.23
	(.13)	(.13)	(.10)	(.10)	(.09)	(.09)	(.09)	(.07)	(.07)	(.07)
N	7266	7266	7266	7266	7266	7266	7266	7266	7266	7266
Chi square	45.73*	49.34*	58.47*	65.62*	69.76*	74.57*	76.71*	83.42*	87.11*	85.04*
	(.03)	(.02)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)

NOTE.—SDs are in parentheses

* $P < .05$.

analyses, we also introduced a variable capturing the number of alliances announced in the industry in the previous year, but this variable was not significant once we controlled for industry density and thus was not included in the models.

The variables for alliance history of each organization were insignificant across all models, indicating that the individual prior experience with alliances of each organization within a dyad did not make an alliance between them more likely. Model 1 also included ratios measuring the similarity between pairs of organizations in size, performance, liquidity, and solvency. Except for size, similarity of financial indicators did not have a significant impact on the probability of alliance formation. Alliances are more likely to occur between organizations of different size. While these ratios are introduced as controls, they also capture any residual effects of interdependence not accounted for by our measure of interdependence. We also examined whether the above financial attributes introduced separately for each organization in the dyad influenced alliance formation, but these variables had no impact on our main results and were omitted from our final analysis in the interest of parsimony.

Model 2 introduced our measure of interdependence between the members of the dyad. As predicted in hypothesis 1, organizations separated by a larger normalized euclidean distance in the matrix that captures interorganizational interdependence were more likely to enter alliances. This result is congruent with research on the role of interdependence in alliance formation and also helps enhance the construct validity of our indicator. Our alternative measure for interdependence, using membership in the clusters corroborated by industry experts, yielded similar results.

We introduced structural differentiation in model 3. As predicted by hypothesis 6, structural differentiation has a strong positive effect on alliance formation. Introducing this measure also leads to a significant improvement in the fit of the model, as measured by the chi-square statistics. Moreover, the effect of density became nonsignificant at the .05 level once we introduced structural differentiation into this model. This suggests that the systemic effects on tie formation captured by density may actually be mediated by the structural differentiation of the network. Although hypothesis 6 was not formulated as an alternative for a density-dependent legitimization process, the statistical insignificance of the density effect in model 3 suggests that the increase in the probability of alliance formation is perhaps best explained by the growing differentiation of the network structure. Thus, the upward-sloping rate of alliance formation during the 1980s may be prompted by the emergence of a differentiated social structure that made it easier for organizations to identify suitable partners in

an uncertain environment, rather than a consequence of a legitimization effect driven by the mere accumulation of ties over time.¹³

Models 4–7 test the effect of the various embeddedness mechanisms on alliance formation, as predicted by hypotheses 2–5. Models 4 and 5 confirm the expected influence of both relational and structural embeddedness on subsequent alliance formation, as proposed in hypotheses 2 and 3. In model 4, the positive and significant coefficient of repeated ties indicates that the presence of prior ties between organizations in the previous five years positively influences the likelihood of their forming a new alliance. In model 5, the positive and significant effect of common ties indicates that shared third-party ties between previously unconnected organizations increases their probability of entering an alliance. The effects of both direct and indirect ties on alliance formation remain significant across all the models.¹⁴

Models 6 and 7 examine the role of the position of organizations in the emergent structure of interorganizational ties on their alliance behavior. Model 6 shows that the probability of two organizations forming a new alliance increases with the joint centrality of the potential partners, as predicted by hypothesis 4. The evidence for hypothesis 5, which predicts an increase in the probability of an alliance between organizations with similar centrality, is less conclusive. The results indicate that the difference in centrality scores does not have a statistically significant influence on the likelihood of an alliance between two organizations. In separate

¹³ We also assessed the impact of diversity-dependent legitimization on alliance formation by modeling the effects of the diversity of alliances formed within industries. Diversity was conceptualized in terms of the kinds of governance structure organizations used to formalize their alliances. We assessed diversity with two sets of measures. First, we computed the reciprocal of the Herfindahl index for governance structure of prior alliances. Second, we used a specification akin to Blau's index of heterogeneity (Blau 1977; Powell et al. 1996). We computed the proportion of organization i 's ties of type j until year t , out of the total number of ties the organization had entered until that year, denoted as $P_{i,j,t}$. We defined six types of governance structures. We computed the index of diversity $Y_{i,t}$ by subtracting the summation over all j of the square of $P_{i,j,t}$, that is, $Y_{i,t} = 1 - \sum_j (P_{i,j,t})^2$ and $(1 \leq j \leq 6)$. The results were insignificant for both measures of diversity across all models and were thus not included with the final analyses.

¹⁴ We also tested polynomial transformations of the two cohesion variables to account for nonlinear effects. The results suggest that the relationship between previous alliances and future alliances within the dyads is best described as an inverted U-shaped relationship, captured by a second-order polynomial function. The effect, however, is exponential for shared common ties between unconnected organizations. As the number of common ties between organizations increases, the likelihood of their allying with each other increases disproportionately. The inclusion of the polynomial transformations does not affect the results obtained with the linear forms. We report the results of the linear model for the sake of parsimony.

estimations, however, we found that the ratio of centrality was positive and significant if joint centrality was excluded from the model. To interpret this result, it is worth noting that the ratio of centrality approaches 1.0 for any two organizations that have similar centrality scores, regardless of their absolute level of centrality. In other words, while two "central" organizations are similar, so are two "peripheral" ones. Yet our positional analysis of the networks has shown that peripheral organizations are much less likely to enter alliances. If they do, they are likely to do so with central organizations, not with other peripheral ones. Central organizations form ties with other central organizations and, to a lesser extent, with less central organizations. Thus, the homophily tendency implicit in hypothesis 5 only applies to central organizations. Once the joint centrality of the dyad is controlled for, the effect of homophily is no longer significant. Viewed in this light, these results are consistent with the distinction introduced by Mizruchi (1993) between "central" and "peripheral" role equivalence, which suggests that there are significant differences in the behavior of these two types of actors that cannot be easily captured by a single sociological construct.

Models 8, 9, and 10 assess the moderating influence of structural differentiation on both endogenous and exogenous drivers of alliance formation. We had predicted that structural differentiation in the system would moderate the influence of positional embeddedness on alliance formation (hypothesis 8). This prediction should translate into a significant and positive coefficient for the interaction between structural differentiation and both joint centrality and similarity in centrality. But we also predicted that the effect of exogenous interdependence on alliance formation would diminish with structural differentiation (hypothesis 7). This effect should yield a significant and negative coefficient for the interaction between interdependence and structural differentiation. We tested these models separately because of concerns of multicollinearity across the interaction terms.

Model 8 introduces an interaction term between interdependence and structural differentiation. The negative coefficient for the interaction term supports hypothesis 7 and suggests that the explanatory power of interdependence diminishes with the growing differentiation of the social structure of interorganizational ties, but interdependence on its own has a positive impact on alliance formation across all models. Thus, while exogenous factors do influence the creation of new ties, the increasing structural differentiation of the network enables organizations to use this network as a source of information for their future partnerships, which mitigates the effects of exogenous interdependence on the formation of new alliances.

Models 9 and 10 introduce the interactions between structural differentiation and the two indicators of positional embeddedness. Model 9 tests

the interaction between structural differentiation and joint-dyad centrality, and model 10 adds the interaction between structural differentiation and the similarity in centrality within the dyad. Together, these two models test the contingent influence of positional embeddedness on alliance formation (hypothesis 8). Both models show significant positive effects for the interaction between positional embeddedness and structural differentiation. The impact of both joint centrality and similarity in centrality increases with the level of structural differentiation of the emerging network. This suggests that the effect of positional embeddedness on alliance formation increases with the level of structural differentiation of the emerging network. As predicted by our framework, the effective impact of positional-embeddedness mechanisms on subsequent tie formation is thus contingent upon the level of structural differentiation of the network.

Although similarity in centrality was not a significant predictor of alliance formation, the interaction between this variable and structural differentiation is statistically significant. This suggests that, with the growth of structural differentiation, organizations may become increasingly aware of differences in centrality when choosing a partner, although this tendency is not strong enough to make the difference in centrality statistically significant during the period of observation. The more the respective positions of organizations in the network become apparent, the more difficult it may become for a peripheral organization to build alliances with a central one. Although this does not mean that such alliances will not occur, it does suggest that peripheral organizations may need to possess some unique attributes that can enhance their attractiveness as alliance partners with central organizations.

DISCUSSION

The central message of this research is that the formation of a new interorganizational network structure results from a longitudinal dynamic in which action and structure are closely intertwined. Our model portrays the social structure of interorganizational relations as a "macro" phenomenon emerging out of the "micro" decisions of organizations seeking to gain access to resources and to minimize the uncertainty associated with choosing alliance partners. The network structure that results from the accumulation of those ties increasingly becomes a repository of information on potential partners, helping organizations decide with whom to form new alliances. The emerging alliance network consequently increasingly influences organizational action. In this model, the dialectic between macro and micro is thus translated into the dialectic between structure and action.

The results show that both interdependence and network embed-

dedness factors have a significant impact on new alliance formation. Consistent with prior research, organizations build ties with other organizations that have complementary resources and capabilities, but they also take into consideration the position the potential partners have in the emerging social structure of the network. The influence of interdependence and network factors is contingent on the level of structural differentiation of the social system. The role of positional embeddedness in alliance formation increases with the growing structural differentiation of the emerging interorganizational network, while the impact of exogenous factors declines. These findings support our claim that the increasing differentiation of social structure reflects a process by which the network becomes a repository of information about potential partners. The higher the structural differentiation of the emerging network, the more organizational decisions about new partnerships are guided by endogenous network considerations and the less by exogenous factors.

As we interpret the results, the emerging alliance network progressively internalizes relevant information about competencies, needs, and reliability of potential partners. The embeddedness mechanisms enable organizations to identify complementary and reliable partners, reducing the hazards of cooperation. This interpretation implicitly assumes that there is no tension between instrumental and social drivers of alliance formation, yet our results do not preclude the possibility of such a tension. The emergence of a network structure increases the information available to organizations, but it may also limit the effective range of potential partners organizations are likely to consider. The possibility that instrumental rationality could be subordinated to embedded action has been emphasized often in neoclassical economics. Yet sociologists have also suggested that in some situations, social structures may actually hinder, rather than help, the pursuit of economic interest. Studies of ethnic entrepreneurs (Portes and Sensenbrenner 1993) and middle managers (Gargiulo and Benassi 1998) suggest that the same social mechanisms that facilitate instrumental cooperation may also have a "dark side" that can damage an actor's ability to pursue instrumental goals (Gulati and Westphal 1999). One of the themes in these studies is that membership in cohesive clusters hinders the actor's ability to build cooperative ties with actors not connected to that cluster. A similar risk is implicit in our structural-embeddedness mechanism and could equally limit the formation of ties within a cohesive "core" of central organizations.

By relying on an evolving social structure, boundedly rational organizations effectively diminish the uncertainty associated with picking partners, but gains in partner reliability may be offset by the limitations on the choice of potential partners. Some features of strategic alliances suggest that this trade-off may be more than a theoretical possibility. The

hazards of interorganizational cooperation, coupled with the difficulty of assessing complementary capabilities and the often ambiguous link between alliances and organizational performance, may prompt organizations to enter "secure" partnerships that could fail to realize the full potential of strategic alliances they could have entered. Future research in this field should investigate a possible trade-off between the reduction of uncertainty attained through embedded partnerships and pursuit of the instrumental logic that promotes interorganizational cooperation. If such a trade-off exists, the search for an alliance partner could result in a path-dependent process (Arthur 1989), in which instrumental rationality is at times subordinated to considerations of embeddedness. While available evidence suggests that organizations usually avoid the perils of excessive involvement with the same partner (Gulati and Lawrence 1999), they may still be victims of subtler forms of "overembeddedness" that could limit their search for partners, depriving them of the full benefits of strategic alliances. Consistent with Gargiulo and Benassi's (1998) work on managerial networks, we could expect that organizations tied to a cohesive cluster of alliance partners might run a higher risk of falling into a path-dependent process that effectively limits their range of potential partners.

Our analysis of the structure of alliance networks uncovered the emergence of core-periphery structures in the automotive, new materials, and—to a lesser extent—industrial automation industries. The structural-differentiation process that is at the core of our model of network formation, however, can be compatible also with alternative structural configurations. In some circumstances, structural differentiation may result in a structure with two or more blocs of organizations (or "factions") with relatively few ties across blocs. Such structures typically arise when there are strong exogenous barriers to the formation of ties between organizations from different blocs, like those between defense firms belonging to the Soviet and the western blocs during the cold war. It is worth noting that core-periphery structures are compatible with polarization, a clear example of which is the world system before the collapse of the former Soviet Union (see Van Rossem [1996] for analysis and discussion). In such structures, the process of structural differentiation leading to core-periphery structures may still operate within clusters, while exogenous forces restrict the formation of ties across clusters. The increasing globalization of the economy, however, makes it imperative for most large business organizations to have access to all major markets and to all possible sources of innovation. Insofar as strategic alliances are a crucial tool to attain these goals, faction-type interorganizational structures should be less likely to occur, except when geographical distance, competitive network dynamics, or geopolitical considerations restrict access to other organizations that may have complementary capabilities.

While our model proposes that the emergence of network structures is driven by the endogenous differentiation of this structure, emerging interorganizational networks may not always evolve into a definite structural pattern. This could occur in certain new, extremely dynamic, innovation-driven industries, where all players could benefit from alliances with almost any other player. In the absence of players that can establish their dominance through their superior command of financial or other resources, like Microsoft in the software industry and the large pharmaceutical companies in biotechnology, the evolution of the emerging network may not reveal any definite pattern. According to our theory, organizations in such industries would face extremely high levels of uncertainty at the time of building cooperative partnerships because they would lack the guidance of embeddedness mechanisms. In those cases, however, it is conceivable that other networks—such as those resulting from the circulation of engineers between firms in Silicon Valley or from the relationships between university researchers and industry researchers as in biotechnology—may provide an alternative to the network of prior alliances as a source of information tapped by organizations. While this may result in an interorganizational network structure that partially mirrors the pattern of the networks that provided information to organizational decision makers, this may not necessarily occur since such alternative networks may have resulted from exchange processes that differ from the ones that drive strategic alliances.

Another important issue raised by the results pertains to the relationship between structural differentiation and information, on one hand, and alliance formation, on the other. Although our model implicitly assumed a linear relationship between the amount of information internalized in the network structure and the differentiation of this structure, we have also acknowledged that further increases of structural differentiation beyond a certain critical level could actually *decrease* the level of information in the system, which in turn would have a negative effect on the formation of new alliances. This outcome would be consistent with the logic underlying our theory but not with the monotonic relationship between structural differentiation and alliance formation predicted by our hypotheses. Yet the monotonic relationship in the hypotheses is a simplification warranted by our data, which cover only a segment in the development of the observed interorganizational networks. A more extended observation period would have allowed us to explore the relationship between structural differentiation and alliance formation in more detail.

From a theoretical standpoint, the relationship between structural differentiation and alliance formation could take two different forms. Each of these forms corresponds to alternative effects of new ties on the differentiation process in the network structure. First, new ties may prompt a

continuous increase in the differentiation of the emerging social structure. This continuous increase would eventually result in lower levels of information, which should reduce the probability of new alliances. The relationship between structural differentiation and new tie formation would then correspond to an inverse U-shape. Second, the emerging social structure may reach a state where the creation of new ties simply reproduces a stable pattern of distinct positions occupied by organizations with similar relational profiles, without further increasing the differentiation of the network. Such an evolution would result in stable levels of structural differentiation over time, which would effectively turn it into a constant in our dynamic models. This is consistent with existing research showing that mature interorganizational structures typically evolve into stable positions occupied by actors with similar network profiles (White 1981; Burt 1988). It is unfortunate that we could not test these alternatives with our data, since alliance networks were far from stabilizing at the end of our observation period. The evolution of structural differentiation in emerging networks may also be specific to the system—or type of system—under consideration. In this case, future research should investigate the factors that might affect the growth of structural differentiation as well as those that might influence its stabilization in some mature structures.

While this article has focused on the effects of structural differentiation on alliance formation, our construct may be important also in other areas of research on the effects of social structures on behavior. The central tenet of network research is that the pattern of social ties among actors is the main driving force behind those actors' attitudes and behaviors (Wellman 1988). Network scholars have shown how this approach can provide new insights into a varied set of social phenomena, including diffusion of innovations (Burt 1988; Westphal, Gulati, and Shortell 1997), social influence (Galaskiewicz and Burt 1991), political contributions (Mizruchi 1989, 1992), control strategies (Gargiulo 1993), and organizational performance in competitive situations (Burt 1992). Most network research, however, assumes a relatively stable structure that creates constraints and opportunities for individual behavior. Our research suggests that the effects of network structures on behavior may be contingent on the level of structural differentiation of that network. Future longitudinal studies on network effects on the behavior of organizations should take into consideration not only the mechanisms through which the network structure affects behavior but also how the level of structural differentiation of this network moderates the effective impact of those mechanisms on organizational action.

Our focus on the origin and evolution of networks complements recent efforts to develop mathematical models of longitudinal network data, in which the dynamics of social networks are similarly modeled as a function

of exogenous factors and endogenous network parameters (e.g., Iacobucci and Wassermann 1988; Carley 1990, 1991; Zeggelink 1994; Leenders 1995, 1996; Snijders 1996). These models often include specific network mechanisms such as reciprocity and transitivity to spell out how previous ties pattern the formation of future ties (e.g., Leenders 1995), which are akin to our relational- and structural-embeddedness mechanisms. In most cases, however, the main goal of this work has been the development of mathematical models to analyze longitudinal network data, rather than to develop specific theory on the factors driving the dynamics of networks. Our research contributes to these attempts by showing empirically how the formation of cooperative interorganizational networks results from a dynamic process in which networks are both a driving force and a product of this process.

Although this article has focused on the emergence of cooperative interorganizational ties that take transactions out of the market logic, it nevertheless has implications that are pertinent for the development of the economic sociology of markets. Sociologists have demonstrated that under conditions of uncertainty and imperfect information, market players use the network of interorganizational relationships to guide their action. The reliance on existing networks leads to a self-reproducing market schedule (White 1981; Leifer and White 1988). Our results suggest that the social mechanisms that sustain a mature market structure might also play an important role during the formation of that structure. There may be an important difference in network dynamics between the formative and mature stages of a market structure that does not originate from differences in the nature of the mechanisms that guide the behavior of organizations but results from differences in the effective impact of these mechanisms on organizational action. If the emergence of market structures follows a pattern similar to the one we uncovered in alliance networks, the information content of the social structure of the market is likely to be scant in the early stages of the market formation process. This primitive market structure would provide little guidance to market players, who should face considerable levels of uncertainty. In a mature market, the informational content of this network stabilizes, resulting in the markets that White (1981, p. 518) described as "self-reproducing social structures" among organizations that evolve different roles by observing each other's behavior. In such markets, organizations may come and go, but the overall structure of market transactions remains stable, as Burt (1988) has demonstrated in his longitudinal analysis of American markets.

Our focus on the coevolution of organizational actions and of macrostructures resulting from the cumulated networks complements recent attempts to understand the coevolution of organizations and institutions (e.g., Davis, Diekmann, and Tinsley 1994; Haveman and Rao 1997; Davis

and Greve 1997). While we have focused on interorganizational networks of cooperation, our approach could be extended to consider the potential role of other types of interorganizational networks in enabling the creation of new ties and in facilitating the production of institutions that may ultimately regulate the subsequent production of such ties (e.g., Stern 1979). Thus, the development of coevolutionary accounts of embedded organizational action and of the macrostructures that result from that action remains an important line of inquiry that could benefit from attention to the endogenous network factors described in this article.

This study opens several important avenues for future research. First, scholars could examine the relative importance of endogenous embeddedness dynamics across a wider array of industries. Since endogenous embeddedness is a way to cope with the hazards of cooperation, its role on alliance formation may be affected by industry-specific factors such as the level of technological uncertainty and the rate of change. In a preliminary examination of this question, we found that structural differentiation played a more significant role in the high-uncertainty new materials sector than in the more certain automotive sector. Additional research in this direction may shed light on the contingent effects of structural differentiation across industries.

Second, since structural differentiation facilitates the selection of adequate alliance partners, embedded alliances formed in more differentiated social structures should have comparatively higher levels of success. Testing this proposition, however, would require detailed survey data on the quality, duration, and relative performance of the cooperation within the alliances (Zaheer, McEvily, and Perrone 1998). Third, since different types of alliances entail different levels of risk, future studies could examine the role of network embeddedness across various types of alliances.

Finally, the domain of inquiry could be expanded beyond the formation of alliances and consider additional organizational activities such as mergers and acquisitions, which also may be influenced by endogenous embeddedness. While each of these possibilities opens important avenues for future research, examining them would require significant additional data. We hope that this study will stimulate scholars to collect such data and expand our understanding of the dynamics associated with various types of interorganizational networks.

This article proposed a model in which the formation of interorganizational networks is the evolutionary outcome of socially embedded organizational action. Our model provides a systematic link between the social structure of an organizational field—understood in network terms—and the behavior of organizations within the field. This link is bidirectional. On the one hand, the emerging social structure progressively shapes organizational decisions about whether and with whom to create new ties. On

the other hand, this social structure is produced by the (structurally shaped) decisions of individual organizations to establish relations with one another. Seeking an answer to the question in our title, we have shown that interorganizational networks result not only from exogenous drivers such as interdependence but also from an endogenous evolutionary dynamic triggered by the very way in which organizations select potential partners. In this model, actors not only react to conditions of their own making but in the process reproduce and change those very conditions. The endogenous embeddedness model opens the way to more detailed studies of network-formation processes that go beyond the role of exogenous factors and consider the dialectic between action and structure that is at the core of many social processes.

APPENDIX

Interdependence of Observations

We conducted a number of additional tests to address concerns of interdependence across observations resulting from our dyadic approach, which led to the presence of the same organization across multiple dyads. We employed a procedure similar to the Multivariate Regression Quadratic Assignment Procedure (MRQAP), routinely used by researchers studying dyads (Krackardt 1987, 1988; Manley 1992). Our approach differs from MRQAP in that we used the random-effects probit model instead of ordinary least squares regression for each iteration of the simulation. As a result, we randomized the key network variables for each time period for each industry. We ran 500 iterations of a completely specified random-effects model with a new randomized independent network variable obtained by random permutations of the rows and columns in each alliance matrix for each industry and year. The coefficients obtained were compared with those obtained in the original formulation. The percentage of frequency with which the independent variables exceeded their original values divided by the number of permutations plus 1 (in this case, we used 501) indicates the statistical reliability (pseudo *t*-test) of the original results.¹⁵ This test can be interpreted like conventional tests of significance: a result of less than 5% (or, even better, 1%) provides evidence that the original estimates are indeed accurate. The benefit of a randomization procedure is that obtaining satisfactory results does not require an assumption of independent observations, a random sample, or a specified

¹⁵ A more complete specification of this test would have entailed randomly extracting the 500 permutations from all possible ones for each industry (Mizruchi 1992), which was not feasible here due to the extremely large number of permutations that would be necessary for each industry and for each year.

distribution function. This procedure allowed us to assess the efficiency of our results, a primary concern resulting from any dyadic interdependence.

The manner in which we specified our network-embeddedness effects makes our model akin to the P^* logit models recently proposed by Wasserman and Pattison (1996). Building on the pioneering work by Holland and Leinhardt (1970) and on Strauss and Ikeda (1990), P^* models produce pseudo-maximum-likelihood estimators of the probability of observing a binary tie x_{ij} , conditional on the rest of the data, without having to make the implausible assumption that the observations (dyads) are independent. Specifically, these models build into a logistic regression parameters that capture possible sources of interdependence of the observed dyads—such as reciprocity, transitivity, in and out degree of each dyad member, and network density—and obtain estimators of the effect of these parameters on the conditional probability of $\{x_{ij} = 1\}$. Our models include network parameters that are similar to the ones of a typical P^* model—transitive triads, the degree of each dyad member, and network density—but we measured these parameters on the network at $(t - 1)$, while a strict pseudolikelihood estimation requires parameters measured on the same network that contains the predicted tie. Since the inclusion of the $(t - 1)$ parameters cannot be considered an adequate safeguard against the potential effects of nonindependent observations, we used the above-mentioned MRQAP-like procedure to test the robustness of the results and limit concerns of interdependence. The percentage of frequency with which the results in the random-sample simulations exceeded the original estimates was far less than 5% in all instances. Thus, we can say with some confidence that for these data reasonable coefficients were obtained.

The problem of cross-sectional dyadic interdependence can also be understood as one of model misspecification (Lincoln 1984). If a statistical model incorporated all essential nodal (organization-level) characteristics that influence alliance formation, no unobserved effects resulting from common nodes would remain. To capture any organization-level effects across dyads sharing the same organization, we controlled for each organization's cumulative history of alliances. Organization history is an important factor that captures any residual organizational propensities to engage in alliances (Heckman and Borjas 1980; Black, Moffitt, and Warner 1990). As noted earlier, we also ran separate estimations in which we included a host of financial attributes of each of the organizations in the dyad, including its size, performance, liquidity, and solvency. In addition to these controls, the models used here account for unobserved heterogeneity and adjust for such systematic biases resulting from missing variables. We expected the unobserved heterogeneity term (ρ) to capture any residual dyad-level effects not included in the model.

Unobserved Heterogeneity

An issue that arises when analyzing data on a time series of cross-sections, or panel data, is the possibility of unobserved time-invariant effects known as "unobserved heterogeneity." This is of particular concern for this study with respect to the claim that the prior history of alliances between two organizations affects the future likelihood of their entering an alliance. There are two distinct explanations for this empirical regularity, if it occurs (Heckman 1981*a*, 1981*b*). One explanation is that a genuine behavioral effect exists, whereby, because of the prior alliances it has experienced, a dyad's preferences are altered in the future. In econometric terms, such a behavioral effect is called "state dependence"—the likelihood of an event is a function of the state of the unit.

If state dependence alone encapsulated the empirical reality, there would be no problem; however, there is another possibility that, if not accounted for, could lead to spurious results: dyads may differ in their propensity to enter alliances because of unobserved factors. In this instance, such unobservable effects could result from permanent differences between dyads in their preferences for alliances, such as geographical proximity, not captured by the independent variables. If this noise were systematic for the same unit over time, it could lead to a serial correlation among the error terms for those observations, which would yield consistent but inefficient coefficients, rendering any statistical testing inaccurate. Furthermore, prior alliance experience may appear to be a determinant of future alliance formation solely because it is a proxy for temporally persistent unobservable factors that determine alliance formation and nonformation. Improper treatment can lead to spurious effects appearing with attempts to assess the influence of past experience on current decisions; this phenomenon is also termed "spurious state dependence" (Black et al. 1990; Heckman 1981*a*, 1981*b*; Hsiao 1986).

In a statistical sense, the problem of unobserved heterogeneity relates to model specification (Peterson and Koput 1991). If a model is completely specified, no such problem occurs, but most statistical models suffer from some degree of omitted variable bias. Another way to confront this problem is to refine the risk set studied. In the current design, we include all possible dyads within each industry for each year as the set of dyads at risk of entering an alliance. It is quite likely that some of these dyads are in fact not at risk of entering an alliance in some or even all observation periods, while other dyads have a higher propensity to ally. This suggests the possibility of misspecification of the risk set unless adequate allowances are made for such unobserved differences in propensity. One way to deal with such a bias is to clean up the risk set by eliminating records

unlikely to experience the event, a process analogous to removing men from pregnancy studies. The difference in propensity is frequently a result of unobservable factors, however, making it impossible a priori to weed out records from the sample on reasonable grounds without biasing the sample.

Two approaches frequently used to address problems of unobserved heterogeneity are fixed- and random-effects models. Fixed-effects models treat the unobserved individual effect as a constant over time and compute it for each unit (dyad). The method entails estimating a constant term for each distinct unit and including dummy variables for each and is similar to least squares with dummy variables (LSDV) regression models (Hannan and Young 1977; Mizruchi 1989). Random-effects models treat the heterogeneity that varies across units as randomly drawn from some underlying probability distribution. Both types of models have shortcomings. Both assume that the unobserved effects are time invariant. Fixed-effects models are applicable only to repeatable events (Yamaguchi 1991), do not allow the inclusion of time-independent covariates (Judge et al. 1985; Reader 1993), and involve estimating a large number of parameters, which grows with sample size (Chamberlain 1985). This approach can be problematic when there are many groups but only a few observations in each group (Chamberlain 1985). Random-effects models are more tractable but also assume that the unobserved effect is not correlated with any of the exogenous variables in the system (Chintagunta, Jain, and Vilcassim 1991; Hausman and McFaden 1984).

To address concerns of heterogeneity, we employed a random-effects panel probit model, developed by Butler and Moffitt (1982), for the statistical analysis.¹⁶ Our decision to employ a random-effects model was based on the following. First, estimates computed using fixed-effects models can be biased for panels over short periods (Chintagunta et al. 1991; Heckman 1981*a*, 1981*b*; Hsiao 1986). This is not a problem with random-effects models. As all the dyads in our sample were present for only nine years, random effects was clearly the favored approach. Second, fixed-effects models cannot include time-independent covariates, a limitation that would have meant excluding several variables, and an analysis without some of these variables would have been severely limited. The computa-

¹⁶ In random-effects models, numerous alternatives are possible, depending on the choice of form for the distribution of unobservables. Although Butler and Moffitt (1982) specified a normal distribution, other functional forms are also possible. Recent efforts have moved away from functional specification of heterogeneity toward semi-parametric random-effects approaches that estimate the probability distribution directly from the data (cf. Chintagunta et al. 1991).

tion of random-effects models is relatively straightforward for continuous dependent variables but more problematic for qualitative choice variables and was implemented here using LIMDEP 6.0.

We also tried to address concerns of heterogeneity by conducting the analysis using three increasingly restrictive definitions of the risk set. The first set included all dyads in the sample, the second set included only dyads in which at least one member had prior alliance experience, and the third set included dyads in which both members had entered into at least one alliance. The results obtained with different sets were convergent, and we report those based on the complete set.

Comparative Analyses

The primary theoretical contention underlying our use of network measures is that the ties formed in an industry are not random but are driven by the structure of relationships formed in prior years. The models that include network variables were expected to be powerful predictors of alliance formation to the extent that (a) alliance formation among organizations arises from the flow of information underlying the networks of preexisting relationships and (b) the specific structural models used to reflect these information flows cluster organizations that are densely connected by such informational links (Friedkin 1984).

To verify our claims of systematic interorganizational alliances, we compared the results for this study's sample against results obtained with a sample in which the formation of alliances was assigned randomly. The implicit null hypothesis here is that an observed pattern in the data is due purely to chance. Such a comparative analysis serves as a valuable baseline (cf. Zajac 1988). Finding no differences in the predictive power of the independent variables for the actual and random dependent variables, or greater predictive power for the random dependent variable, would suggest that the postulated independent effects could have predicted the random occurrence of alliances just as well or better. As a result, our claims for systematic patterning of alliances would be moot.

We tested the predictive ability of each model specified in table 3 against random assignments on the dependent variable on the basis of its original distribution. The results indicated that none of the hypothesized effects are better predictors of randomly assigned alliances than those in table 3. Not a single independent variable is significant in all the models. This finding allows us to reject the implicit null hypothesis and suggests that the postulated independent effects are not at all good predictors of the random occurrence of alliances. The exogenous interdependence and endogenous embeddedness effects explain the systematic pattern of alliances.

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Family Integration and Children's Self-Esteem¹

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In this article, we introduce the concept of family integration to describe the way in which family social organization affects individuals. We hypothesize that when parents are integrated into the family, it benefits their children's development of self. Using panel data, we test three mechanisms of parental family integration—activities within the home, family social networks, and family support networks. The results show that parental family integration early in a child's life has positive effects on the child's self-esteem in early adulthood, 23 years later. These findings provide important new insights into both the social processes affecting self-esteem and the long-term consequences of various dimensions of family integration.

INTRODUCTION

For decades, sociologists have studied the impact of the social organization of the family on individuals. Although studies such as *New Families, No Families?* (Goldscheider and Waite 1991), *Embattled Paradise* (Skolnick 1991), and others reflect contemporary interest in this area, William F. Ogburn and others were examining changes in the family organization of activities and their effects on the well-being of individual family members in the early 1930s. They reported that social and technological changes were substantially altering the roles and activities of the family, sometimes leading to family disorganization and negative outcomes for parents and children (Ogburn and Tibbitts 1933, pp. 661, 688). And decades earlier, Emile Durkheim ([1897] 1951, p. 378) observed that family integration and solidarity were decreasing, thus weakening the family's

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power to maintain personalities and prevent anomie. Even Karl Marx and Friedrich Engels's ([1888] 1978, pp. 476, 487–88) critiques of capitalism were inspired, in part, by capitalism's destructive effects on the family, which, in their view, transformed true family relationships into mere organizations for industrial production. In short, the study of families, their organization, and their impact on individual well-being is as old as the study of sociology itself. In this article, we return to these theoretical questions of the impact of family social organization on individual well-being by examining a central dimension of the self: self-esteem.

Although much research suggests the family greatly influences self-esteem, the literature tends to focus on the supportive and emotional aspects of the family rather than its characteristics as a form of social organization (Demo and Acock 1988; Demo, Small, and Savin-Williams 1987; Gecas and Schwalbe 1986). As we describe below, there are sound theoretical reasons to expect the social organization of the family to affect children's self-esteem. This development of the self and the social organization of the family as a whole, however, are related in reciprocal ways. These complexities make empirical estimation of this relationship difficult. There are also important reasons to believe that multiple dimensions of family social organization may affect the development of the self, but such considerations also add to the complexity of the relationship. These complexities impede both theoretical and empirical progress toward a clearer understanding of the impact of family social organization on self-esteem.

The present article advances our knowledge of the relationship between family social organization and self-esteem in three ways. First, we draw on the modes of social organization approach (Thornton and Lin 1994) to derive a theoretical model arguing that family integration develops when family organization of activities is high. We theorize several mechanisms through which family social organization affects family members' lives. Focusing on self-esteem, we then hypothesize that family integration influences children's development of the self. We argue that when a child grows up in a family with high family integration, that integration has positive effects on the child's self-esteem. Second, our empirical tests of this hypothesis investigate the long-term impact of family integration in early childhood on young adulthood self-esteem. This long-term perspective helps to untangle the issues of reciprocal causation, and it focuses on the causal relationship between family integration and self-esteem. Third, we explicitly recognize the multidimensional nature of family integration, giving both theoretical consideration and empirical attention to its multiple dimensions.

The data for the empirical tests come from a 23-year intergenerational panel study of mothers and their children. This unique resource includes

rich measurement of multiple dimensions of family integration from the first year of the children's lives. The data also include measures of the children's self-esteem (from the Rosenberg Self-Esteem Scale) when they reach early adulthood, 23 years later. We use these data to examine family integration during early childhood and its long-term effects on the self-esteem of young adults.

THEORETICAL ISSUES

The Family as a Form of Social Organization

In this article, we examine the impact of the family as a form of social organization on children's self-esteem. The consequences of any form of social organization—whether it be a factory, a family, or a school—depend on at least two factors: characteristics of the individuals who compose the organization and the configuration or form of the organization itself. The former factor—characteristics of the individuals—has been widely studied in family research. For example, the children of parents with more education tend to have better outcomes in a variety of domains, including educational attainment (Marini 1978) and premarital childbearing (Wu 1996). Similarly, children of parents who are affectionate and supportive appear to have higher self-esteem and lower rates of depression (Robertson and Simons 1989).

The latter factor of social organization—the configuration, arrangement, and organization of the individuals themselves—has received only the briefest treatment in studies of family effects. The literature abounds with studies of “family structure,” yet this concept yields only a crude idea of how the family is organized; family structure is often simply conceptualized as the presence or absence of certain adults in the household. This is certainly an important factor, yet it tells us little about the inner workings of the family and its activities and relationships with other forms of social organization such as the workplace, schools, and extended family relations.

We focus on the family as a form of social organization and specifically investigate the ways in which different forms of family organization affect children (in this case, their self-esteem). To aid our conceptualization of the family as a form of social organization, we turned to the modes of social organization framework (Thornton and Fricke 1987; Thornton and Lin 1994). This framework argues that there are two ideal types of family life on two ends of a continuum. On one end is the highest level of the family mode of organization, in which all of an individual's social life is organized around the family, including production, resources, recreation, education, protection, associates, and other social activities and relationships. The other end of this spectrum is the nonfamily mode of organiza-

tion, in which all of an individual's social activities are organized around nonfamily units, such as businesses, schools, and governmental agencies. Although no one family's organization matches either of these two extremes, most fall somewhere between these two ideal types of family organization. We use the term "family integration" to describe the extent to which individual lives are characterized by a high degree of family organization. For example, a person may be integrated into the family through economic, social, and recreative activities or only through a subset of these domains.

Family Integration and Self-Esteem

Self-esteem is generally conceptualized as the central evaluative component of the self, and it reflects the extent to which individuals believe they are worthwhile and merit respect (Coopersmith 1967; Rosenberg 1965; Rosenberg and Kaplan 1982; Wylie 1979; Rosenberg, Schooler, Schoenbach, and Rosenberg 1995). Self-esteem is often considered to consist of two components: specific self-esteem and global self-esteem (Rosenberg et al. 1995). Specific self-esteem reflects the performance of a particular activity, global self-esteem reflects the individual's attitude to the self as a totality, and these two dimensions of self-esteem are not the same construct (Rosenberg et al. 1995, p. 143).

We consider global self-esteem, which is often measured by the Rosenberg Self-Esteem Scale. Rosenberg (1965, p. 65) writes: "By self-esteem we refer to the evaluation which the individual makes and customarily maintains with regard to himself: it expresses an attitude of approval/disapproval." An important aspect of Rosenberg's global conception of self-esteem is that this attitude of approval/disapproval must be expressed within a social context with implicit comparisons to the values of others (Mruk 1995, pp. 13–14). Analogous to the way Durkheim argued that the causes of the most individual action—suicide—had a social basis, Rosenberg's conceptualization argues that social relations are at the root of the most individual feelings about one's self: self-esteem.

As the central evaluative component of the self, self-esteem is considered an individual attribute (Coopersmith 1967; Rosenberg 1965; Wylie 1979), but researchers have recognized that self-esteem is created and molded through social interaction between individuals and the institutions and relationships in which they are imbedded. For example, children with a better developed social self will likely have more positive relationships with others, thus having higher global self-esteem.

Yet if a child is to have a social self capable of interacting with others, he or she must develop this social self, a process that begins at birth and occurs through social interaction and stimulation. This argument can be

found in some of the earliest social psychological theories. Mead's (1934, p. 135) premise was that "the self is something which has a development; it is not initially there, at birth, but arises in the process of social experience and activity." And in his theories of primary groups, Cooley also stressed shared social activity as a crucial prerequisite to the development of the self. Primary groups are social groups "characterized by intimate face-to-face association and cooperation," groups that are "fundamental in forming the social nature and ideals of the individual" (Cooley 1909, p. 23).

Because a child's primary social role is that of a family member—especially during the early years of life—family integration is likely crucial to the child's development of self. When a parent is integrated into a family—that is, when the parent has a high level of family organized activities—the child is likely to be exposed to these activities as well. On the other hand, if a parent is not integrated into the family—not only working many hours outside the home in paid labor but also spending leisure and consumption activities with coworkers and others outside the family—the child is less likely to be exposed to important social interaction and stimulation. Our argument here is very similar to one proposed by Coleman (1990, p. 594), whose typology distinguishes primordial social structures (such as the family) from purposive social structures (such as firms, professional associations, and governments). Writes Coleman, "The adults from the purposive structure with whom a parent has reasonably strong relations seldom have occasion to know the child well. The role-differentiation of parents' lives means that their work-related activities seldom intersect with their family-related activities" (p. 594). When parents are integrated into the family, it has positive externalities for children's development; these externalities are less evident when the parents' activities take place outside the child's sphere of life (Coleman 1990).

Thus it is important that social activities and experiences happen within the child's social context. One crucial mechanism through which this can occur is the family integration of the parents. In fact, social interactions in the family may be the most important "springboard" or prerequisite from which the child can develop relationships outside the family. Cooley (1909, p. 26) wrote that the family—along with neighborhood groups—were by far the two most influential, two most important primary groups for the genesis of the self. These primary groups are the sources from which more elaborate social relations can develop (Cooley 1909, p. 27).

Furthermore, in a framework proposed by Parsons and Bales (1955, p. 54), the self—which Parsons and Bales call the structure of personality—differentiates into an increasingly complex system of social relationships. First, as a child, the self is exposed to the most simple relationships: mother-child and father-child relationships. The self becomes a more developed social being as it learns and participates in these relationships,

relationships that Parsons and Bales call "social objects." In this framework, the self is socialized through the process of "the internalization of social objects" (Parsons and Bales 1955, p. 55). The key point from Parsons and Bales's theory is that the self can develop more complex social relationships only after it has learned or "internalized" the simpler relationships (p. 117). The family, therefore, is the crucial starting point for the development of a properly adjusted and mature social self. According to Parsons and Bales (1955, p. 181), if the young self experiences "malintegration" within early familial relationships, it can prevent more complex and differentiated relationships from forming later thus leading to the "disorganization" or "pathology" of the self (p. 243).

Yet the premise that socialization must progress from the simple to the complex, from the family of origin to the larger society, is not unique to Parsons and Bales's framework; it is a well-developed principle in other fields, particularly in the field of human and child development. Consider the striking similarities between Parsons and Bales and the work of Urie Bronfenbrenner: "In order to develop—intellectually, socially, and morally—a child requires participation in progressively more complex reciprocal activity, on a regular basis over an extended period in the child's life" (Bronfenbrenner 1990, p. 29). Bronfenbrenner (1990, p. 33) also writes that informal education and socialization in the family early in life is a "powerful prerequisite" of the child's subsequent social success in other domains of life, including school, work, and family. And similar to Mead's (1934, pp. 173–78) writings on the development of the "I" and the "Me," Piaget ([1927] 1977, p. 137) stressed that social interaction throughout childhood is needed so that the child can recognize his individual self as distinct from others, a recognition that is necessary if the child is to have successful relations with others in later childhood and adulthood.

In summary, a diverse group of theorists from both the sociological tradition and a developmental perspective emphasize how the formation of the self is aided by interaction in early childhood, especially interaction that takes place within the family. To develop a healthy self capable of participating in a wide variety of social interaction, a child must first receive in the family a rich socialization. This early socialization within the family lays the foundations on which more complex social relations can develop outside the child's family. When parents are integrated into the family, children are likely to be exposed to these social interactions and experiences.

Empirical Predictions

To use the family mode of organization framework to generate empirical predictions, it requires that we develop hypotheses that are specific to

both the outcome studied and the context in which the process occurs (Thornton and Lin 1994; Thornton and Fricke 1987). Our hypotheses pertain to the development of self among children in the United States in the latter half of the 20th century. We hypothesize a number of ways by which family integration positively affects children's development of self.

Parental activities within the home are a path to family integration that is likely to affect children. These include family eating or playing games together. Important activities for the development of the self are those that bring parents and children together, such as when parents help with homework, encourage creative activities involving arts and crafts, or enlist children's help in household chores and duties. These activities are likely to both stimulate a child's cognitive and social development and build a sense of accomplishment and self-worth, factors that should lead to higher self-esteem in childhood and beyond.

Because it is the primary location of a child's early life, aspects of daily family routine may be especially important to the development of the child's self. In their report on family change, Ogburn and Tibbitts (1933, p. 664) observed that many of the family's economic activities had been lost or were in the process of being transferred outside the family home. They mention the production of food as one of the changes that could alter the relationships of household members: instead of canning and producing food at home, families increasingly purchased food or ate out (Ogburn and Tibbitts 1933, p. 665). In particular, canning is a home activity to which all family members can contribute: although the adults must do the cooking and sealing of the food, small children can participate in the picking or washing of fruits and vegetables. Placed within Cooley's framework, canning is a cooperative activity that takes place within the primary group, a face-to-face activity that, theoretically, should aid the formation of the self. And empirical evidence also finds that home canning and positive self-evaluations are linked. In a study of children's attitudes toward home production, Dickins, Ferguson, and Fanelli (1954, p. 27) found that children enjoyed home canning activities because of feelings of pride, ownership, and accomplishment. Another effect of home canning was the children's "belief that one is needed, that one is helpful; helpful to one's parents, to one's associates, to others" (pp. 32-33). Home canning is time spent in the household, time shared with children, and, more important, time clearly devoted to the well-being of family members: it is likely a mechanism of family integration.²

Family integration is also likely to affect children when parents organize their leisure, socializing, and recreation activities among family and

² We discuss the historical specificity of canning as a mechanism of family integration in the home in greater detail below.

extended family. For a child to have a positive self-image, he or she must feel that others are interested in him or her (Rosenberg 1965, p. 142). Previous research has found that parental support or communication is positively related to adolescent self-esteem (Gecas and Schwalbe 1986; Demo et al. 1987). Similarly, we expect that extended family activities also make the child feel that he or she is part of a caring group, a group that is interested in his or her well-being. These feelings of group solidarity subsequently lead to a healthy self, which we expect to be revealed in higher self-esteem. This hypothesis parallels Durkheim's argument that family density and contact lead to a sense of integration and unity, which is crucial for preventing anomie and the destruction of the self (1951, pp. 201–2). More specifically, children will benefit from the closure that develops when the child and his or her parents spend time with relatives: a network of adults who are interested in his or her well-being, a network of overlapping and reinforcing care and concern.

Parents may also be integrated into families when they receive or exchange support with relatives. This, too, we expect will positively affect children's self-esteem. Intergenerational exchange of support (e.g., money, labor, gifts) is clear evidence that both the receiving and giving family are integrated within a strong, collective family network. In regards to children's well-being, it is not necessarily the material objects themselves that are the hypothesized antecedents of higher self-esteem but the truly supportive, extended family environment and collective feelings, which give rise to the assistance. Family support may also indirectly affect child well-being. Bronfenbrenner (1986, p. 730) notes several studies that have shown that mothers who received higher levels of support were more responsive to their children and had more positive attitudes toward themselves and their babies.

We have hypothesized several reasons why these various dimensions of family integration would have positive effects on children's development of self. There are also reasons to believe, however, that family integration may not always have positive effects on individual outcomes. Furthermore, the level of family integration and the nature of effects may vary across different groups.

Reasons for potential negative effects of family integration are similar to those suggested by scholars who have discussed the possible negative effects of social capital (Portes and Landolt 1996). Negative effects are suggested by two possibilities: (1) too much or hyperintegration into family relationships so that interaction within other forms of social organization (e.g., workplace, nonkin, school) are impossible and (2) integration into family relationships that are destructive or socially pathological. The first possibility calls to mind the literature on family "crowding," which suggested that not only excessive physical but also psychological closeness of

family members could be detrimental (Gove, Hughes, and Galle 1979). In our framework, we might expect a curvilinear or upside-down U-shaped relationship: the benefits of family integration may increase until a point where individuals become too involved with family to the point of exclusion from other domains of social life, at which point the benefits decrease or even become negative. The second possibility suggests an interaction between family integration and other family characteristics: for example, if one is integrated into and has his or her life organized around a family that is abusive or affected by alcoholism or substance abuse, it would have worse effects than being integrated into a healthy family; it may even be worse than not being integrated into a family at all.

Another important issue is how family integration is a consequence not only of the family's social structure and its relationships outside the household but a consequence of gender relations of family members. In other words, to treat the family as a unified whole is to obscure the gender-specific relationships that are at the family's base (Hartmann 1981; Perry-Jenkins 1994), relationships that also are potentially at the base of some mechanisms of family integration. Di Leonardo (1987) and Bourdieu (1996) note that the mechanisms of integration or holding the family together are most often performed by women, who bear the majority of responsibility for visiting relatives and keeping correspondence. The benefits that family integration provides to members in a given family, therefore, may be partly a consequence of gender. This premise holds two concerns for the study of family integration.

The first is a possible interaction effect, a differential effect on male and female children. That is, family integration may benefit males but not females because it could be the males that reap the benefits of the integrative work, which is most commonly done by females. Boys in integrated families may feel the warmth and support, which increases their self-esteem, while girls in such families may receive the message that their place in life is to be the supporters and caregivers at the expense of their own instrumental goals. For example, Elder's (1974) work on the Great Depression found that when families became economically deprived and mothers sought paid labor outside the home, daughters were forced into more extensive household labor, which subsequently may have exposed them to greater family conflict and discord (Elder, Nguyen, and Caspi 1985).

The second issue is that family integration may be the result of gender struggles in the family. Family integration, which often falls on the responsibility of women, may be more likely to occur in families where women are prevented from pursuing activities in other domains. In this case, it would be important for an analysis to control for the family's

decision-making structure or some other measure of the family's distribution of power between male and female household members.

Finally, our hypotheses have considered only activities and relationships that are exclusively related to family: social interaction and exchange of aid with family. An alternative hypothesis is that the benefits of family integration derive not from the family nature of the activities but the activities themselves, no matter what their source (family or nonfamily). To address this issue, we include an important hypothesis test—what Stinchcombe (1968, pp. 24–28) calls a “crucial” test—to contrast social activities derived from family versus nonfamily relationships. We expect that when parents spend social time with nonfamily (such as nonkin friends), this “nonfamily integration” will not affect children's self-esteem. Unlike socializing with family, nonfamily socializing is less likely to involve close affective ties, intergenerational closure, and positive externalities for children (Coleman 1990).

The scope of this paper is limited to how parents' family integration affects their children's development of self and subsequent self-esteem, yet family integration may influence many other outcomes. For example, the transmission of attitudes from parents to children may depend on the level of family integration of both parents and children. We would expect children's attitudes to resemble their parents more so when there is high family integration. Similarly, the transmission of parental characteristics, such as religiosity or education, may also depend on the family members' degree of integration.³ Our reasoning here is similar to Coleman's, who predicted an interaction effect between parents' human capital and social capital in the family on children's outcomes (Coleman 1988, p. S110). Children's self-esteem, however, is a special case where we expect an additive effect of parental family integration.

DATA AND METHODS

The data for these analyses come from a 23-year, seven-wave panel study of mothers and their children. Mothers were originally selected using a systematic probability sample of 1961 birth records from the Detroit Met-

³ The transmission of social class is another domain in which family integration might be useful. In our paper, however, we do not develop a theory of how the effects of family integration on self-esteem vary by social class. A theory on the differential effects of family integration by social class is likely to be most useful with other outcomes, such as educational attainment and occupational prestige. The findings on even the bivariate relationship between self-esteem and social class remain mixed (Gecas and Seff 1990), and an interactive theory of social class, family integration, and self-esteem would be even less clear.

ropolitan Area (Wayne, Oakland, and Macomb counties). The study chose approximately equal numbers of married white women who had given birth to a first, second, or fourth child, and the mothers were interviewed beginning in the winter of 1962. Those women were reinterviewed in the fall of 1962 and again in 1963, 1966, 1977, 1980, and 1985. The children born in 1961 were interviewed in 1980, at age 18, and again in 1985, at age 23. Our analysis sample is 913 of these children interviewed in 1985 and their mothers.⁴

Although the data represent a birth cohort from one metropolitan area, the data are extraordinarily valuable for examining the effects of family integration on self-esteem as a process over time for several reasons. First, although all children were born in the same area, by 1985 about half lived outside the Detroit metropolitan area, and both the experiences and attitudes of this sample closely resemble those of other national samples of the white population (Thornton and Axinn 1996). Second, the original sample included only married women who had given birth to a first, second, or fourth child, but during this time in the United States—the baby boom—most women in marriages were having children, having children outside of marriage was rare, and divorce was not nearly as common as today. Although restricting the initial sample to women who were married and had just given birth would be an important limitation in a contemporary study of children's family experiences, such restrictions do not negatively affect our sample to a high degree given the historical circumstances. Third, a single birth cohort greatly enhances our ability to study the effects of family integration over time; because all the children are the same age, it reduces the chance that our results are affected by unobserved age or period effects. Finally, the absence of nonwhites from the study does not permit us to generalize to this population. It is possible that both the level of family integration and the process itself varies among non-white populations.

⁴ The original 1962 survey had a response rate of 92%, yielding 1,113 cases, and subsequent waves have had high rates of respondent cooperation. Of the 1,113 families interviewed in 1962, 923 of the children were interviewed in 1985, representing a retention rate of 83%, and 913 of these cases had no missing data on the needed variables. In table A1 in the appendix, we compare the analysis sample with the original 1962 sample. The comparisons are based on a simple two-sample *t*-test for a difference of means, equal variance assumption. Overall, the samples are not significantly different from each other on nearly all of the variables collected in 1962. One exception is that the mothers in the analysis sample had significantly more education. Given the similarity between the two samples, the absence of cases due to item missing data and attrition by 1985 should exert minimal influence on our overall results (we control for mother's 1962 education in our multivariate models).

Young Adults' Self-Esteem

Our measure of young adults' self-esteem is the combined responses to seven questions, five of which are from the Rosenberg Self-Esteem Scale (Rosenberg 1965). In 1985, each young person (age 23) responded to the following items using the responses "always," "almost always," "often," "sometimes," "seldom," or "never." Questions directly from the Rosenberg scale are marked with an "R."

1. I take a positive attitude toward myself. (R)
2. I feel I do *not* have much to be proud of. (R)
3. I am able to do things as well as most other people. (R)
4. I feel that I can't do anything right.
5. As a person I do a good job these days.
6. I feel that I have a number of good qualities. (R)
7. I feel that I'm a person of worth, at least on an equal level with others. (R)

Answers to the questions were coded 1–6, with 6 indicating high levels of self-esteem. Thus, for questions 1, 3, 5, 6, and 7, "always" is coded "6," and "never" is coded "1." Questions 2 and 4 are coded so that "always" = 1, and "never" = 6. To form the self-esteem scale used in these analyses, the seven responses to these questions are averaged, allowing for missing values on up to two questions. The resulting scale has virtually the same measurement properties as the Rosenberg Self-Esteem Scale, which is the most widely used measure of self-esteem in the sociological literature (e.g., Whitbeck et al. 1991; Rosenberg and Pearlin 1978; Oates 1997; Hughes and Demo 1989; Bachman and O'Malley 1984; Rosenberg et al. 1995; Jussim, Coleman, and Nassau 1987; Robertson and Simons 1989). Cronbach's alpha for this scale is .83. In table 1, we present the means, standard deviations, minimums, and maximums of this self-esteem scale and the other variables.

Family Integration

All measures of parental family integration are from questions asked of the mother on the original winter 1962 survey and pertain to when the child was less than one year old. Because it is unlikely that the family integration measures were a response to the young baby's specific characteristics, we can be confident that these measures are exogenous. It may be possible, however, that simply having a baby evinces a set of changes in parental behavior patterns that are favorable to the development of family integration. For example, Munch, McPherson, and Smith-Lovin

TABLE 1
DESCRIPTIVE STATISTICS FOR ANALYSIS SAMPLE

	Mean	SD	Minimum	Maximum
Self-esteem, 1985 (child age 23)	4.97	.63	2 14	6
Home production, 1962 (child age 6 months)70	.73	0	3
Family social networks, 1962 (child age 6 months)	2.01	.75	4	3 9
Family support networks, 1962 (child age 6 months)	1.08	.88	0	4
Nonfamily social networks, 1962 (child age 6 months)	2.16	.80	0	4
Child's sex (female = 1)50	.50	0	1
Child's parity	2.30	1.25	1	4
Child's education, 1985	13.60	2.01	6	17
Father's education, 1962	12.41	2.51	3	16
Mother's education, 1962	12 17	1.85	3	16
Mother's age, 1962	25 53	4.85	16	39
Mother/father decision-making structure, 1962	2.05	.63	0	4
Mother/father division of labor, 1962	2.58	.54	4	4
Family income, 1961	\$6,850	\$2,950	\$500	\$15,000
Parents' combined number of siblings	6.04	3.54	0	18
Mother's employment 196210	.30	0	1
Parents ever divorced, 1962-8516	.37	0	1

NOTE.—*N* = 913

(1997) reported that parents' social networks change upon the birth of a child, and their networks vary as the child ages. But in our data, all families had just had a birth of a child (and we control for the parity of that child's birth). If simply having a baby affects the development of family integration, then all families in our study experienced this effect. The effects of variance in family integration among these families are net of any possible effect of simply having a child. Thus the measures allow us to evaluate the long-term impact of family integration on children's subsequent self-esteem.

We measure three dimensions of family integration, dimensions that we call home production, family social networks, and family support networks. Home production is measured by two questions. The first question measures the extent of food produced in the home, and it asked, "How many of the canned and frozen foods you eat are put up at home?" The mother could respond "more than half," "less than half," "just a few," or "none." The responses were coded 3-0, where higher values represent a higher incidence of home canning and freezing.

The second measure of home production is homemade clothing. Along with food preparation, Ogburn and Nimkoff (1955, p. 128) noted decreased production of clothing within the home as another activity that altered the family's economic and productive organization. Although making clothes is not an activity in which young children can participate, it is an activity, like home canning and freezing, that indicates a family mode of organization and increased opportunities for interaction between parents and children. The production of clothes that the family will wear is a substantial activity, more substantial than occasional mending or sewing. It is likely to involve many family members, especially female family members and children when they become old enough.

The question on homemade clothing asked, "How many of the blouses, dresses, or sweaters you and the children (or baby) have were made at home?" The response categories were "more than half," "less than half," "just a few," or "none." The responses were coded 3–0, where higher values represent more home production of clothing. Because both questions—home canning and freezing and homemade clothing—are part of our concept of home production and have the same metric, we average the responses to both questions to form the measure we call home production. This measure takes values from a low of 0 to a high of 3.

The second dimension of family integration is family social networks, which represents family organization of social activities and an opportunity for family interaction. These extended family social networks are measured by two questions. The first asks, "Do your relatives or your husband's relatives have large family gatherings in which you participate on birthdays, anniversaries, holidays, or any other time?" Each mother was asked how often she participated in such gatherings. The possible responses were "once a month or more," "several times a year," "less often," or "never." The responses were coded 3–0, where higher values represent higher family social contact. The second question of family social networks inquired, "Now, of all your married friends and relatives, would you think of the three married women near your own age that you feel closest to and know best." For each person mentioned, the mother was asked if that person was a friend or a relative. This measure was coded as a proportion and varied from a low of 0 (none of the mother's three closest married friends are relatives) to a high of 1 (100% of the mother's three closest married friends are relatives), with various fractions in between. Because both measures capture the same concept but use different metrics, we standardized each variable (to mean = 2; SD = 1) and then averaged them. This created a single measure of family social networks.

The third dimension of family integration is family support networks. Whereas our measure of family social networks simply assesses the incidence and likelihood of extended family interaction, family support net-

works capture the tangible aid and benefits exchanged between families. The four questions of family support networks measure different kinds of resources, help, and the ends they may achieve. The first question asked the mother about financial help: "Have you and your husband received large money gifts from parents or other relatives since you were married?" Responses of yes were coded "1," and no responses were coded "0." The second and third questions inquired into other kinds of help the family received from parents or relatives in the last year. The mother was asked if the family received "advice on money or business matters" or "help in getting a job." Following the previous coding scheme, yes responses for these two questions were coded "1," and no responses were coded "0." The fourth measure of family support networks asked, "During the first week or so, when you were home with the baby, did you have someone besides your husband to take care of the family and the house?" If any relatives helped, or if relatives helped along with any combination of nonrelatives (friends or paid help), then this variable was coded "1." If no relatives were mentioned, or if she received no help from any sources, then this variable was coded "0."

Like the home production concept, the family support networks concept is composed of multiple constructs with identical metrics. Therefore, we add together the individual questions to form a single measure. The result, a variable we call family support networks, ranges from a low of 0, in which the family received no forms of help, to a high of 4, in which the family received all four kinds of help.

The hypothesis test of nonfamily integration—in which we predict no effect on children's self-esteem—is measured by a question that asks, "How often do you visit with people other than relatives, to go out, or just get together at your home or theirs?" The response categories were "almost every day," "once or twice a week," "once or twice a month," "a few times a year," and "never." They were recoded from 4–0, where higher values represent more frequent social interactions with nonrelatives.

Family Integration Measures in Historical Perspective

We acknowledge that our measure of home production is limited to a specific time in history, and though an excellent measure of integration in 1962, such a measure might be inappropriate today. Time series data on home production are rare (Caplow et al. 1991), yet trend data on households that practice vegetable gardening—often a prerequisite for canning—are available since 1975. As expected, the incidence of vegetable gardening among households has decreased, from 49% in 1975 to 26% in 1993 (Caplow et al. 1991; U.S. Bureau of the Census 1995). Canning and home production of clothing are likely to have decreased even more, espe-

cially from the levels in 1962. Given these historical trends, a more appropriate contemporary measure of family integration within the home would probably include activities such as how often the family eats together, how often these meals are eaten at home, or how many minutes per day family members spend talking to each other.

Our measures of family social networks and family support networks, however, would probably be equally as appropriate today as they were in 1962. Both the frequency of contact and the incidence of intergenerational support appear stable since the 1970s when reliable trend data became available (Caplow et al. 1991). General Social Survey data from 1974 through 1994 show no change in how often Americans "spend a social evening with relatives." Similarly, comparing several national studies of intergenerational aid reveals little change in the incidence of support between 1957, 1962, and 1989 (Caplow et al. 1991). Of course, there is wide variation among subgroups in the levels, incidence, and frequency of exchanges (Hogan, Eggebeen, and Clogg 1993) and social interactions (Silverstein and Bengtson 1997), but there is no reason to expect that our measures do not capture dimensions of family integration that are just as important in America today as they were in 1962.

Controls

We include a variety of controls. Some concern individual characteristics of family members; others concern aspects of the family or household's background that may influence how the family and its interactions are organized.

Individual characteristics.—The child's sex is a control because some research has suggested that females may have lower self-esteem than males (Simmons and Rosenberg 1975; Bush et al. 1977–78), and child's sex may be correlated with some of the family integration measures. To test for possible sex differences in the effects of family integration, we investigate interaction models between the child's sex and the family integration measures. The child's parity is an important control because birth order has been shown to affect how parents raise their children (Kidwell 1982). We control for parity with a set of dummy variables to contrast first, second, and fourth parity children.

Previous research has shown that parental education has significant positive effects on children's self-esteem (Axinn, Duncan, and Thornton 1997; O'Malley and Bachman 1979; Bachman and O'Malley 1977). Parental education might also be correlated with the family integration measures. Some research suggests that better-educated parents devote more time to child care and subsequently invest more in their children's human capital (Leibowitz 1974, p. 249). Also consistently reported is that people

with more education have higher self-esteem (Bachman and O'Malley 1977). We therefore include as controls the mother's, father's, and child's education. The parents' education was measured during the original 1962 interview and is coded in years of education completed. The child's education is included as a potential intervening variable; it was measured at age 23 during the 1985 interview and is also coded in years completed.

We include the mother's age as a control because the family integration measures may reflect a parent's position in the life course. It may be that family integration is higher among those parents in the younger, family building years. Mother's employment is an important control because mothers who work outside the home may be less likely to be involved with family integration activities. We control for the mother's employment in 1962 with a dichotomous variable that is coded "1" if she was employed, "0" otherwise.

Household characteristics.—We control for family income because some family integration measures have an implicit economic context.⁵ Home production of food or clothing, receipt of large money gifts, and household assistance from relatives may reflect the family's economic circumstances. For example, if a family is struggling to meet daily necessities, then receiving support from relatives could bring strain to these support relationships: dependency, guilt, or even bitter feelings toward the giving party. In some cases, research suggests that support can be a source of antagonism as well as amelioration, conflict as well as help (Cramer, Riley, and Kiger 1991; Belle 1982; Revenson et al. 1991). This possibility would suggest an interaction between support and family income, in which the effects of support on self-esteem were less beneficial for children in needy families. And although recent research has failed to find evidence of persistent relationships between parental resources and children's self-esteem (Axinn, Duncan, and Thornton 1997), other research has found links between family economic hardship and self-esteem (Whitbeck et al. 1991; Wiltfang and Scarbecz 1990).

We recognize that our family integration measures may represent the gendered relations of household members. We therefore include as con-

⁵ Our measure of family income is from the 1962 survey, which asked the mother what the household's income was in the previous year (1961) to the nearest thousand: under \$1,000, \$1,000–\$1,999, \$2,000–\$2,999, and so on, until the last two categories of \$10,000–\$14,999 and \$15,000 and above. To use these categories in an analysis, we coded each category to its median (except for the last category, which remained \$15,000), and then took the natural logarithm. These income categories seem low, but the reader must remember that in 1960 the median family income among whites was about \$5,800 (U.S. Bureau of the Census 1980).

trols two scales, one that tries to measure the couple's decision-making structure and a second that attempts to capture the division of labor between the mother and father.⁶

The frequency of family interaction and amount of exchanges with extended family is likely to be affected by the quantity of relatives (Logan and Spitze 1994). To control for this possibility, we include a variable measured in 1962 that represents the quantity of relatives: the combined number of siblings the mother and father had at that time.

We control for divorce because it is likely that if a family experiences a divorce, the family's social organization will change—in particular, the frequency of interactions with extended family may be altered. The amount and frequency of support may increase, because a single parent may appear to be more in need than before. We include divorce as a possible intervening mechanism, and it is coded as a dichotomous measure that is 1 if the child's family experienced a divorce between 1962 and 1985, 0 otherwise.

Model Estimation

Consistent with previous research on self-esteem (e.g., Axinn, Barber, and Thornton 1998; Hughes and Demo 1989; Bachman, O'Malley, and Johnston 1978), we treat the ordinal self-esteem scale as an interval-level variable. To estimate our multivariate models of young adult's self-esteem, we use ordinary least squares regression. We estimate the effects of the 1962 family integration measures and controls on the children's 1985 self-esteem.

⁶ The scale measuring the decision-making structure comes from a series of questions that asked, "In most families either the husband or the wife has the most say about some decisions although they talk it over first." The mother was asked who usually has the most say in six areas: which couples they see and visit, how much should be spent on major purchases, which relatives they see, how much money should be spent on the children, how often they go out for an evening, and who is likely to have the most say about the children's responsibilities and habits. These responses were coded 0–4, where 4 represented "wife almost always." These six questions were averaged to estimate the couple's decision-making structure. The scale measuring the couple's division of labor comes from five questions that asked, "We would like to know how you and your husband divide up some family jobs." The mother was asked who usually does the following activities: grocery shopping, evening dishes, repairs around the house, putting the children to bed, straightening up before company comes over. Like the decision-making questions, the responses were "husband almost always," "husband mostly," "husband and wife exactly the same," "wife mostly," and "wife almost always." These responses were coded 0–4, where 4 represented "wife almost always," and the five questions were averaged to form a single scale of the couple's division of labor.

RESULTS

Recall that our measures of family integration come from 1962, when the focal child was less than age 1; our measure of the child's self-esteem comes from 1985, when the focal child was age 23. Thus we are confident that the measures of family integration are exogenous to the child's self-esteem, and the models we estimate represent tests of the effects of family integration on the child's self-esteem.⁷

Table 2 displays the results of these tests. The first column of table 2 shows that children's early adult self-esteem benefits from parental integration: family food and clothing production early in the child's life has a positive effect on self-esteem. This finding supports our hypothesis predicting that when parents are integrated into household activities in childhood, children's long-term self-esteem is increased. The fact that we find a positive association with self-esteem at age 23 indicates that this effect persists well into early adulthood.

The second column of table 2 displays the results for our test of the effect of parental family social networks in childhood on early adult self-esteem. As argued above, family social networks are an important dimension of family integration because they increase family solidarity, benefit a child's social development, and provide intergenerational closure. We expected higher levels of extended family social interaction in childhood to produce higher self-esteem among children in the long run, and the results confirm this hypothesis: family social networks have a positive and significant effect on children's self-esteem.

The third column of table 2 displays our estimate of the effect of parental family support networks on children's young adult self-esteem. Once again, we find that family integration—this time in the form of support networks—has a significant positive impact on children's self-esteem. This long-term effect of family support networks is particularly meaningful because these supportive exchanges are processes that reaffirm family social networks, and they integrate both the giving and receiving family members closer together. Often assumed to be important factors facilitating the intergenerational transmission of status (e.g., Bourdieu 1986, 1996), our results suggest that family integration also affects the development of the self. The results, therefore, strongly support our hypothesis that higher levels of family integration in early childhood have a positive impact on children's self-esteem. The fact that this effect lasts into early adulthood, 23 years after our measures of family support, strengthens our

⁷ Of course, as in most social science analyses, it is possible that both family integration and children's self-esteem are determined by other unmeasured factors.

conclusions that family integration is a persistent force in the long-term development of the self.

The fourth column of table 2 shows that nonfamily social networks do not have a significant effect on self-esteem of young adults. This result is consistent with our hypothesis that nonfamily social networks are less important to children's self-esteem. We believe these nonfamily social networks are not as relevant to children's self-esteem because they are less likely than family social networks to be part of the children's social world.

We want to note that family integration in 1962 probably did not directly affect the child's self-esteem in 1985, but it was likely part of a pattern that existed throughout childhood and adolescence. It is this long-term pattern of family integration—the beginning of which is measured by our data—that directly affects the child's development and subsequent self-esteem. Ideally, a study would measure both family integration and the child's self-concept at repeated points over time, creating streams of measurement.

As depicted in figure 1, our study has measured only the beginning and end of this process (the upper-left and lower-right boxes). Capturing family integration at the very beginning of the child's life has given us great purchase on its exogeneity in reference to children's self-esteem, yet the cost is that the amount of variance explained in the models is relatively small. If dimensions of family integration were measured in a continuous stream throughout the child's life course, then the family integration measures would likely explain more variance (of course, issues of endogeneity then become much more serious and harder to untangle).

Of the controls used in our multivariate models, many of their effects are consistent with those found in previous research. Female children had lower self-esteem scores than male children (Axinn et al. 1998; Simmons and Rosenberg 1975; Bush et al. 1977–78), and second parity children had higher self-esteem scores, as did those children who had achieved more education by age 23. Father's education also had a positive effect on self-esteem, which corroborates previous findings (Axinn et al. 1997; O'Malley and Bachman 1979). In two models (cols. 1 and 4), children of older mothers had slightly lower self-esteem than children of younger mothers.

None of the other controls has statistically significant effects. We failed to find that family income in childhood significantly affected self-esteem in early adulthood. This latter finding is important because it suggests that family income cannot explain the impact of the specific dimensions of family integration used in our analysis. In addition, the interaction term between family support and income was not significant (tables not shown). Thus there is no evidence in our study to support the hypothesis that

TABLE 2
TESTS OF HYPOTHESES OF FAMILY INTEGRATION AND SELF-ESTEEM

	1	2	3	4	5
Family integration:					
Home production06* (2.12)				.06* (2.12)
Family social networks06* (2.31)			.05* (1.90)
Family support networks07** (2.89)		.07** (2.76)
Nonfamily integration:					
Nonfamily social networks				-.02 (-.67)	-.01 (-.28)
Controls:					
Child's sex (female = 1)	-.31*** (-5.26)	-.22*** (-5.33)	-.21*** (-5.28)	-.21*** (-5.24)	-.22*** (-5.37)
Parity 2 (parity 1 is reference)09* (1.75)	.09* (1.80)	.10* (1.86)	.09* (1.75)	.09* (1.83)
Parity 4 (parity 1 is reference)01 (.15)	.02 (.26)	.02 (.36)	.01 (.18)	.02 (.36)
Child's education, 198505*** (4.16)	.05*** (4.10)	.05*** (4.08)	.05*** (4.13)	.05*** (3.99)
Father's education, 196203** (2.64)	.03** (2.76)	.03** (2.72)	.03** (2.74)	.03** (2.76)

Mother's education, 1962	-.01 (-1.01)	-01 (-1.01)	-02 (-1.23)	-01 (-1.26)
Mother's age, 1962	-.01*	-01 (-1.52)	-01 (-1.16)	-01* (-1.23)
Mother/father decision-making structure, 1962	.03 (1.06)	.03 (1.02)	.04 (1.13)	.03 (.86)
Mother/father division of labor, 1962	-.01 (-.32)	-01 (-1.17)	-01 (-.23)	-01 (-1.12)
Family income, 1961, logged	-.01 (-.14)	00 (-.03)	.02 (.38)	.02 (.42)
Combined number of siblings, mother and father	.01 (1.17)	.01 (1.03)	.01 (1.48)	.01 (1.34)
Mother's employment, 1962	-.05 (-.75)	-.04 (-.61)	-.05 (-.69)	-.03 (-.49)
Parents ever divorced, 1962-85	-.03 (-.53)	-.02 (-.39)	-.03 (-.51)	-.03 (-.54)
Intercept	4.37*** (11.59)	4.19*** (10.94)	4.07*** (10.43)	3.95*** (9.93)
Model fit:				
N	913	913	913	913
Adjusted R ²	.06	.06	.07	.07

NOTE —t-statistics are in parentheses.

* p < .05, one-tailed tests

** p < .01

*** p < .001.

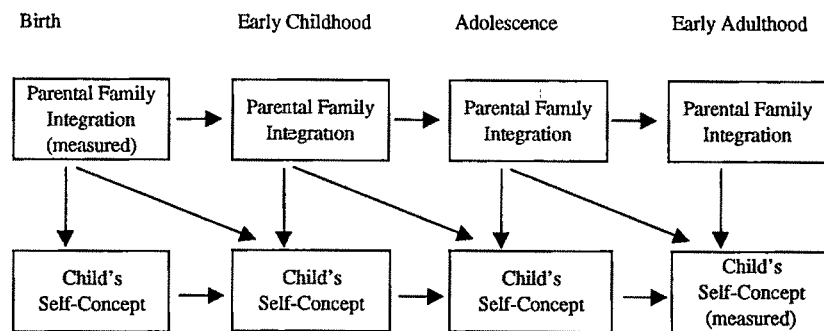


FIG. 1.—Model of family integration and child's self-concept over time

family support to economically needy families is less beneficial than support to children in other families.

Independence of Effects

Our dimensions of family integration may be conceptually distinct, but they are certainly related. The results displayed in the fifth column of table 2 show that these three dimensions—home production, family social networks, and family support networks—have independent effects on children's early adult self-esteem. Although these three dimensions of family integration may be interrelated, each dimension has its own specific long-term consequence for children's self-esteem. This finding is consistent with the conclusion that not only does family integration have long-term effects on children's self-esteem, but family integration affects children through multiple dimensions that are distinctly different.

Variations in the Effects

We next summarize some results from tests of hypotheses regarding how the effects of family integration can vary. First, having too much integration into family structures may reduce self-esteem. The benefits of family integration may increase steadily until a certain point, after which the benefits level off or even become negative. We tested this hypothesis by incorporating a quadratic term in which each measure of family integration was squared, and a negative coefficient was predicted for these terms. In the resulting analysis (tables not shown), none of the squared terms were significant, suggesting no evidence of a quadratic trend in the effects of family integration on children's self-esteem.

The second way in which family integration could vary is by what could

be called family characteristics. That is, integration into an abusive or pathological family could have negative effects, especially for child outcomes such as self-esteem. This hypothesis would be tested with an interaction between the family integration measures and a separate indicator of family well-being: perhaps a measure of domestic conflicts or a measure of family members' coercive parenting styles (Conger et al. 1994). Unfortunately, the information to specify this model of long-term consequences for children is not currently available. We believe that the coefficient for these tests would be negative, though we suspect that finding significant results would be difficult, even if the measures existed. For example, if it is unlikely that abusive or destructive family members would engage in family integration activities, then estimating an observable interaction effect would be difficult.

The third way family integration could vary is through gender effects. We theorized that gender could influence family integration and self-esteem through two pathways: first, there could be a spurious relationship between family integration and self-esteem if gendered relations among household members were the common cause of both. We controlled for this possibility by including measures of the division of labor and decision-making structure of the parental home (these control measures had no significant effect). The second way in which gender could influence family integration is through a differential effect on male and female children. We tested this hypothesis with an interaction between the family integration measures and the sex of the child. The analysis provides little evidence to support a differential effect by sex. Only one of the three interactions was statistically significant (tables not shown): the interaction between family support networks and sex was significant and negative. This means that female self-esteem benefited significantly less from family support networks than male self-esteem. Neither the home production nor the social networks interactions with sex were statistically significant (tables not shown).

DISCUSSION

This paper is a specific case of the more general sociological effort to discover the impact of larger social structures on the well-being of individuals imbedded in those structures (Coleman 1993). In this case, we have used the modes of social organization approach to examine how family integration affects the self-esteem of children. By carefully considering this framework, we identify several aspects of family integration that shape children's self-esteem: parental activities within the home, family social networks, and family support networks.

Our empirical analyses revealed that measures of these aspects of family

integration have a significant, positive effect on children's subsequent self-esteem. These results confirm our hypothesis that family integration in early childhood benefits children's later self-esteem. These results are consistent not only with sociological theories of social interaction and the self (Cooley 1909; Mead 1934; Parsons and Bales 1955) but also with more recent child development research. Cochran and Brassard (1979, pp. 603-5) hypothesized that parental social networks could stimulate children's cognitive and social development and improve adults' parenting quality, and Melson, Ladd, and Hsu (1993) found a positive effect of these networks on children's cognitive performance. Our results, coupled with the child developmental literature, suggest two mechanisms that may link family integration to children's subsequent self-esteem. Family integration early in childhood may benefit development of the self early in life, which persists through early adulthood. Or, family integration early in life may help to improve parenting quality, improvements that persist to influence the child's self-concept in later years.

Our analyses also demonstrated family integration's long-term consequences. Our measures of family integration were collected during the first year of the children's lives, and these measures had a significant impact on self-esteem at age 23. It is extremely important that a long interval separates our measures of family integration and our measures of children's self-esteem, as children's behaviors are very likely to affect parental behaviors and the family's organization (Belsky 1984; Furstenberg and Hughes 1995, p. 585). Thus, although a number of studies have linked aspects of family social organization (such as family social capital) to a variety of child outcomes (Furstenberg and Hughes 1995; Hagan, Merken, and Boehnke 1996; Parcel and Menaghan 1994), conclusions regarding the direction of causation remain tenuous: child behaviors are just as likely to have caused family organization as family organization is to have caused the child outcomes. One solution to this problem is to use longitudinal measures beginning very early in children's lives to examine the impact of family organization on child development (Parcel 1993; Parcel and Menaghan 1994). Most efforts using this design, however, have been limited to brief durations and therefore only early child outcomes. Our results extend this research across a much longer time span, into early adulthood. This allows us to document consequences of early childhood family integration that endure across long periods of time.

We find that the three different dimensions of family integration we examine each have significant, positive, and independent effects on children's self-esteem. This result suggests that family integration is a multifaceted concept, operating through multiple pathways to affect individual well-being. These three factors are conceptually separable dimensions of family integration: the organization of family activities within the home,

interactions with extended family members, and familial exchanges of support. Thus it is not surprising to find that their effects on children are independent.

Yet other dimensions of family integration, which were beyond the scope of the present analysis, may also have independent effects on children. These may include organizational aspects of family integration other than the organization of household activities, social networks, and support networks. In fact, other dimensions may include emotional and ideational aspects of family integration that are separable from its organizational aspects. Certainly, previous research on self-esteem indicates that the emotional and relational dimensions of the family have a significant impact on children's self-esteem (Demo et al. 1987; Gecas and Schwalbe 1986). But sociological research has also identified ideational aspects of family integration, such as familism, that have important consequences for other domains of social life (Goldscheider and Goldscheider 1993).

Furthermore, we theorized several ways by which the effects of family integration could vary: by amount, by family characteristics, and by gender. We empirically investigated two of these possibilities but found little support. The reader must keep in mind the long time span separating the independent measures and dependent outcomes. The effects might have been more perceptible had there been a larger sample, more precise measures, and a shorter time span between measures and outcomes.

More broadly, our results emphasize the importance of examining social organization or social structure—how collections of individuals organize their activities and resources—as a potential determinant of individual well-being. Our arguments add to those from several studies that assert social structure and social support may be critical determinants of individual well-being, particularly in terms of health (House, Umberson, and Landis 1988; Turner and Marino 1994; Umberson et al. 1996). We join this stream of research in advocating that social scientists look beyond simply the quality or quantity of relationships to examine the “structural properties that characterize a set of relationships” (House et al. 1988, p. 293). This approach is what Coleman (1993, p. 10) advocated when he argued that too many sociologists have failed to recognize the importance of the social organization of institutions as a powerful determinant of individual outcomes.

Our focus on family integration is motivated by the outcome we examine in this paper: the self-esteem of children. We argue that a focus on family social structure is appropriate for studies of many different domains of social life, particularly those that take place within the context of the family. The long-term determinants of children's self-esteem is one example, but the same strategy is likely to provide important insights into other child outcomes. In fact, family social structure is likely to have im-

portant consequences for many domains of family social life, including the quality of relationships, such as parent-child or husband-wife relationships. Other possible domains include processes of family formation and dissolution, such as marriage, childbearing, and divorce. Additional issues that may be influenced by family social structure even include trade-offs between family and other dimensions of social life, such as the decisions to stay in school or marry, to remain in the labor force or have children, or to maintain high levels of religious participation or enter premarital cohabitation. Overall, a focus on family social organization, specifically family integration, is likely to provide important new insights into many dimensions of family social life.

APPENDIX

TABLE A1
COMPARISON OF ANALYSIS SAMPLE TO ORIGINAL SAMPLE

	MEAN		T-STATISTIC ON DIFFERENCE
	Analysis Sample	Original Sample	
Home production, 1962	70	.67	.91
Family social networks, 1962	2.01	1.99	.67
Family support networks, 1962	1.08	1.08	.15
Child's sex (female = 1)50	.48	1.20
Child's parity	2.30	2.33	-.48
Father's education, 1962	12.41	12.22	1.62
Mother's education, 1962	12.17	11.99	2.13*
Mother's age, 1962	25.53	25.58	-.22
Mother/father decision making, 1962	2.05	2.02	1.02
Mother/father division of labor, 1962	2.58	2.59	-.05
Family income, 1961	\$6,850	\$6,697	1.17
Nonfamily social networks, 1962	2.16	2.13	.73
Parents' combined number of siblings	6.04	6.15	-.71
Mother's employment, 196210	.10	-.22

NOTE.—N = 913 for the analysis sample, N = 1,113 for the original sample.
* p < .05, two-tailed tests.

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Book Reviews

The Disenchantment of the World: A Political History of Religion. By Marcel Gauchet. Translated by Oscar Burge. Princeton, N.J.: Princeton University Press, 1998. Pp. xv+228. \$24.95

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Marcel Gauchet's *The Disenchantment of the World* has been published in a series called New French Thought, which might be read by some as an advertisement, by others as a warning. Both readings are correct. Gauchet, who is among the rather influential contemporary French social thinkers, offers a sweeping argument about the trajectory of religion. No, not any particular one, but religion in general. He argues that religion has undergone three major transformations; contrary to some philosophies of history this is not a progress report but deals instead with the "fall" of religion. The primary aim of this book is, however, not to debunk religion as a swindle, to celebrate its decline, or to simply repeat one of the many versions of the secularization theses. Gauchet rather engages in a very serious reflection on the question of what it means to live in the modern Western world. He argues that our relationship to the "world" is radically different from premodern and non-Western ones. In order to make us understand these differences he writes his "political history of religion." Unfolding his argument, Gauchet draws, sometimes explicitly, sometimes implicitly, on many familiar insights, from Max Weber's "disenchantment of the [Western] world" to Karl Jaspers's "axial age" thesis—appropriated most prominently by Shmuel Eisenstadt—to Thomas Luckmann's thesis of the privatization of religion. Although many of Gauchet's arguments are not at all new, he combines them into a new synthesis that deserves our attention.

What is Gauchet's understanding of religion? "At the core of the religious lies an implicit assumption that whatever causes and justifies the visible human sphere lies outside this sphere" (p. 33). This is most fully achieved in societies where this truth is not represented through human agents. This understanding of religion is only fully realized in the "early" stage of "primeval" religion. Here the past reigns. It is traditionalism at its best, a holistic universe where everything is taken for granted and governed by myth. "Religion in its pure state is drawn into a temporal division that puts the present in a position of absolute dependence on the mythical past, and guarantees the irrevocable allegiance of all human activities to their inaugural truth" (p. 25).

According to Gauchet, the transformation of religion originates with the rise of the state, becomes intensified with the "axial age," and culminates in the inner dynamics of Western Christianity. The emperors of the

early states, by representing the divine, bring the "religious Other" into the human sphere. By doing so they allow the religious to become contested. This differentiation becomes intensified with the axial revolutions, which introduce the principle of individuality, understood as inwardness, into history. There is a latent differentiation between the political and the religious. This differentiation sets a dialectic in motion that becomes dramatically accelerated in Western religious history through Judaism and ultimately Christianity, which for Gauchet is the religion to end religion.

Gauchet's book offers a very complex and sophisticated argument that is full of interesting and provocative insights, for example, the observation that "we passively accept situations where our ancestors took the initiative; and we try to take the initiative where they were happy to submit. For it is true that we initiate our world by creating it, we conversely submit to the changes thus unleashed" (p. 7).

Compared to the multitude of narrow and parochial studies in the sociology of religion, Gauchet's approach is refreshing because he dares to ask rather general but important questions. However, his answers and methods are sometimes quite problematic. In many respects this book reads like a piece written at least decades if not a century ago rather than one composed at the end of the 20th century. His account of "primeval" society is highly speculative and homogenized into a mythical fiction. His political history of religion represents less a historical-sociological account of the modern world than a philosophy of history informed by some sociological thinking. His binary oppositions between myth and reason, dependence and autonomy, immersion in nature versus transformation of nature are crude and much more designed to produce a worldview than to analyze worldviews. Moreover, Gauchet's political history of religion is based on the concept of secularization—in fact this book represents one of its strongest recent affirmations. But by placing the seeds of secularization into the rise of the early empires, secularization becomes the logical outcome of the reconfiguration of religion as soon as the paradise of "primeval" society is lost. Gauchet is certainly right that religion plays a different role in different types of societies. However, his argument of the decline of religion is fundamentally based on an essentialist notion where the "essence" of religion lies in its "origins."

Although Gauchet refers to some literature, major authors and texts he actually is or should be in conversation with are absent. Most surprising is the lack of reference to the work of Eric Voegelin who, in his *New Science of Politics* (Chicago: University of Chicago Press, 1987), tells a story that is in many respects quite similar to Gauchet's. Nevertheless, this book offers rich intellectual stimulation for readers interested in such a grand vision.

Ritual in Early Modern Europe. By Edward Muir. Cambridge: Cambridge University Press, 1997. Pp. xii+291. \$54.95 (cloth); 17.95 (paper).

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Ritual theory and analysis constitute a major component of recent work published in cultural sociology and anthropology. In the introduction of his book, Edward Muir acknowledges this by stating that his task is "not to explain ritual in a universally applicable way" (p. 6) but rather to examine rites and ritual during a particular historical period in Europe. Intuitively, one would not expect a theoretical contribution from Muir's book, as it is a textbook, part of the series "New Approaches to European History," geared toward undergraduates. However, Muir's book surprises: it is not a dull rehash of classical or current work on ritual; instead, it is innovative, both as a textbook and in its contribution to social history and ritual theory.

In writing a textbook, Muir surveyed a vast amount of literature on ritual theory, ritual historiography and social history focused on ritual. (A real bonus of the book is the extensive bibliographic references that include a brief synopsis of each work—immensely useful for both the novice and the scholar looking for literature on specific topics.) But the book is not a survey. Muir presents a specific and definite argument concerning the historical rationalization (not his term) of ritual. According to Muir, during the Reformation rituals began to be understood as "an aspect of language that communicated meaning," instead of "a kind of behavior that created presences and enacted states of being" (p. 8). That is, the Reformation transformed ritual from practices that did something concrete (through the supernatural) to practices that represented the meaning of something.

Thus, the historical trajectory of Muir's analysis is the nature and function of ritual in early modern Europe before and after the Reformation. Prior to the Reformation, rites addressed peoples' material and emotional needs by making something "present." Pre-Reformation rites often accompanied important moments and passages in time (baptism, marriage, manhood), and ritual practices focused on the body "whether the body of an infant at the baptismal font, the body of Christ in the Eucharist, or the body of the king as he progressed in triumph into a city" (p. 7). The first two parts of Muir's book are devoted to showing how this pre-Reformation "doctrine of presence" influenced both concepts of time and understandings about the body. Muir's analysis of ritual bifurcation (the passionate lower and rational upper) and presentations of the body is especially fascinating, as he demonstrates its impact on social values and consciousness.

During and after the Reformation, ritual practices became what we today understand them to be—symbolic practices and representations of meaning. As Muir so articulately explains in the third section of the book,

the Reformation, and the attendant social and political struggles over it, did not just transform ritual practices and beliefs. Over the long run, the Reformation marks a major shift in the understanding of the relationship between human behavior and meaning. The obvious example is the Eucharistic debate: no longer was the bread understood to be Christ's actual body that infuses the receiver with grace but a representation by which to understand Christ's spiritual presence.

According to Muir, as religious reformers asked and debated the question What do rites do? the Reformation also marked the beginning of ritual theory. And with the crisis of confidence in the emotional and material efficacy of ritual (p. 167), the heuristic value of rituals emerged. Henceforth, for both participants and observers (and social analysts), rituals became vehicles of interpretable meaning. As such, ritual practices were appropriated from the spiritual world by the political world. In the final chapter, Muir discusses early political and civic ritual, completing his demonstration and argument about the "disenchantment" of ritual in the early modern era.

Muir's book will be useful in various undergraduate sociology courses. For example, in social theory it might be used along with Weber's *Protestant Ethic and the Spirit of Capitalism*, to help students understand Reformation ideas, and Durkheim's *Elementary Forms of Religious Life*, to concretely illustrate ritual and its functions. But again, this is not just an undergraduate text. Anyone interested in the social history of ritual and its role in historical and theoretical change will find this well-written book worthwhile.

Sociology and Nature: Social Action in Context. By Raymond Murphy. Boulder, Colo.: Westview Press, 1997. Pp. xiii+321. \$72.00.

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The theme of this book is important, theoretically, methodologically, and politically. Raymond Murphy raises important questions regarding what sociology can say about nature and opens an exciting horizon for sociology. The book's critical intentions center around the idea that nature's importance as a context of social action has been previously underplayed by sociologists. Its answers ultimately are a political credo rather than a theoretically convincing argument.

The bold claims in part 1 present the image of a "sociology as if nature did not matter" (p. 3). This is a highly selective construction. Nature mattered in Montesquieu, Marx, Spencer, Durkheim/Mauss, and Simmel. What is clear is that they were not environmentalists (not even Marx). After the classics, nature vanished from sight in sociology—nature was a given in the high time of welfare capitalism. Murphy's central claim is that sociology still deprives social action of nature and thus of its "true"

context. The enemy is social constructionism. This idea is generalized into a theoretical problem of sociology: that modern sociology has developed toward a theory of creative action and underrated the structural (i.e., natural) aspects of action.

Apart from such unsubstantiated theoretical claims, the question is how to conceive of a sociology in which nature really matters. In showing how social projects are rooted in the dynamics of natural processes, he argues (p. 18) that one should see, for example, nuclear energy as part of the stabilization of the Communist party and Chernobyl as a contributing factor to its disintegration. Sociological colleagues would probably regard this as a courageous hypothesis, contributing another theory about the dissolution of the Soviet empire. That technological failures contribute to the dissolution of the legitimacy of political power is more probable the more images at the base of such legitimacy are linked to the idea of progress. I do not have to understand the dynamics of nature to understand this. The point is another: we have to take into account that industrial policies are not only legitimating, but also delegitimizing mechanisms, given specific constellations.

Part 2 deals with action in a context "in which nature matters" (p. 16). This is mainly a discussion of Ulrich Beck's (*Risk Society* [Sage, 1992]) ideas about "risk society" and some Marxist writings (see Ted Benton, *Natural Relations* [Verso, 1993]). It is an ad hoc discussion, sometimes with interesting insights, held together by the overarching idea of a sociological analysis of action in which nature matters as context. Under this heading, ethical questions in reproductive technologies come after considerations of technological dynamics to which human actions are bound, and they are followed by the discourse on delusions about technology and the self-deconstruction of modernity.

To solve the puzzle of action in natural context, part 3 presents a particular type of modern actor: the risk species. The first argument relates to health risks: BSE, antibiotics, DDT, genetic manipulation. This is the issue of human nature as an object of social action. To human risks are added social risks: the risks of growth, capitalism, globalization, unemployment, class conflict, generational conflict, demographic growth, and shallow science. It is obvious that the organizing principle of Murphy's discussion of rather diverse subjects is the idea of the brute fact of nature from which they follow. Whether this leads to less shallow science is debatable.

Better news is offered when risks are reworked "dialectically" (p. 170) as opportunities. It is interesting that the opportunity to stop pollution is redefined as the opportunity to stop the social construction of pollution, which means the opportunity to make real pollution visible. Social constructivism is the bad guy who deflects attention away from the real problems. This "realism" vanishes in the course of the chapters, as Murphy returns to the old arguments about the market as an institution that does not lead to ecologically benevolent results, to the question of ecological taxes and its effects, to ideas about the positive labor market effects of

green policies. All this is called "symbiotic practices" (p. 101). This, however, is not an answer to the issue of decontextualized social action but an analysis of competing discourses that guided controversial politics in the last decades. To this are added speculations about a lot of opportunities resulting from different, that is, ecological practices in modern societies: better science, more democracy, less inequality, cultural renaissance, and last, but not least, "coupling" (p. 193), from politics to science (interdisciplinarity). All the good hopes are assembled here—the good news after reading Beck's risk society.

The final thoughts regard the relationship of humans to animals, the closure of humans to animals (alluding to the closure and exclusion debate) and to future human generations, a critique of deep ecology (see Mary Bookchin, *Defending the Earth* [Black Rose Books, 1991]), which end in a plea for a posthumanism that goes beyond truncated humanism, an argument very similar to the postmodern critique of enlightenment. French and German diagnostic ideas of our times are put together for whatever it means: a naturalistic humanism. I take this less as a philosophical tradition than a political program and project.

This is the book of an engaged environmentalist whose ethical and moral impetus is out of question. The book is also full of interesting examples, arguments that are to be taken seriously. However, the theoretical concern is lost in a compilation of arguments against real and imagined enemies. In terms of contribution to sociological debates on the role of nature, be it in terms of structuralist, culturalist, or motivational (social-psychological) explanations of social action, little in this quarry of ideas will bring that debate onto new ground.

Emotion, Social Theory, and Social Structure: A Macrosociological Approach. By J. M. Barbalet. New York: Cambridge University Press, 1998. Pp. ix+210. \$54.95.

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In the 1980s, the sociology of emotions came of age in America. By the mid-1980s, there was a section on emotion in the American Sociological Association. Major works by Hochschild, Kemper, Collins, Scheff, and other helped define the field, which was soon divided into two camps. The structuralists (Scheff, Collins) and behaviorists (Kemper), working from classical social theory (Weber, Durkheim, Marx), connected emotional experience to social structure and social relationships. In contrast, social constructionists (Hochschild) and phenomenologists (Denzin) attempted to formulate interpretive models that worked outward from the self and emotionally embodied practices, gender, and the lived body to performative views of social structure and culture. Constructionists moved back and forth from lived emotional experiences and cultural texts

to the cinema, the mass media, and popular culture. Giddens's structuration theory and Elias's historical studies of manners and morals shaded into and helped define these interpretive models. By the mid-1990s, the works of Ellis, Flaherty, Wouters, Hochschild, Connell, Turner, and others would further solidify this tradition. At the same time, scholars such as Tierney, Seidman, Gamson, hooks, West, and Anzaldúa made emotionality, desire, and sexuality a central part of gay and lesbian studies, as well as African-American and Latino studies.

It remained for classical social theory to reenter this picture, and this is what *Emotion, Social Theory and Social Structure* does. In seven closely reasoned chapters and an epilogue, Barbalet examines the works of Weber, Durkheim, Marx, Simmel, Adam Smith, William James, and Schler. Focusing on the specific emotions of fear, resentment, vengefulness, shame, and confidence, he offers fresh insights into the operation of such classical sociological processes as rationality, class structure, conformity, social action, and social change. A single thesis, taken from Kemper, Scheff, and Collins, organizes his analysis; namely, emotional experiences are determined not by culture or individual consciousness but by the structural properties of social interaction. Further, particular emotional experiences determine certain courses of action (p. 27). In this model, emotions are hypothetical constructs, transient states of being affected by external events (pp. 79–80). Culture shapes only the details of experience, not its gross character. Following Scheff and Kemper, Barbalet posits a direct causal relationship between specific emotions (fear, shame, anger), specific external social events (power and status configurations), underlying physiological responses, and forms of emotional display. Barbalet rejects several features of the constructionist approach to emotion, including its overemphasis on culture, its neglect of structure, and its failure to offer explanations of specific emotions.

Thus does Barbalet move (in chap. 1) from Kemper and Scheff's micromodels of social interaction to the significance of emotion for large-scale social processes (p. 28). Chapter 2 argues that background emotions (somatic feelings, feelings of rationality, trust) are central to the operation of rationality and self-interest in market economies. Chapter 3 accounts for the emotion of class resentment in terms of class inequality and business cycles. Chapter 4 expands this discussion, using Keynes's work on investors and confidence in markets, to argue for the centrality of temporality and emotional confidence in theories of action. Chapter 5 presents a typology of shame. Chapter 6 suggests that persons become anxious and filled with resentment when they believe that their claims to basic rights have been violated. Chapter 7 signals the importance of the emotion of fear for the study of social change. For example, elites who confront fearful situations develop corrective actions that have positive effects on social structure. The epilogue brings everything back together. We live in a shrinking phenomenal world. Self-regulation and the management of emotions have become paramount. These conditions set the stage for criticisms of late capitalism, including the increasing commodi-

fication of emotionality, which has been accompanied by a decline in public guilt surrounding shameful experiences. There have been, as well, marked increases in resentment and interpersonal violence.

Thus are the emotions brought back into social theory, read into and through standard sociological concepts: change, conformity, rationality, class conflict. Barbalet's book answers to the criticism that the intellectual descendents of Marx, Weber, and Durkheim have not taken up the challenge to further develop the central place of emotions in social theory (p. 188). And so this is an important book. It reasserts the place of emotionality in classical theory. It makes the classical formations answer to the specifics of the current historical moment. It extends the reach of the micromodels associated with Kemper, Scheff, and Collins, offering a powerful and subtle framework for moving from social interaction to social structure.

But there is more to the emotions and social theory than physiology, social relations, and shifting configurations of status and power. I am not convinced of the value of an approach that locates explanations of emotionality in factors external to the person. Such approaches treat the emotions as if they were static objects with fixed labels. The underlying search for unifying physiological or structural conditions that would account for the emotions will never speak to the basic question of how emotion, as a form of consciousness, is lived, experienced, and articulated by interacting individuals. But sadly this is not Barbalet's project. This absence signals a deeper silence, namely, the need to articulate a critical, gendered, racially sensitive, emotional phenomenology of the media and the lifeworld under late capitalism. None of the classical theorists did this.

Emotions, the Social Bond, and Human Reality: Part/Whole Analysis. By Thomas J. Scheff. New York: Cambridge University Press, 1997. Pp. ix+249. \$59.95 (cloth); \$19.95 (paper).

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Thomas J. Scheff has for his career been a leading explorer of the relation of the emotional and psychological to the institutional and societal, beginning with his well-known examination of mental illness and more recently with a sequence of books and articles that spell out a particular theory of human behavior and methodological prescriptions for studying it, set against critiques of the current social sciences. He asks no less than the biggest theoretical and methodological questions we have: "How can a scientist or scholar capture the reality of human life, when the people whom we study usually cannot do it themselves?" (p. 15). In search of an answer, Scheff intends to connect successfully theory and data, macro- and microlevels of analysis, thought and feeling, inner and outer worlds. It is an ambitious and comprehensive enterprise.

The academic disciplines, Scheff says, are too territorial and conventional to foster creativity and innovation in the study of human behavior. He argues for inter- and transdisciplinary efforts, and, using a term borrowed from botanical morphology, he prescribes "part/whole analysis": studying the very small parts in order to understand the whole. The smallest parts for sociologists are words and gestures. Scheff claims that usually human beings do not say entirely what we mean and hardly mean what we say: though we are fascinatingly expert communicators and interpreters of meaning, we participate in social interactions at such high speed and with such complexity that we are not in the moment entirely clear about all that others and we ourselves mean. The only way, really, to understand what happens in any interactive event is to take the smallest parts (words and gestures), freeze the frame, and review repeatedly—microanalyze—verbatim transcripts, with theory firmly in mind. Scheff demonstrates such data analysis with diverse source material, from Shakespeare plays to television videotape to family psychotherapy sessions.

Scheff argues that all social research implies a theory of human behavior. His own theory places a human emotion—shame—at its center. Maintaining social bonds is essential for the survival of individuals as well as societies, but the security of social bonds is almost never optimal—particularly for anyone in the contemporary age. Fear of disconnection is the fundamental human fear, and shame is the master human emotion, shame being the response to a perceived threat of loss of a social bond. The possibility of experiencing shame is embedded in every single social encounter, as we constantly monitor emotionally our interactions, exactly, though not necessarily consciously or deliberately, noting the disapproval or approval of others. Avoiding, suppressing, repressing, and processing the emotion of shame fundamentally motivates humans' behavior, and as we do this we cumulatively, interactively, and collectively generate the societal patterns of conformity and structure. Social structure in turn creates the multiple contexts in which interactions—and hence shame, or pride—occur and are understood. Microlevel phenomena, then, are understood to be the basis for macrolevel phenomena, and macrolevel phenomena are observable in the smallest of microlevel details.

Scheff is a traditionalist theoretically and methodologically, staunchly emphasizing the importance of specifying theoretical concepts and propositions and of attending to methodological rigor, precision, and verification. He is particularly fond of Durkheim's model, provided in *Suicide*, of testing theory with data; his contemporary sociological orientation, meanwhile, would best be described as symbolic interactionist. But the most energy in the book comes from his injection of the psychotherapeutic into the mix of classic sociological concerns. He does this primarily via family systems theory, with the help of his collaborator Suzanne Retzinger, and following psychologist Helen Lewis's textual analysis of therapy sessions. It is this that will be particularly new to, and provoca-

tive for, sociologists. The most difficult aspect of this to accept will be the application of family systems analysis to such levels as nation states and the analysis of minute verbal detail when it can never be fully contextualized. The energy here is infectious, however, and the theoretical contributions, especially to microlevel processes, are intriguingly engaging.

Scheff has introduced this material before. This book can usefully be considered a kind of collection of Scheff's works; all but one of the chapters are partially or wholly based on (though not exactly reprinted from) previous publications. His intention here is to restate some of his ideas, revise some, elaborate others, and mostly to draw them into one place, with a methodological emphasis, giving examples of how to do the work in the way he advocates. Those who have been following Scheff's ideas will find a move toward extension and coherence here. Those new to the paradigm Scheff has been developing will find this book serves well as an introduction, with particularly good conceptual maps at the beginning of the book and at the beginning of each section; the central argument is multiply repeated, slightly reworded, sometimes differently nuanced. Whether one accepts the larger arguments or not, the book has gems of insight and connection well worth discovering.

Science Incarnate: Historical Embodiments of Natural Knowledge, edited by Christopher Lawrence and Steven Shapin. Chicago: University of Chicago Press, 1998. Pp. vii+342. \$55.00 (cloth); \$19.00 (paper).

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Christopher Lawrence and Steven Shapin have assembled an entertaining and thought-provoking collection of essays on the relationship between bodies of knowledge and the bodies of people who produce knowledge. The volume contains eight essays, by historians of science, which discuss a host of popularized scientists and their range of intriguing bodily conditions, habits, and ailments. Thus, for example, we acquire a deep understanding of Newton's melancholy and ultimately his "break-down" in 1693, just as we acquire a deep appreciation of Darwin's flatulence and the troubles it spelled to have guts that were, in the apt words of the essayist Janet Browne, "noisy and smelly." The entire collection is aided by an essential and evocative introduction by the editors, essential because it places this unusual work in scholarly context, evocative because it answers as much as it raises questions about this highly animated undertaking.

Most readers, including those schooled in the traditions of social studies of science, will puzzle over how to approach this book, about what inspires a concern with the bodies of people who know. The history and

philosophy of science is clearly one place in which *Science Incarnate* finds its home. But another, with considerably wider and more general appeal, is a sociology of culture that seeks out the ways in which the body, at any given time, interacts with and responds to the wide world of social life prevailing at that time. Each of the contributors, while taking up diverse subjects and motley scientists, begins on the premise that the body signals knowledge and does so in stylized, historically contingent ways. As Lawrence and Shapin put it, the body is a “culturally embedded, and culture-constituting, signaling system, and one of the things the body can signal is the possession and reliable representation of *truth*. . . . [T]ruth may be conceived as a personal *performance*, an individual act that uses culturally given materials for its point and value” (p. 10). The body is therefore seen as the embodiment of social order, inscribed with social and cultural codes that place and constrain people, what they do, and how they do it. Consequently, as Lawrence and Shapin argue, if one were interested in examining such macroprocesses as social change, modernization, or the evolution and application of scientific thought, one could turn to historical accounts that articulate subtle meanings of how the body behaves in time. If the notion of embodied knowledge seems novel, even absurd, it is because, Lawrence and Shapin contend, scientific truth has occupied a privileged position over other, less guarded forms of culture.

Knowledge is socially construed as disembodied from its creators by virtue of an ascetic ideal of early antiquity that continues to express itself in modernity. “The belly is conceived to stand at the opposite pole to truth,” mostly to carry on the business of constructing a legitimate and credible state of science, and a trope thus locates the scientist throughout several centuries as a rationale being whose calling transcends the vulgarities and fallibilities of living (p. 23). Yet, if anything, the several essays here call attention to the many ways in which real-life scientists fail to live up to this normative ideal. The public presentations of science and the private experiences of scientists—their passions, aches, pains, physical and moral weaknesses—often run afoul of one another.

Biography often operates to cast its subjects in illustrious positions throughout history, propagating myths from individual accounts of heroic accomplishment. Scientific biography is, along with its many valuable insights, an exemplar of such cultural myth-making. But for all the accounts of individual heroism that so often place science and scientists among the heavens, we have in *Science Incarnate* a series of intimate reminders that scientists are real people too. Indeed, Shapin, in an engaging essay of his own, ventures with suggestive speculation about what modern culture might be up to in its forays into the lives of scientists and all other people who produce valued forms of knowledge. Is it not engaged in a “great experiment to see whether we can order our affairs without a sacred conception of knowledge, and, thus, without a notion that those who produce and maintain truth are any differently constituted, or live any differently, than anyone else”? (p. 45). This book, and

those steeped in the embodied traditions of social studies of science, appear to make the answer to that question quite plain.

Science. By Steve Fuller. Minneapolis: University of Minnesota Press, 1998. Pp. viii+159. \$37.95 (cloth); \$14.95 (paper).

Andy Pickering

University of Illinois at Urbana-Champaign

Steve Fuller's fourth book appears in a series, *Concepts in Social Thought*, intended as primers for undergraduates, but its topic is narrower than its title might suggest. Fuller's target is actually the "public understanding of science"—a phrase more current in Britain than in the United States—which he takes to refer to popular images of science: How do, and should, men and women in the street think of science? These are slippery questions, which, as the book makes clear, ramify endlessly. Attitudes toward science are bound up with ideas of how science connects to technology and how both together connect to large-scale social change. These ideas in turn connect to images of the history of the West and the place of science in it. Is the global hegemony of Europe and America due to the fact that science developed here first? Did that somehow have to be the case? Should we be pleased about it? What about the transplantation of science to other cultures: Does that demonstrate that the truths of science are universal and that it was just an accident that we stumbled upon the paths to them first?

One way to proceed here would be empirically. There is an important and growing literature in science and technology studies that ranges from case studies and analyses of laboratory practice to the historical macroengagements of science, technology, and society. But Fuller proceeds differently and is content to let the substance of science remain an unexamined black box. His method, instead, is to postulate the existence of a set of popular attitudes toward science and then invert them. Two obvious problems arise here. One is that while it is easy enough to dream up stories about what people think about science, it is not always easy to find living exemplifications of them. When, for example, Fuller asks, "why do we automatically say that Africans, Incans [*sic*], or some other non-Western peoples" unconsciously anticipated "Western discoveries?" or when he invokes "the widespread belief that one should consult a medical doctor before judging the state of one's health" (pp. 35, 92), I wonder who he has been talking to (if anyone).

The other problem is that since Fuller continually imputes to the public positive attitudes about science's role in society, his inversions convey an immense and unyielding cynicism. Thus a mention of the "purported 'applications'" of science in "medicines, machines or mathematics classes" (p. 42) is followed not by any nuanced discussion of how specific sciences have intertwined with specific medical or technological develop-

ments but with the comment that "religion provides a useful point of comparison," leading into a discussion of the priesthood, the Catholic Mass, and religious orthodoxy. Sociologists will particularly relish Fuller's demonstration that "Merton's famous norms [of scientific conduct] can be read in a diabolical light" (in the longest chapter, "Science as Superstition"). "Universalism" gets a new "negative spin" as "culturalism imperialism," "communism" becomes "Mafiosism," and so on (pp. 63–64). One has to assume that much of Fuller's cynicism is rhetorical. He wants to shake up what he takes to be the preconceptions of his students. He frequently asserts that he wants to improve science, to help it live up to its own ideals, and to make it more socially responsible than he believes it is now. All are worthy projects (though Paul Feyerabend's writings are the key texts here). But, as Fuller knows, references to "public understanding of science" often function as British code for what others call the science wars, and I shudder to think what the science warriors might make of this book. These days, debunking science by inverting crude propaganda is a political as well as an intellectual mistake.

The Diffusion and Consumption of Business Knowledge, edited by José Luis Alvarez. New York: St. Martin's Press, 1998. Pp. x+318. \$45.00.

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Knowledge is a complicated thing. Entire scientific disciplines focus on the nature, mechanics, and philosophy of knowledge, and on the process of acquiring it, that is, learning. This book's underlying presumption is that knowledge is the key to the modern economy and hence to modern businesses. It is an edited book and contains 12 variations on the theme of business knowledge. The source is a conference sponsored by the European Science Foundation.

José Alvarez introduces the theme and states the goal of the exercise: "to explore the degree to which research being done in Europe was covering the different types of academic knowledge, the several intermediate social institutions producing and channeling those kinds of knowledge to their final user, and their link to national and regional business systems" (p. 2). An additional goal was to find out to what extent the frameworks used in this research are similar. Accordingly, the book operates mainly on a metalevel and explores the knowledge about the diffusion and consumption of business knowledge. Someone who wants to know why managers are so easily carried away with the magic of business gurus and what business schools and business consultants really add to managerial practice and what can be gained from making knowledge explicit when so much of it is tacit may end up disappointed.

The common framework that the editor himself advances is a combination of neoinstitutional theory and the sociology of knowledge. Thus he

distances himself from approaches that emphasize agency and transaction costs. In his first chapter, Alvarez traces the history of the (neo)institutional approach with brief discussions of the ideas of scholars like Weber, Selznick, DiMaggio, and Powell, and he comments on the revival of the sociology of knowledge. The latter field turns out to be responsible for stressing the notions of value, power, and ideology. Although somewhat inaccessible, this extensive but still very succinct overview is worth reading. The number of references is overwhelming: 219 books and articles are cited. One theme is the divergence between the formal representation of an organization and the real process with its irrationalities, informalities, power struggles, myths, and the like. Another theme concerns the actors and institutions that play a role in the diffusion of knowledge. The elusiveness of knowledge processes stands out.

The focus of the majority of the 11 other contributions is on the diffusion of academic business knowledge. Virtually all attempt to pay homage to the neoinstitutional theory that Alvarez espouses. It is not possible to do more than mention a few of the contributions. Haldar Byrkjeflot deals in a difficult article with the alleged failure of the scientific management education. Lars Engwall provides a map of academic business schools and journals under the motto that Mercury, the god of commerce, has joined Minerva, the goddess of arts and sciences. As could be expected, the U.S. influence is strong. Staffan Ferguson provides an intriguing rhetorical analysis of two popular management texts (*In Search of Excellence* and *Service Management*), sorting out their platitudes and their use of labels (like commonplaces that do not get clarified) and their (implicit) ideologies. Again not so surprising, these books turn out to produce a mix of knowledge, belief, and myth. Carmelo Mazza reaches a more or less similar conclusion when he considers the popularization of academic business knowledge. Sue Newell, Maxine Robertson, and Jacky Swan highlight the role of professional organizations, which is to facilitate the diffusion process on one hand and on the other hand to constrain the type of knowledge that passes through. The third and last part of the book contains a few studies of consulting practices and the influence of local conditions on a local outfit of a multinational (by Hope Finney Botti).

The final article, by Nitin Nohria and Robert G. Eccles, could have been just as well an introductory article, as it addresses fundamental questions on the roots and nature of knowledge in general and business knowledge in particular. It points at the big gap that exists between academic and practical business knowledge. Practitioners apparently do not pay attention to what the academics write and say about what they do and do not do. One of their suggestions is that academics might be more useful as advisors to consultants rather than directly to practitioners.

Much of the research in this book is preliminary. The contribution of the neoinstitutional theory that Alvarez advances remains dubious, and the sociological approach is not fleshed out. Clearly, much more work on the role of business knowledge in business practice is in order.

Policing the Risk Society. By Richard V. Ericson and Kevin D. Haggerty. Toronto: University of Toronto Press, 1997. Pp. xiv+487. \$65.00 (cloth); \$24.95 (paper).

Stephen D. Mastrofski
Michigan State University

In 1970, Egon Bittner produced what has become the dominant scholarly perspective on contemporary Western police, one that influenced social scientists to focus on order maintenance as the primary concern of police and coercion as *the* defining capacity of police, a capacity exercised by low-ranking organizational actors who enjoy great discretion in its use. There have been few explicit dissents from Bittner's perspective, and no one has offered a comprehensive theoretical alternative until *Policing the Risk Society*. Here Richard Ericson and Kevin Haggerty critique the received wisdom and provide a new framework that should alter what questions scholars ask about policing, as well as how they find answers.

Ericson and Haggerty argue that the police operate in and are profoundly influenced by a postmodern society organized to detect and manage risks and that the risks to which police attend are far more diverse than what has heretofore been credited: financial and property risks (dealing with certificates, credentials, and financial instruments), hazards to careers and the life course (records on participation in crime, employment, accidents), and threats to identities (e.g., as race, sex, and age). The police are but one of many institutions that monitor risk, exchange information about those risks, and provide expert advice on how to deal with them. Insurance companies, financial institutions, and health and welfare agencies are all a part of a large, loosely connected network constructed of institutions that communicate with each other about risk. The police draw upon these sources for dealing with their own constructions of risk, but they also provide essential risk information (which is often of no interest to the police themselves) to the network, thus participating indirectly in a diffuse and fragmented but pervasive system of risk control that transcends the policing of territories.

Police objectives, structures, and practices are profoundly influenced by the external demand for risk information. Far from being an isolated and special institution of social control, police interact frequently with other agencies in an elaborate, if fragmented, web of symbiotic information interdependency. Ericson and Haggerty maintain that policing has shifted from order maintenance, which defines problems in terms of moral deviance, to risk management, which defines problems in utilitarian terms of danger. Success is defined not in terms of achieving order based on "morality, procedure, hierarchy, and territorial protection" (p. 42) but rather in terms of "security," which is a diminution in the risk of "bads." And the core methodology of policing is not merely coercion but surveillance and the processing and dissemination of its products to constituents outside the organization as well as within.

For Ericson and Haggerty, police are knowledge workers; the technical core of their organization is not coercion but acquisition and communication of information and the creation of knowledge. Police work is accomplished less frequently through patrol of geographic areas and control of people in face-to-face encounters: "Walking the beat is now a matter of 'walking' through the reporting process with a keyboard rather than a nightstick in hand" (p. 395). The authors illuminate the many ways in which police activities (e.g., completing automobile accident report forms) and entire police units (e.g., crime analysis) are devoted to knowledge work. The exercise of police discretion is embedded in the faceless rules and technology of communication about risk—for example, the highly structured forms that police must complete to document events such as encounters with suspicious persons, law violations, and security risks. The forms have become increasingly close-ended, allowing officers to select from established categories to characterize events, people, and circumstances. The forms influence what police observe, when they intervene, how they analyze, and how they dispose of problems, and they do it in ways that are subtle and self-enforcing. And the policing of knowledge work within the police department is itself accomplished by a second order of rules and forms that track information, check its reliability, timeliness, and utility to others.

Risks are thus tracked, assessed, and made known to constituents who demand such information. The popular reform, community policing, is an important rationale for this form of "communications policing." It encourages police to reach out to external constituents, helping them attend to their own risk management. It is not a mystification of the iron fist in the velvet glove, as some critics claim, but a rhetorical mobilization "to mediate and appropriate what has occurred [in the transformation to risk communications policing] and to adjust police organizations accordingly" (p. 70). It promotes security and compliance-based (as opposed to punitive, deterrence-based) law enforcement, while transforming the traditional notion of community (shared territory, values, identities, and traditions) to one dominated by shared fears of risks defined in terms of categories on police report forms.

The volume discusses the implications of communications policing for how police are supervised and held accountable. The "panopticon" of police surveillance details the practices of the police themselves as well as those outside the organization. Computer technology blurs the division of labor between rank-and-file officers, civilian clerks, supervisors, and managers. This diffuses decision making and effectively embeds management and supervision in the department's communications system itself, a system over which individuals and subunits have little control. It makes readily available new information to groups within (and outside) the organization about who is doing and accomplishing what.

Policing the Risk Society extends its analysis into many other areas to which a brief review cannot do justice. It is particularly strong in its effort to place theorizing about police in a much broader framework of

social theory. Implications for other institutions of risk management are discussed and propositions set forth. It should appeal to a much broader range of scholars than those interested in policing. Sections of the volume are abstract and will be heavy going for undergraduate students; it seems best suited for graduate-level courses.

The authors offer detailed support for their arguments, drawing on their field work in Canadian police agencies (interviews with 155 police personnel, observations, and document analysis). They compile the strongest possible case in support of their argument rather than to test hypotheses. The authors have taken a most important first step toward refreshing the way we think about policing, and they have uncovered fertile ground for new empirical analyses that will undoubtedly follow.

Controlling Misbehavior in England, 1370–1600. By Marjorie Keniston McIntosh. New York: Cambridge University Press, 1998. Pp. xviii+289. \$59.95.

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Among the principal issues raised by this important study are the level of local concern with social misbehavior, its regulatory contours, and the attitudes that shaped regulatory initiatives up to 1600. Prior historical research on these issues relies on urban records, though most persons then resided in rural areas, or intensive study of one locality. This comparative and longitudinal study uses a nearly representative sample of local records produced by juries in lesser public courts convened in England's villages and market centers with fewer than 3,000 inhabitants. Marjorie McIntosh constructs a data set from records that represent different geographical regions and types of public courts in communities and market centers. Although the author wanted to explore developments from the late 13th century, this was precluded by the absence of sources with documents that covered long periods. Even so, it is remarkable that she was able to assemble data runs from 267 local courts, for which a third spanned 200–230 years and nearly two-thirds extended for 150 or more years.

McIntosh analyzes 11 forms of misbehavior for 20-year periods (except for the period 1370–99). Of the 11, 10 were not explicitly a violation of law or within the purview of local courts and hence are measures of local concern with disorderly conduct. These include eavesdropping, scolding (quarrelsome or malicious speech), and nightwalking (being out after dark with no good reason), which McIntosh aggregates into a larger category, the "Disharmony cluster." In a "Disorder cluster" she includes sexual misconduct, unruly alehouses, and a vague (but common) allegation that a person was of bad governance. A third cluster contains misbehavior associated with poverty: taking wood from hedges for fuel, sheltering

vagabonds, refusing to work, and permitting subtenants to move into one's holding. For clusters and their components (she combines eavesdropping and nightwalking), McIntosh analyzes change in the proportion of courts whose juries make presentments or pass bylaws on these offenses. An eleventh offense, playing at illicit games, was chosen because, unlike the others, it contravened laws that required local jurors to report the offense. For duodecades (and the 30-year period from 1370–99), McIntosh samples (in most instances) four to six sessions of a court for the presence or absence of presentments and bylaws pertaining to misbehavior.

From 1370 to 1600, 59% of the courts displayed no concern with misbehavior, 36% reported one to three forms of misbehavior per duodecade, and 6% reported four or more forms (p. 139). A gradual rise in concern with misbehavior is evident: in 1370–99, 14% of the courts made presentments or passed bylaws; in the 1460–79 duodecade, this had risen to 40%; by 1580–99 it peaked at 59% (p. 10). These findings militate against prior work that suggests that regulatory initiatives against misbehavior were novel developments fueled by Puritan anxiety. Agreement exists that these initiatives were widespread in 1600. But McIntosh shows that periodic onslaughts against misbehavior erupted earlier, under economic and demographic conditions—for example, high population density, bad harvests, high rates of migration—similar to those at the end of the 16th century.

Other widely held views on regulatory developments fare equally poorly when assessed in the light of McIntosh's research. From the late 14th century to 1600, we observe growing gender equality in jury presentments for scolding. A steadily *declining* proportion of women transforms an overwhelmingly female offense into one in which men and women are equally likely to be reported. Historians who interpret contemporary concern with scolding women as a symptom of gender conflict induced by the rise of capitalism, McIntosh suggests, have been misled by their sources, prescriptive writers, and proceedings in church courts. Equally surprising is the trend for sexual misconduct: jurors presented both men and women in reporting offenses. In any duodecade, women only were reported in no more than a fifth of all cases, roughly the same for male offenders. This hardly supports the hypothesis that in this area regulatory initiatives sprang from male fear over female sexuality.

The author reinforces this last point with additional evidence that she uses to draw inferences about attitudes on regulatory at both the local and national level. Her reading of bylaws and presentments leads McIntosh to conclude that jurors' concern with sexual misconduct flowed, not from explicitly moral objections, but from its potential for provoking conflict. Elsewhere, McIntosh cites rhetorical similarities in texts produced by local courts and in those by Parliament and the Crown and argues that regulatory initiatives were not invariably a "top-down" phenomena. They could originate in the periphery and subsequently influence enactments in the political center of the nation. At the same time, a process

of moral isomorphism occurs and transforms social regulation from a welter of local initiatives to a national phenomenon.

This is a meticulous yet engaging piece of scholarship. And though it is written primarily for historical specialists, it should be read by sociologists interested in the moral order and social regulation.

Crime Is Not the Problem: Lethal Violence in America. By Franklin E. Zimring and Gordon Hawkins. New York: Oxford University Press, 1997. Pp. xii+272. \$35.00.

Gary Kleck
Florida State University

Franklin Zimring and Gordon Hawkins argue that America's crime control resources should be redirected to focus more on lethal violence (i.e., homicide) and less on crime in general. This unexceptional suggestion would probably be endorsed by most social scientists solely on the self-evident basis that homicide is the most serious crime and that resources directed toward other crimes detract from the effort to reduce homicide. While this thesis scarcely requires a book-length justification, the authors have other agendas, addressed in gratuitous, and highly dubious, ancillary arguments.

Contradicting those who might justify efforts aimed at crime in general on the basis that reducing crime will reduce homicide as well, Zimring and Hawkins claim that neither the general crime rate nor the number of active criminals affects rates of homicide in most nations (p. 1), and that homicides rarely evolve out of "criminal activity," by which they appear to mostly mean robberies and burglaries (pp. 61–62).

The authors rely on cross-national comparisons to argue that it is only lethal violence that sets America apart from other nations and that there is little association between homicide rates and general crime rates. They quickly limit their claim to comparisons of the United States with other "developed" nations, but even this limited claim is not supported by the data: the United States ranks highest among all nations on both homicide *and* other crimes, including nonfatal violent crimes, such as robbery, rape, and aggravated assault, and property crimes. So the authors qualify their claim still further by arguing that while the United States may also lead other nations in nonhomicide crimes, it does so only by a modest amount (pp. 35–39).

It is questionable whether either police or victim surveys data are adequate for making precise comparisons regarding any crime other than homicide, even across areas within the United States. It is even more dubious to assume that comparisons across nations, with all their additional complications, are so reliable that one can accurately measure the size of the gaps between them. These data simply cannot bear the weight that Zimring and Hawkins place on them.

Readers may wonder why the authors rely on dubious cross-national data in a book concerned with U.S. crime control policy. Perhaps it has something to do with the fact that within the United States, homicide rates are in fact very strongly correlated with other crime rates. State-level FBI Uniform Crime Reports data for 1990 indicate that correlations with the homicide rate were .87 for robbery rates, .73 for aggravated assault rates, and .57 even for motor vehicle theft rates.

The authors also make no mention of a huge body of research on criminal careers and on the etiology of delinquent and criminal behavior, which has yielded some highly relevant findings: (1) active criminals rarely specialize in just one crime type, (2) the most active offenders engage in a wide variety of crimes, both violent and nonviolent, and (3) many of the same factors that cause violent (including fatally violent) criminal behavior also cause other kinds of criminal behavior and, indeed, rule-breaking behavior in general. Further, research on the prior criminal histories of homicide offenders in particular has repeatedly shown that at least three-fourths of killers have official records of prior criminal behavior.

The evidence is even weaker for the authors' claim that homicide rarely evolves out of other crimes. They concede that about 15% of homicides result from robbery and other "collateral felonies" but for the other 85% lean heavily on the fact that police classify the circumstances of many homicides as argument-based to support their rather vague claim that lethal violence usually develops out of social processes that "are not distinctively criminal in most cases" (p. 61). There is no indication that the authors considered the possibility that disputes over the quality or price of illicit drugs, the division of spoils among criminal accomplices, or juvenile gang turf would also be classified as "argument-based" conflicts, though they hardly evolved out of ordinary noncriminal social interaction.

For a book nominally about violence and crime policy in general, this volume devotes a lot of space specifically to guns and gun control, which the authors describe as the *sine qua non* of American lethal violence (p. 199). Guns or gun control are mentioned on no less than 45 pages of highly dogmatic text, noteworthy mainly for the authors' extreme claims and their apparent ignorance of virtually every significant research finding on the topic of the past 20 years.

To justify this extraordinary emphasis, the authors repeat the same unacknowledged tautology for which they have been criticized since the late 1960s: the association between the homicide rate and gun *use* in crime, as distinct from the more pertinent association between the homicide rate and gun *availability* levels. Since gun use in homicide is itself a measure of homicidal behavior, it has a tautological association with rates of homicide. Ironically, the authors seem aware in general of the problem with associations based on variables containing common components (p. 111) yet are blind to the fact that this is the very error they commit when they correlate the fraction of homicides committed with

guns with the total homicide rate (the number of gun homicides is the main, common component in the numerator of both variables). In contrast, the best available research (ignored by Zimring and Hawkins) indicates that gun *availability* levels have no net effect on homicide rates.

This is but one example of the authors' habit of dealing with contrary evidence by either ignoring it or using one-sided speculation about its alleged flaws to get around it (e.g., p. 121; in general, contrast pp. 106–23 with the research reviewed in my *Targeting Guns* [Aldine de Gruyter, 1997]).

The authors' policy recommendations consist primarily of vague non sequitur suggestions for unspecified "restrictive handgun regulations" (p. 201) and revisions of the criminal law, especially regarding penalty levels, aimed at discouraging lethal violence (pp. 159–84). Generally, empirical research has provided little support for the authors' belief that revising statutory penalty levels reduces violence or crime. In contrast, the powerful link between ghettoized poverty and violence plays no role at all in the authors' thinking about violence, so poverty reduction has no place in their policy suggestions.

Depraved and Disorderly: Female Convicts, Sexuality and Gender in Colonial Australia. By Joy Damousi. New York: Cambridge University Press, 1997. Pp. x+221. \$64.95 (cloth); \$18.95 (paper).

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University of California, Los Angeles

Depraved and Disorderly seeks to shift the debate in studies of Australian convict women "away from earlier discussions of the positive or negative experiences of convict women and the essence of their morality" (p. 3) to a set of different questions "focusing on the ways in which cultural meaning is shaped, and on the nature of relationships" (p. 3). A central concern of the author is to make "gender and sexual difference the basis of cultural analysis, rather than simply 'adding' women to the narrative" (p. 3). In these endeavors, Damousi is quite successful with a study that is based on thorough going research in archival and published sources, that is full of engrossing detail, that reads extremely well, and that does indeed move the historiography of convicts into the realm of cultural studies in a convincing manner.

The book is divided into two parts, the first focusing on sexuality, punishment, and resistance, the second on family life and the convict system. The chapters in the first part examine from a variety of sites and perspectives the conflict between the officials' desire for what they termed "order" and the women's resistance to what they considered oppressive treatment and conditions. A chapter on life on the ships bringing convicts from England to Australia documents the multitude of ways in which women contested the power of their custodians, constantly by engaging

in prostitution, often through verbal abuse, and occasionally by physical assault. Subsequent chapters examine the never-ending battles waged between guards and prisoners over the latter's sexuality and reveal the official attempts made to control and humiliate women through such practices as headshaving (to which the author devotes an entire chapter).

Part 2 focuses on the family life of convicts, examining by turn the problems of motherhood, the life and image of the female orphan in colonial society and in the colonial imagination, and the "transitory nature of marriage" among the poor in the 1820s and 1830s (p. 154). Throughout the three chapters, as in those in the first part of the book, the author makes particularly effective, and often moving, use of stories by and about individuals drawn from a wide range of sources: ship's logs, letters written by the convicts and their captives, and reports on institutions such as the prisons and orphan schools (all too reminiscent of *Oliver Twist*) in which so many of the poor were incarcerated. The letters written by people about their children or those they seek to adopt are particularly evocative. They demonstrate also the difficult economic circumstances of the poor, often having to place their children in institutions when times were bad and seeking to release them when conditions improved, and the strains such circumstances had on family life.

Indeed, the greatest success of this book is putting a human face on all the subjects of its analysis as it demonstrates the myriad ways in which the convicts sought and often were able to resist the attempts at total control attempted by their jailers. As such, *Depraved and Disorderly* makes fascinating reading not just for anyone interested in the Australian convict experience but for scholars who wish to find out how gender can impact the organization of prisons and how prisoners themselves, through their actions, can exercise a profound impact on the practices of the institutions in which they are incarcerated.

Perhaps the only odd note in the book is struck by the cover photograph of a woman adopting, or being required to adopt, a sexually provocative pose (and the close-up of her knee enlarged on the back flap). Who is this unidentified woman? A convict? The author? And why is she posing thus? As proof of the ability of convict women to use their sexuality? While meant perhaps to emphasize the author's argument about the power of convict women to protect and use their sexuality, the ambiguity of the image and the pose undercuts that argument.

"In the Mix": Struggle and Survival in a Women's Prison. By Barbara Owen. Albany: State University of New York Press, 1998. Pp. xii+219. \$19.95 (paper).

Lee H. Bowker
Humboldt State University

Qualitative studies of prisoner subcultures once were common in the sociological literature, so much so that I was able to fill a book on the subject in 1977 (*Prisoner Subcultures* [Lexington Books]). As was common throughout our discipline until recently, male matters received much more attention than female matters. The silence about women's prisons was broken in 1965 with the publication of *Women's Prison: Sex and Social Structure* by David Ward and Gene Kassebaum (Aldine-Atherton). Rose Giallombardo (*Society of Women: A Study of a Women's Prison* [Wiley, 1966]) found similar cultural elements and resisted the temptation to oversexualize the subject with a suggestive title. The three classic studies of women's prisons were complete when Esther Heffernan published *Making It in Prison: The Square, the Cool, and the Life* (Wiley, 1972). Although there have been many journal articles on limited aspects of women's prisons since the mid-1970s and several literary books on the subject during the same period, the women's prisoner subculture domain has not been approached in a quarter of a century. For this reason, the publication of *"In the Mix"* is a major event for students of female criminality and corrections.

Owen spent three years visiting the Central California Women's Facility (CCWF) while teaching at California State University at Fresno. She labels her methodology "quasi-ethnography" because the ways in which time and space are structured in prisons makes it impossible to immerse oneself as fully in a prisoner subculture as would be possible with a street-corner gang or a work crew. Owen writes sensitively and reflexively, always respecting the privacy rights of the prisoners and realizing that she could not be present when illegal activities were being planned or carried out. Following Bruce Berg's *Qualitative Research Methods for the Social Sciences*, 2d ed. (Allyn and Bacon, 1995), she began by examining the physical setting; developing relationships; tracking, observing, and eavesdropping; and locating sociometric stars and subgroups.

"In the Mix" consists of an introduction, a methodology chapter, and four substantive chapters on the women at CCWF. These concern how women came to be in prison, doing time and the ecology of place, prisoner relations within the prison and with outsiders, and the mix. The mix is prison argot for "one's involvement in trouble, hustles, conflicts, and drugs" (p. 167). For most incarcerated women, these are activities to be avoided. Choosing a mixing lifestyle is likely to result in a prisoner losing some of her good time (thereby lengthening her sentence) and in her suffering a period of segregation away from other prisoners. Most women

prisoners have children waiting for them on the outside, and the value of children in the prisoner subculture at CCWF is both extremely positive and intense. Losing good time means putting off the long dreamed of reunion between mother and children, as well as other relatives and lovers.

It is a great challenge for an author to live up to the high standards for women's prisoner subculture studies set by Ward and Kassebaum, Giallombardo, and Heffernan. I am pleased to report that *"In the Mix"* meets these challenges head on and holds up under careful scrutiny. I recommend it heartily to practitioners and scholars in corrections and general criminology. *"In the Mix"* would be wonderful as a supplemental reading in courses on women and crime. Finally, an acquaintance with Owen's work would greatly benefit students in classes on qualitative methodology in the social sciences.

The Color of Crime: Racial Hoaxes, White Fear, Black Protectionism, Police Harassment, and Other Macroaggressions. By Katheryn K. Russell. New York: New York University Press, 1998. Pp. xvi+203. \$24.95.

Hernán Vera
University of Florida

In *Birth of a Nation* (1915), the film that praises the Ku Klux Klan as a cornerstone of the American nation and is hailed by some as a cinematic masterpiece, Flora Cameron chooses to jump to her death into a precipice rather than accept the advances of Gus, a black Union sergeant (played by a white actor in black face). The rage over Gus's conduct is presented as the trigger that a Confederate hero known as the "Little Colonel" needed to organize the Ku Klux Klan. D. W. Griffith, the film's director, saw the creation of this white supremacist organization as the true birth of the American nation. The notion that fear of blacks and the hooded exercise of white supremacy are constitutive of the United States is no longer in vogue. However, the idea that blacks, through their disproportionate share of street crime are responsible for white racism, and the violence that results from it, is still alive and well. As constitutive of the nation's white supremacy or as just a source of white racism, white fear of black crime is more than sufficient reason for examining the issue of black crime in the United States carefully. In the *The Color of Crime*, Katheryn Russell, a criminologist, makes an indispensable, intelligent, and practical contribution to this examination.

The core of Russells' analyses is her measuring of racial equity in criminal justice in the United States. To achieve this, after a parsimonious examination of our historical record, she proposes six fairness principles minimally required for a fair justice system. The first five principles require that criminal penalties apply to all equally and that the race of the offender and victim do not determine the type of crime or the punishment

applied to the offender. These five principles, Russell notes, were openly violated in our slave codes (1619–1865), the black codes adopted in 1865, and the Jim Crow segregation statutes, which only the Civil Rights Act of 1964 and the Voting Rights Act of 1965 dismantled formally.

The sixth fairness principle is the existence of “checks and balances that mitigate against racial bias within the legal system” (p. 25). Russell observes that “explicit racial double standards have been excised from the American criminal law” (p. 131). However, she also observes that “race still affects today’s criminal justice system” (p. 131). This is the case, she notes, when police department policies target particular crimes and particular communities that may result in racial disparities in arrests and convictions. She reviews in some detail two specific examples that dramatically illustrate her point: the disparate sentencing guidelines for crack versus powder cocaine and the death penalty and the race-of-the-victim effect. The 1986 federal crack cocaine law mandates a minimum sentence of five years for possession of 5 grams of crack cocaine or for possession of 500 grams of powder cocaine. The 100 times more severe penalty is not based on medical evidence, and according to the U.S. Sentencing Commission is “the primary cause of growing racial disparity between sentences for Black and White defendants” (p. 132). In 1995, Russell remarks, 88% of the people incarcerated under this statute were black. Nonetheless, in spite of the fact that the U.S. Sentencing Commission, federal judges, and the attorney general have recommended the equalization of the sentences for crack and powder cocaine, Congress has overruled this recommendation. Concerning the death penalty, Russell examines *McClesky v. Kemp*, a case in which the defense of a black man introduced a study of more than 2,000 Georgia homicides. In these cases, 22% of blacks who killed white persons received the death penalty. Only 1% of whites who killed black persons were sentenced to death. In rejecting statistical evidence, admitted routinely in other cases, the court required that there was discrimination in the particular case. Since white jurors, judges, and prosecutors are unlikely to admit openly to their racial biases, “the McClesky Court sets an impossible standard for proving that the death penalty is racially discriminatory” (p. 134).

From this core, Russell reaches out to explore images and realities of crime and race that are seldom considered relevant to the criminal justice system. She studies the effect that the media portrayal of black men as criminals or super entertainers, with few images in between, has on law abiding black students. Her analysis of the O. J. Simpson trial and its media coverage is the most lucid I have read. The polemic with James Q. Wilson’s notion that white racism can be reduced by reducing the disproportionate high black crime rate is as informative as it is devastating for that notion. Her analysis of racial hoaxes and micro- and macro-aggressions is incisive and important to keep in mind in research and policy making.

The book is packed with data, eloquent debate, and level-headed analyses. Its scholarship and imagination are highly satisfactory. Social scien-

tists, policy makers, and educated men and women will find it highly instructive and provocative.

New Right, New Racism: Race and Reaction in the United States and Britain. By Amy Elizabeth Ansell. New York: New York University Press, 1998. Pp. xi+351. \$35.00.

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Amherst College

In the United States in the mid-1990s, racially charged issues have jumped to the forefront of the political Right's agenda as affirmative action, welfare, immigration, and multicultural and bilingual education have come under increasingly successful attacks. This makes *New Right, New Racism* an especially timely work. Employing a comparative framework rare in work on the Right, Amy Ansell examines the role of race in the "right turn" in American and British politics since the 1970s by summarizing important changes in education, welfare, immigration, and civil rights policies and, more important, by closely examining and comparing the racialized ideologies of the New Right in both countries.

Ansell employs a "symbolic conflict model" (p. 33), arguing that politics involves struggle over how to define social reality as well over the distribution of social resources. Symbolic conflict is shaped by its distinct material, historical context, but it also affects that context by determining the terms in which the distribution of "material and political resources" are "perceived and judged" (p. 35). Competing political discourses play a "critical role" (but not an all-powerful one) in "producing and shaping the links between public beliefs and social experience" (p. 269). That is, they work by turning often inchoate popular feelings and perceptions into relatively organized political identities and preferences.

Specifically, New Right ideologies address popular anxieties and resentment arising from the economic, political, and social problems that have accumulated in the United States and Britain since the 1970s and from the crisis of national identity resulting not only from these problems but also from the collapse of the communist enemy that had once served as the basis of national self-definition. Identifying "racialized others" (p. 14) gives New Right ideologies a powerful way of explaining societal problems and redefining national identity in ways that justify conservative policies.

Ansell focuses especially on the *distinctive ways* that contemporary right-wing ideologies in both countries use race. Gone is explicit racist language. Disavowing any racist intent, the New Right has developed two kinds of discourse for addressing race-related issues in nonracial language. The first, an individualist discourse, argues that individuals, not groups, possess rights. As a consequence, government policies should be color blind, pursuing equal opportunity for individuals, not equal results

for groups. From this perspective, affirmative action or any other form of preference for minority group members is simply "reverse racism," which is morally wrong, harms the groups it seeks to help, and exacerbates racial conflict.

The second new racialized discourse, in contrast, emphasizes collective identity and culture: it seeks to defend an American or British "way of life," an implicitly white culture under attack from a growing minority underclass, an invasion of nonwhite immigrants, and liberal policies from welfare and affirmative action to multiculturalism and bilingualism that sustain alien minority identities and cultures.

Ansell emphasizes that these two discourses exist in distinctly different mixes in New Right ideologies in the United States and Britain. The individualist discourse has predominated in the United States, while the national/cultural discourse has in Britain.

New Right, New Racism, in short, makes several important arguments, but I still have several reservations about Ansell's analysis. She oversells the newness of the so-called New Right, at least in the United States. Many of the features that she identifies as "new" (i.e., emerging since the mid-1970s) are in fact fairly old: the ideological fusion of neoliberalism and social conservatism, the use of populist rhetoric, the attempt to draw a line between the respectable and extremist rights, even the attempt to sanitize racial discourse all date from the late 1950s and early 1960s.

More important, *New Right, New Racism* equivocates about the significance of race in New Right ideology. Ansell initially regards race as a "nodal point in the right turn of US and British politics" (p. 23) but later calls it only an "important secondary bond" (p. 192) in New Right ideology. Indeed, her detailed historical accounts frequently marginalize race within the New Right project. In the American case, Ansell tells us successively that "racial issues were present, but not predominant" in the New Right's agenda in its early years (p. 79), that they "remained relatively dormant" during the Reagan years (p. 82), and that they remained "submerged . . . throughout the early 1990s" (p. 84). Race seems to have become a truly "nodal point" only after the 1994 elections gave the New Right the clout to deal with racial issues.

Finally, although Ansell argues strongly that New Right ideology affects popular political identities and outlooks, she takes this relationship for granted, apparently content to infer the effects of ideology from its content. Hence, the reader is left asking: How much influence did the New Right really have on public opinion? How do we know? Did it have greater impact on some groups than others? Why?

These shortcomings, while important, are not unique to Ansell but plague many studies of the Right. Despite them, *New Right, New Racism* is an interesting, significant contribution to our understanding of the contemporary Right.

"White Power, White Pride!" The White Separatist Movement in the United States. By Betty A. Dobratz and Stephanie L. Shanks-Meile. New York: Twayne Publishers, 1997. Pp. xvii+362. \$29.95.

Sara Diamond
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The recent emergence of armed right-wing militias is the latest chapter in the long history of the white supremacist movement. One needs a score card to keep track of the eccentric characters and fractious organizations that populate this milieu, and yet much of the available information about the movement comes from sensationalized media accounts and from interest groups that pay their bills by heightening public fears about the far Right.

What is needed is a disinterested study, and *"White Power, White Pride!"* is among the best there is. Dobratz and Shanks-Meile begin *"White Power, White Pride!"* by explaining their use of the term *white separatist*. Most outside observers prefer white *supremacist* because out-right racism is a centerpiece of the movement's ideology. Dobratz and Shanks-Meile say they are not trying to sugar coat their subjects, but *white separatist* is currently in vogue among the racist Right. Those who claim this label say they just want to preserve their race and culture by living separately. This is not believable given the ongoing, though sporadic, acts of violence perpetrated by white racists.

Watchdog groups estimate a stable figure of about 25,000 organized white supremacists in the United States. The largest factions consist of about 15,000 adherents to Identity Christianity, 5,000 to 6,000 Klan members, and 3,500 to 4,000 neo-Nazis and skinheads. There are also an estimated 150,000 supporters who buy literature, make contributions, and attend occasional movement meetings.

Dobratz and Shanks-Meile give a long list of the rallies and cross lightings they attended. They went sometimes as part of the press corp and, as such, were heckled as members of the "Jewsmedia." To supplement their up-close observations, Dobratz and Shanks-Meile scoured the movement's abundant literature and interviewed willing spokespersons. The interviews revealed the multiple viewpoints among white separatists. Some talk about unifying their ranks into a central organization. Others advocate a "leaderless resistance" strategy, by which small cells are secretly arming themselves for a future race war.

The most useful parts of the book are the historical chapters on the numerous factions of white separatists. The predominant religious tenet, Christian Identity, holds that people of white, North European ethnicity are the only true descendants of the lost tribes of Israel. The Identity submovement overlaps with the remnants of the 1970s and 1980s Posse Comitatus tax-protest groups. This faction comes closest to fitting the *separatist* label. They are survivalists who want as little to do with main-

stream society as possible, yet they are drawn into conflict with government authorities over tax and weapons law violations.

Dobratz and Shanks-Meile stress the growing virulence of antigovernment ideology among white separatists. In the Old South, the Klan often collaborated with white law enforcement agencies in repressing black citizens. In recent decades, the white supremacist movement has become as preoccupied with government power as it is with race. There is a process of ideological fusion underway. Organized racists now frame government, at all levels, as the enemy working to promote the interests of racial minorities at the expense of white people.

After providing a densely packed compendium of facts about the white supremacist movement, Dobratz and Shanks-Meile conclude with a theoretical chapter. They say they want to provide a "political economy analysis of white supremacy" (p. 256), yet their study is about a tiny fringe of the U.S. racial order. The authors cite well-established data on the increasing disparities of wealth and income in the United States and then assert that more people will join the white supremacist movement during times of economic downturn. This is what many of the movement's own leaders say and hope, but it is a stretch to attribute participation in disreputable and dangerous organizations primarily to economic factors that affect everyone in society. The white supremacist movement is not growing alarmingly in numbers. What does seem to be increasing is the decibel of apocalyptic rhetoric and the willingness of members to take up arms against the government and their fellow citizens.

Like many social movements scholars, Dobratz and Shanks-Meile focus on why people join social movements, when it may be as worthwhile to ask why they stay. Is it thrilling to be part of something eccentric, violent, and dangerous—a movement ostracized by the rest of the American Right as well as by mainstream society? Aside from putative political-economic motives, what other needs are being met here? I am not suggesting that we return to psychoanalyzing "extremist" social movements as a way of disowning them. I am suggesting that something beyond tangible, rational interests is at play when people persist in movements doomed to cultural marginalization and political defeat.

Growing Up American: How Vietnamese Children Adapt to Life in the United States. By Min Zhou and Carl L. Bankston III. New York: Russell Sage Foundation, 1998. Pp. xii+270. \$34.95.

Kelly H. Chong
University of Chicago

This comprehensive investigation of the adaptation process of second generation Vietnamese in the United States fills an important gap in the study of contemporary "new" immigrants and the second generation as-

similation experience. The story of the Vietnamese second generation, the children of the latest group of "new" Asian immigrants to enter the United States, provides a particularly provocative material for studying the experiences of contemporary immigrants groups and for reexamining existing perspectives on assimilation and ethnic stratification.

Based mainly on survey and interview material (but supplemented by a variety of national data sources) from New Orleans's Versailles Village, one of America's major low-income urban Vietnamese communities, this study pivots around the following question: Why, despite the economic and educational disadvantages of the parent generation, most of whom are poverty stricken, uneducated refugees with little economic resources or human capital, have many of the Vietnamese children been academically successful relative to other U.S.-born children of similar socioeconomic circumstances? For the authors, this paradoxical situation challenges some of the traditional theories of social stratification that focus on individualistic and socioeconomic/class factors to explain achievement, and it promises a new way of viewing the second generation assimilation process.

The main theoretical perspective introduced by the authors to analyze the unique adaptation process of Vietnamese children is what they call the "theory of ethnic social relations." Employing James S. Coleman's concept of social capital and Émile Durkheim's idea of social integration as key analytic tools, this perspective focuses on the effects of cultural and organizational traits of an ethnic community and its integrative capacity—moving beyond explanations based simply on ethnic values or the family—on second generation achievement.

According to the book, the scholastic success of the Vietnamese second generation can be explained largely by the social capital formed and provided within the ethnic community. In the case of Versailles Village, its "dense, multiplex social system of family or kinship, religious, organizational, and work ties" (p. 222) provides a system of supports and controls that integrates its second generation members into a densely knit set of social relations and facilitates upward mobility by shaping attitudes and behaviors consonant with the mainstream values, such as respect for authority, discipline, and academic achievement. In cases like the Vietnamese refugees, who face enormous social handicaps by being settled involuntarily into low-income, underprivileged neighborhoods in the United States, the reconstructed social structures of the ethnic community serve as a crucial source of social capital decisive for the children's overcoming of socioeconomic disadvantages and function as a kind of protective buffer for its children against the adversarial youth culture of the surrounding community.

The authors supply a battery of evidence to support this contention; for instance, it is found counterintuitively that those Vietnamese youths who are more fluent in the Vietnamese language do better in school than those who are more "Americanized," and those who have stronger Vietnamese identity and values and who function within the context of the

Vietnamese community adapt better socioeconomically than those who do not.

Despite the book's empirical and thought-provoking theoretical contributions, there are, however, some serious questions. One major problem has to do with the definition of "adaptation" itself. In the book, the concept of adaptation is narrowly limited to that of socioeconomic advancement, measured primarily by secondary school academic success. However, in attempting to fully understand the experiences of Vietnamese children, it must be questioned whether socioeconomic achievement alone can be viewed as an adequate indicator of successful "adaptation." What about the difficult questions of identity and a host of other cultural conflicts faced by even the most "successful" of immigrant children? Even if socioeconomic achievement is taken to be the indicator of adaptation, is secondary school academic success adequate as a measure of future socioeconomic progress? What kind of structural assimilation do the Vietnamese children actually experience once they leave school? Although it may be premature to expect answers to some of the more long-term questions, the book's arguments are considerably weakened by the narrow definition of adaptation and the lack of any postsecondary school data on these children.

Indeed, one of the major drawbacks of the Durkheimian integrationist approach of the book is the insufficient attention it pays to the exploration of the more conflictive aspects of the second generation experience, a shortcoming of which the authors themselves are well aware. Although the authors do include toward the end of the book a chapter on some bicultural conflicts (in addition to a chapter on "delinquents"), this chapter, added on almost like an afterthought, emphasizes the long-term integrative aspects of these conflicts and does not seem anywhere near adequate for capturing the complexity and contradictions that must surely be involved in the adaptation process of these immigrant children.

Furthermore, the optimistic conclusions about the unique assimilation process of Vietnamese children, that they are becoming "American" through socioeconomic integration not necessarily preceded or accompanied by acculturation, that they are able to assimilate with their Vietnamese identity intact while experiencing little sense of alienation from the larger society, appear mostly speculative and are offered without much substantiation by the voices of the Vietnamese youths themselves. However, given the increasing evidence of myriad adjustment problems faced by "delinquents" and non-"delinquents" alike in many Asian ethnic communities, such a perspective can distract us from exploring some of the serious dilemmas experienced by Asian youths in the American society.

One final issue worth mentioning regards the generalizability of the ethnic social relations model. Although the authors are for the most part careful in specifying their findings to the Vietnamese community or to other immigrant groups that have settled in low-income, underprivileged neighborhoods, it must be noted nevertheless that the effects of ethnic community ties on the second generation children can vary—and are not

always positive—for different groups, not only along class lines but even among underprivileged communities. If so, the applicability of the ethnic social relations model for other immigrant groups remains to be further researched. Despite these problems, this book is a valuable contribution to the field of ethnic and immigrant studies and should be an informative text for all those interested in the subject.

Child Poverty and Deprivation in the Industrialized Countries, 1945–1995, edited by Giovanni Andrea Cornia and Sheldon Danziger. New York: Clarendon Press, 1997. Pp. xiv+430. \$85.00.

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American University

In Western Europe, the welfare state that undergirds the condition of children from disadvantaged families, though still popular, appears threatened by the exigencies of globalization; in much of the former Soviet bloc, the government apparatus that provided services to children lies in ruins; in the United States, where the welfare state never matured, the success of prolonged and vigorous agitation from the right has created a two-party consensus for even lower taxes and for an even lower level of government activism in pursuit of social betterment. In other words, the outlook for progress in reducing deprivation among the children of the industrialized countries does not look rosy, at least in the near term. On the contrary, as the editors warn us, "The possibility of a serious erosion in child well-being is not remote" (p. 1). Even with such an uncomfortable outlook, however, it is worthwhile to assess where we are and to suggest directions for future efforts against the day, if it ever comes, when the outlook turns friendlier. This book represents an attempt to do both of those. As might be expected, the authors are more successful in the assessment phase of their assignment than in the rallying of the troops for future advances.

The editors have assembled a valuable resource, bringing together a large amount of information relevant to the condition of children for the countries it covers: child poverty rates, infant mortality, single parenthood, nuptiality, divorce, maternal labor force participation, income distribution, nutrition, the prevalence of child labor and crime committed by children. The book also details government programs to aid families with children—income supplementation schemes, public assistance to those not working, child support enforcement, health programs, provision of child care and preschools, government-paid maternity leave—and provides estimates of total government spending on such programs.

A summary chapter gives information for the 19 countries in the OECD, and another presents data for 10 eastern bloc countries. In addition, entire chapters are devoted to Russia, the United States, the United Kingdom, Portugal, Italy, Sweden, and Japan, providing room for atten-

tion to their particular institutions and histories. The acute problem of child labor in Portugal and the system of cram schools in which the Japanese students are trapped are among the notable topics covered.

Of 12 of the richer Western countries for which poverty data were assembled by Lee Rainwater and Timothy Smeeding, the United States is by far the worst in the incidence of child poverty, at a 21% rate. Next worst, although not nearly as bad as the United States, are the other English-speaking countries in the list—Australia, Canada, and the United Kingdom. Although the authors do not speculate on this anglophone cluster, it cannot be a coincidence; perhaps the libertarian economic doctrines of American origin that recommend abstention from governmental ameliorative measures lose something when they are translated into other languages.

The policy measures recommended tend to be laundry lists, and priorities and tradeoffs are not dwelt on. The authors understand that a major threat in all countries to children's well-being is the growth of single parenthood; indeed, that truth is repeated in each chapter. But in the enumeration of policy suggestions, the authors—mostly male—give a high measure of approving attention to measures that were designed in the 1930s to give modest help to the then-prevalent two-parent, male-breadwinner family: family allowances and labor market measures. Such programs cannot come near to providing a decent life for the family of a single mother, who may require \$10,000–15,000 worth of child care services annually if her children are to be given care that is both safe and developmentally sound. Nor can long parental leave at partial pay, also emphasized by these authors, be a mainstay of such families. Single-mother families need massive transfers, and measures such as family allowances that give modest sums to a wide range of people rather than targeting large sums to the most needy can plausibly be characterized as a waste of resources. Given the political difficulties of helping single mothers, the provision of expensive services such as housing, child care, and health care that the public can see as a direct help to the children themselves is probably the most promising way to reduce child poverty.

Hurricane Andrew: Ethnicity, Gender and the Sociology of Disasters, edited by Walter Gillis Peacock, Betty Hearn Morrow, and Hugh Gladwin. New York: Routledge, 1997. Pp. xx+277. \$110.00.

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University of Delaware

If there are still some people out there who think of disaster research as glorified sociological ambulance chasing, this edited volume on how Hurricane Andrew affected residents and communities in southern Florida should set the record straight. Much recent work in the field explores the ways in which social, economic, and political factors structure both

hazard-related social behavior and disaster impacts. This book, which blends neofunctionalist, social ecology, and political economy perspectives on disasters, is a fine addition to that literature.

Conventional wisdom sees disasters as natural phenomena that pick their victims at random. Media stories hinging on the notion that disasters "spare no one" perpetuate the myth that socioeconomic and other distinctions make little difference when disaster strikes. In contrast, the separately authored chapters in this book demonstrate that the effects of disasters—including even the physical damage they produce—are social in origin. Using population surveys, ethnographic field work, social indicators, damage statistics, and geographic information system (GIS) data, and supplementing these analyses with striking photo images, the authors show how predisaster economic, racial, ethnic, and gender inequities translated into very different disaster experiences and led to uneven patterns of recovery for the individuals, households, and communities that bore the brunt of the 1992 storm.

Hurricane Andrew begins by discussing how regional economic and sociodemographic trends helped create both Dade County's complex urban landscape and its high hurricane vulnerability. It then moves to an analysis of the social factors that influenced patterns of evacuation when warnings of Andrew's impending impact were issued. Consistent with other research, the authors find that race, ethnicity, income, and household-level variables such as family size were among the factors predicting who fled from the storm and who did not (or could not). Other analyses show how both disaster losses and access to temporary housing, insurance, loans, and other resources needed for recovery were shaped by pre-existing social divisions. The hurricane's devastation was greatest in substandard and poorly-constructed dwellings—the kinds of structures inhabited by poor people, who are also more likely to be members of minority groups. The poor tended disproportionately to end up in the abysmal tent cities that were set up to temporarily shelter the homeless, in which the social solidarity seen in the early days after Andrew's impact was followed before long by a rekindling of interethnic antagonisms. African-Americans were particularly hard hit, and they fared the worst in their attempts to have their losses covered by insurance, largely because of racial segmentation and redlining in the insurance industry.

The findings show clearly how social and cultural capital helped determine who gained and who lost in the aftermath of Andrew, just as they do in social life generally. Navigating through the assistance process required systems skills and knowledge that better-off victims were most likely to possess. Governmental disaster relief programs, based on bureaucratic rules and middle-class notions of what constitutes a household, presented barriers for many disaster victims from low-income and marginalized groups. Aid programs were ill-suited for the extended-family living arrangements that were common among immigrants in the impact region.

The same inequities that were seen among households also played

themselves out at the community level. One chapter compares the small, poor, majority African-American community of Florida City with Homestead, its larger, better-off, predominantly Anglo neighbor. Both were located directly in the path of the storm, but residential damage, property value and job losses, and other negative economic impacts were proportionately greater in Florida City, while government and other disaster aid payouts were proportionately lower.

Gender issues have been undertheorized and underresearched in the field of disaster studies. Hurricane Andrew conceptualizes gender as a stratifying force that, along with social class and ethnicity, played a role in both disaster victimization and the recovery process. Authors discuss the ways in which gender is a source of disaster vulnerability and describe women's experiences as caregivers and providers of social support, links in complex kin networks, heads of households struggling to reconstitute their badly disrupted lives, suppliers of needed but unpaid labor, and activists mobilizing to have a voice in community recovery decisions.

Alejandro Portes and Alex Stepick end *City on the Edge*, their award-winning book on Miami (University of California Press, 1993), with a brief postscript on the devastation wrought by Andrew in which they try to project how the storm could affect the pace and direction of sociodemographic change in southern Florida. This volume, written in the spirit of *City on the Edge, Urban Fortunes* (John R. Logan and Harvey L. Molotch, University of California Press, 1987) and other work on the "political economy of place" concludes by documenting the hurricane-induced changes that have begun to occur. Its theoretical approach and findings will interest not only disaster specialists but also urban sociologists, planners, regional scientists, and race, gender, and public policy researchers.

Uprooted Women: Migrant Domesticity in the Caribbean. By Paula L. Aymer. Westport, Conn.: Praeger, 1997. Pp. 172. \$55.00.

Elsa M. Chaney
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Uprooted Women explores the first two generations of several migrant streams of women from the impoverished Spanish- and English-speaking eastern Caribbean to the more prosperous islands, focusing specifically on Aruba in the Dutch Northern Antilles. This is a novel and fascinating study that sets out to answer two major questions: Why did women continue to go to Aruba long after U.S. investment and the oil boom had ceased (foreign male workers in the refineries were all repatriated by the mid-1960s)? Why did many of the older cohort of women migrants remain on Aruba to become permanent residents while the younger cohort did not? She also sets out, very successfully, to document the crucial economic contribution—largely unrecognized—Caribbean women migrants

make to their households and their countries of origin through domestic service and other work in the informal economy.

The answer to the first question is simple: women from islands of high unemployment continued to find "consistent, predictable, and higher wages than they ever hoped to get in their home islands" (p. 45). The majority of Caribbean women are destined to be mothers, many without stable male partners. They are considered by their societies (and by themselves) as economically responsible for their children. One profession always open to Black Caribbean women has been domestic service, often requiring migration to destinations where pay and opportunities are better.

Aymer's chapter 2 is an excellent history of intra-Caribbean migration, generated by U.S. investment, beginning in the 1930s, in gigantic oil refineries in Venezuela, Trinidad, Curaçao, and Aruba (which had two refineries) that later became essential to the Allies in World War II. Eastern Caribbean women (and later women from Haiti, Colombia, and Venezuela) first came to Aruba, beginning in the 1930s, as maids, nurses, and licensed prostitutes (the latter a concession to the oil workers at the huge Lago refinery, run as an enclave very much like, Aymer says, a U.S. army base). After the war, male job opportunities gradually declined, with foreign men being systematically replaced by Arubans.

In contrast to the haphazard, often nondocumented migration of Caribbean women to many destinations, Aruba has always carefully monitored its recruitment of foreign workers. Men and women from other Dutch possessions had free entry under "Dutch Rights"; other migrants came under stringent controls. Women domestics had to "live in" (literally, in the household, not in a detached building) for the first two years; after that, they could choose a "free signing" status where they could "live out" and have some autonomy over their free time. They often were obliged, however, to take on multiple clients in order to pay their expensive health insurance, have enough to live on, and—all important—send remittances back home. By law, the women earned the official basic wage, had two unpaid vacations, one-half day off per week, and two hours free for church on Sundays. Pregnancy meant immediate deportation.

There often was little time for socializing or for relationships with men (although a number of the older women married Arubans and have remained on the island for good—some of them owning their own homes). Social life was confined to the household workers' community; they had no part in the larger Aruban society. In answer to Aymer's second question: Why the older cohort stayed and why the younger women were anxious to move on, Aymer found that older women tended to disconnect from relatives and friends at home and on visits found that they no longer felt comfortable on their home islands. The younger women—the second cohort who came 1960s and later—appears to be more mobile. They are open to opportunities on other islands and dream about the possibility of finding work in the United States or Canada; some actually go.

Migrant mothers pay a price. Aymer documents the difficulties and heartaches of "mothering from a distance," which continues with the third generation that she dates from the early 1990s. A mother's absence, often for years, means mobilizing relatives and neighbors to watch over their children. When these often tenuous arrangements fall through, mothers have to decide between breaking their work contracts and returning home or continuing to provide for their children's material needs and education.

Since the 1990s, the third generation of eastern Caribbean women have been coming to Aruba. Aymer concludes that they are not improving their situation over the first cohorts; even the relative freedom these "work horses" found on Aruba "was entangled with demanding family obligations, grinding work, social ostracism, and virtual exile" (p. 144). This is still the case.

Aymer's interviews with the first two cohorts were carried out in 1989 for her doctoral dissertation. She, herself, is Caribbean in origin, having spent her childhood in Grenada; she also lived in Aruba in 1975 when her interest in household workers was first aroused. One difficulty in the text stems from the fact that Aymer's respondents represent a very small group (26 women), leading to overuse of the same quotations to buttress various points. Her interviewees were recruited through a snowball technique: one respondent introducing Aymer to her friends. Aymer was often forced to work through translators (understandable since Aruba is an island of many languages: Papaminto—the mother tongue—Dutch, Spanish, and English).

Aymer also struggles with theory, but I believe not successfully. Curiously, she uses only scholarship from U.S. or Caribbean Anglophone sources, ignoring the rich theoretical contributions of Latin American scholars (perhaps the language barrier again, although a great deal of this scholarship is available in English translation).

Nevertheless, Aymer has made an original and most interesting contribution to the field. The price of this book, however, is ludicrous, which means that for the most part it will be available only in university libraries.

Now Hiring: The Feminization of Work in the United States, 1900–1995. By Julia Kirk Blackwelder. College Station: Texas A&M University Press, 1997. Pp. xv+308. \$39.95 (cloth); \$17.95 (paper).

John L. Rury
DePaul University

At first glance, the title of this book may be a bit misleading. "The feminization of work," after all, suggests an examination of the process by which various occupations became dominated, or at least infiltrated, by women. Alternately, it might indicate a discussion of the ways in which American

institutions have adapted to the growth of female employment, with its attendant challenges and cultural transformations. Instead, this is a somewhat conventional history of women's work in the United States since the turn of the century. Its focus is the overall growth of female employment, with particular attention to the ways it affected women's lives. The difference from previous accounts lies in the details.

Julia Blackwelder is a historian, and the book is structured more or less chronologically, although there are some topical chapters that deal with such issues as education and the family. It is not generally a work of primary scholarship, even though the narrative is sprinkled with interesting accounts of how changes in the economy influenced particular women, most of them working class or African-American. Blackwelder also includes a series of brief discussions of the Girl Scouts as an example of a female institution that was affected by changing women's roles. For the most part, however, it is a thoughtful survey treatment of the growth of women's work in the United States, appropriate for advanced courses in American history, women's studies, or related topics (such as social change).

If there is a distinctive feature to this account, it is the consideration given to education in the development of women's work. Drawing on some of the recent scholarship on the development of women's education, particularly in the opening decades of the century, Blackwelder argues that schooling played a critical role in preparing some groups of women for certain work roles, at the same time that it differentiated the female labor force along class and ethnic lines. This augments her attempts to make distinctions between the experiences of different groups of women, a major strength of the book. In the closing chapters, however, Blackwelder's discussion of education seems to lose touch with the work of other historians. Her account of the 1950s, for instance, attempts to situate the meaning of Betty Friedan's *Feminine Mystique* (Norton, 1974), along with other texts and reports from that period, without consulting even such standard works as Barbara Miller Solomon's *In the Company of Educated Women* (Yale University Press, 1985). The result is a somewhat muddled discussion that does not explain the effect of education as clearly as her treatment of the earlier decades.

There are other aspects of the text that are troubling also. Blackwelder seems to adopt a rather optimistic view of recent developments in women's education, noting the growth of female college enrollments and graduate study since the 1970s. She does not consider continuing patterns of curricular differentiation along gender lines, such as the painfully slow progress of women in such fields as mathematics and the sciences. A corollary to this, of course, is the steadfast resistance women have faced in professional fields long dominated by men, even when their numbers have risen in recent years (such as in law).

And then there is the question of the family. Instead of integrating her discussion of the ways women's work affected family roles throughout the book, Blackwelder tackles this critical issue in a single chapter at the

end of the volume. While this has the virtue of focusing the reader's attention on an important element of the story, it also means that a piece of the puzzle is occasionally left out of her earlier attempts to explain the growth of female employment. In her discussion of the development of women's work in the 1970s, for instance, Blackwelder fails to emphasize the stagnation in men's wages that led many working class wives to pursue greater employment. Similarly, rising divorce rates in the 1970s also were undoubtedly a spur to women's labor force participation. A better weaving of family and work issues may have made connections such as these more apparent.

Regardless of these questions and a number of others, this book could prove useful in any number of courses dealing with women's work in the United States and as a general reference to scholars working on topics related to this large subject area. Women's work and related questions of education, family life, and gender status, after all, continue to be active topics of research in a variety of fields. As this body of scholarship grows, new efforts at synthesis are important. Blackwelder's study represents the latest such effort, and for the moment it is a useful compendium.

The Time Bind: When Work Becomes Home and Home Becomes Work.
By Arlie Russell Hochschild. New York: Metropolitan Books, 1997. Pp.
xv+316. \$22.50.

Jerry A. Jacobs
University of Pennsylvania

American families face many pressures, not the least of which is the lack of enough time. Rosabeth Kanter, in her 1977 work, *Men and Women of the Corporation*, described some executive positions that were so demanding that a working husband needed a wife devoted full time to supporting his career. Increasingly, that wife is no longer at home but has her own challenging job. What happens to careers, families, relationships, and parenting when both parents are so deeply engaged in their careers that they have precious little time or energy for the rest of life?

Arlie Hochschild seeks to chart this landscape in her latest book, *The Time Bind*. Hochschild, one of the few remaining public sociologists, is above all a captivating writer. She writes books on important topics for a broad audience. She has coined many evocative phrases—"emotional work," "the second shift," "the stalled revolution"—that have enriched our vocabulary for discussing everyday life. The phrase "the time bind" is likely to join this list as a shorthand for the challenges working parents face in meeting the competing demands of work and family.

Her goal is to change the way people see, and improve the way they experience, their everyday lives. In my view, *The Time Bind* is not entirely successful in achieving these objectives, but it is nonetheless a riveting and challenging book. If it falls short, it does so because it addresses

daunting issues and because we have come to expect so much from its author.

The Time Bind is divided into three sections: an introductory section that lays out Hochschild's thesis about how home has become work and work has become home, a middle section that presents material from her interviews and observations, and a concluding three-chapter coda. The best section of the book is the middle, which reports profiles of workers (and their families) employed at Amerco, the large manufacturing firm Hochschild studied. These stories clearly depict the difficulties working parents face in attending to the demands of the workplace at the same time as they care for their children and struggle to find time for themselves.

While we have come to take it for granted that women work outside the home, Hochschild shows how the rules of the road for working mothers and fathers are still being worked out. How bad must Johnnie's sniffles be before mom decides to stay home and cancel that meeting at the office? How often should dad give in to Suzie, who wants a few more minutes with him while being dropped off at day care? How much pre-packaged food can parents feed their children while still preserving their sense of themselves as devoted parents? Is it OK to check your phone messages and E-mails during family time? These and dozens of other everyday challenges may not determine children's success in life, but they form the contested terrain of middle-class parenthood.

Her vignettes on the job side of the work-family line are less telling. Hochschild advances the thesis that firms now draw in workers so effectively that they become willing volunteers for the corporate family. "Many Amerco employees spoke warmly, happily and seriously of 'belonging to the Amerco family,' and everywhere there were visible symbols of this belonging" (p. 44). But the participatory management schemes Hochschild discusses cover only a small fraction of the workforce, even at large firms like the one she studied. And even there, workers' attachment to their jobs results in no small part from the need to get ahead. A careful reading of Hochschild's vignettes shows that workers often tried to pursue alternative paths toward less demanding schedules, but they found these paths blocked, not clearly marked, or clearly marked "this way to dead-end career." Hochschild wonders why workers did not fight harder for family-friendly work arrangements, but her own data shows the difficult challenge workers face.

I find the central thesis of the book—that there has been a fundamental reversal in the relative attractiveness of home and work—unconvincing. My principal empirical disagreement with Hochschild is in the area of working time. The largest, most consistent and most reliable surveys clearly show that the average American's work week has not changed in the last 30 years. The "time bind" arises because of the growth in the number of dual-earner families with children and single-parent families, not because of a dramatic growth in the work week for the average American worker.

Hochschild thus misreads the key trends regarding working time and constructs a general cultural explanation of a trend that applies to only one segment of the American labor force. Hochschild also fails to notice some of the distinctive features of the firm she studied. Amerco employed almost no part-time workers, a group which has grown to represent one-quarter of the labor force. The average employee at Amerco worked (by my calculations) nearly one day per week more than the average American worker. This is a great place to study the coping strategies of over-worked parents, but it is not an ideal vantage point from which to generalize about trends in labor force behavior and cultural trends in America.

Nearly all her vignettes are chosen from workers who put in 50 or more hours per week, from the 60-hour-per-week executive to the "overtime hound" assembly line worker. Those who work 50 or more hours per week represent an important and growing segment of the labor force, one that stands out as distinctively American in international comparisons. Yet this group remains a minority of workers, encompassing perhaps one-fifth to one-quarter of male and one-tenth of female workers. The premise that this group is representative of the average American worker is simply not tenable.

The Time Bind raises many interesting research questions that should occupy students of work and family life for years to come. Has there been a shift in the relative attractiveness of home and work? If so, does this affect the commitments people make to home and work? Hochschild raises these questions but in my view does not provide thorough answers to them. For example, Hochschild reports that many respondents to a survey agreed that work sometimes felt like home, and home sometimes felt like work (p. 199). However, she presents no further analysis of these data and did not preserve them for secondary analysis. Thus, we will have to wait until questions like this are included in other surveys to learn about the profile of respondents who agreed to the "home as work, work as home" questions and to ascertain whether agreement with these questions is associated with any behavioral consequences. Based on her interviews, Hochschild concludes that the reversal of home and work "was a predominant pattern in about a fifth of Amerco families, and was a prominent theme in over half of them" (p. 45). Again, we do not learn who fits this description or what factors might account for variation among her sample. On this and related topics, Hochschild raises intriguing questions that researchers would do well to explore.

The challenge for public policy is to strike a balance between the workplace and the family that accommodates the new demographic realities of the workforce and does so without reproducing gender inequality in new forms. A number of factors complicate this important task: we live in a world increasingly hostile to regulation, new technologies make it easier to work at home but also make it ever more difficult to be away from the reach of the office, and working parents are only one group in a diverse workforce. Fewer than half of workers have children under 18 at home, and fewer than one in five have preschool children at home.

Hochschild does not detail a set of policies that would address these challenges but instead calls for a national "time movement" to create a new balance between home and work.

The Time Bind is not Hochschild's last word on this subject. She is continuing her inquiries by heading a center for the study of families and work at Berkeley with generous support from the Sloan Foundation. I wish her the best in this important endeavor and eagerly await the insight and passion that her next effort is sure to bring.

Small Firms in the Japanese Economy. By D. H. Whittaker. New York: Cambridge University Press, 1997. Pp. xii+238. \$49.95.

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Washington and Lee University

Whittaker takes on the impossible task of writing about the vast expanse of small businesses in Japan, which employ 77% of the labor force. Such firms are very different from the large firms with their "lifetime" employees that dominate the outside view of Japan and indeed the Japanese image of themselves. There are several "cuts" to analyzing small firms, besides the indigestible encyclopedic approach. One is to focus on the management type, such as "livelihood" enterprises or "entrepreneurial" middle business. A second is to examine factor-market characteristics, such as firms that primarily employ "part-time" housewives. A third is to focus on the type of market, such as subcontractors or retail shopping districts. Fortunately, the author chose a variation of that latter approach and presents us with a concise, thoughtful, and coherent picture of the urban industrial district of Ota-ku, one of the 23 Tokyo wards, which in 1990 employed 70,000 workers in 2,760 establishments in machining and machine tool making (p. 55).

The author begins his analysis by tracing the historical development of the district beginning in the 1930s (chap. 4) and then examines the vertical and horizontal linkages that tie constituent firms to each other and to the outside economy (chaps. 5-6). He next looks at the people in these firms, from the entrepreneurs who run these myriad small companies (chap. 7) to the careers and skill formation of their workers (chap. 8). A theme that runs throughout the book is the future of the Ota-ku district, which he analyzes explicitly across his final three chapters. First, he traces the evolution of recent policy toward small business, including that of the local government of Ota-ku (chap. 9). He next contrasts Ota-ku with the machinery district of Birmingham, England, including the latter's ultimate decline (chap. 10). Finally, he looks to the future, noting the recent outflow of customers, firms, and workers from the district (chap. 11). Several factors are at work. High real estate prices drive firms from the district, while a similar dispersion of customers makes locating in the district less important. Foreign competition is also cutting into their

customer base as production moves offshore. In addition, the next generation of potential workers prefer jobs in the service sector, leading to the erosion of the skill base and of entrepreneurs to replace the founding generation. Nevertheless, while Ota-ku may be in decline, it nevertheless remains very different from Birmingham: restructuring has been through a gradual process of attrition in a dynamic urban setting and has not been accompanied by unemployment.

Whittaker paints a fascinating picture of an underappreciated facet of Japan. Almost a third of the manufacturing workforce in Ota-ku are in firms with under 10 workers, and they historically followed careers that might begin with an informal apprenticeship and end with running one's own show. Given their size, Ota-ku's shops are highly specialized, in the extreme case with only a single type of machine tool. Trading work back and forth to manage capacity, brokering complex orders among multiple firms, and coordinating with firms whose facilities complement a particular shop's specialty are central to survival.

These relationships are managed through a network of personal ties, as well as a host of industry associations. Whittaker could have developed this aspect further. He does not refer to the anthropology literature on Japan's urban areas nor to "lifecourse" studies of unfolding career paths. Nor does he compare Ota-ku with traditional craft production, which is likewise centered in industrial districts. Doing so, of course, could readily have led him far afield. As it is, he provides enough vignettes from his extensive interviews to provide a feel for the human side of Ota-ku's networks, complementing the ream of statistics and surveys he cites and (importantly) keeping the book readable.

One of the book's contributions is to the wider discussion of industrial districts (chap. 3). For example, it complements the work of Michael Piore and Charles Sabel on Italy and the more recent outpouring of writing on Silicon Valley. It also complements the studies of Japanese subcontracting (such as my own work and that of Mari Sako and Toshihiro Nishiguchi), which address small business issues only indirectly. The book is useful for its overview (chap. 2) of the "new" wave of non-Marxist small business studies in Japan, begun by Hideichiro Nakamura and Takao Kiyonari, which moves beyond the largely fruitless effort to view small business through the lens of exploitation and power. Whittaker takes a pessimistic stand on the future of small business in Japan. At least three generations of Japanese scholars have foundered on similar predictions, and he should be wary of extrapolating too much from his observations of Ota-ku. For one thing, manufacturing as a whole is on the decline, but that is normal for a country with Japan's income level and demographics. Even so, new districts continue to develop, such as the one around Suwa in Nagano Prefecture. Indeed, since 1983, the large firms (300+ workers) have seen only a trivial increase in their share of employment (from 27% to 28% of the labor force in 1996). Change in the retail sector is significant; employment there fell from 65% (1983) to 50% (1996) of the labor force, a decrease of 400,000 workers, as larger retailers

and convenience store chains and fast-food outlets displaced small mom-and-pop operations. But the small-firm share increased in other important sectors, such as construction, finance, and services. Small firms will thus continue to employ most Japanese into the foreseeable future, even if Whittaker's Ota-ku machine shops disappear. This, though, is a minor criticism of a book that remains a useful and readable introduction to the small firms that dominate everyday life in Japan.

Strikes and Solidarity: Coalfield Conflict in Britain, 1889–1966. By Roy Church and Quentin Outram. New York: Cambridge University Press, 1998. Pp. xx+314. \$69.95

Samuel Cohn
Texas A&M University

This is *the* big book on English coal mining strikes, the way that Edward Shorter and Charles Tilly's *Strikes in France* (Cambridge University Press, 1974) was *the* big book on French strikes in general. Both books were written by a major figure in their discipline (Church or Tilly), working with a talented younger collaborator (Shorter or Outram). Both books are brimming with statistical analyses of strikes that range from the exhilarating to the misleading. Both books feature qualitative historical analyses, although those of Roy Church and Quentin Outram are far more exhaustive. One would be foolish to cavil with Church and Outram over the factual details of English coal history; they know every microdetail of the record cold. Both books attempt to address nearly every major issue in the literature on strikes at the time of their publication. The Shorter and Tilly made a seminal contribution to North American sociology. Church and Outram also make important contributions, but within sociology they will suffer the fate of being unjustly ignored.

Much of this is no fault of the authors. Shorter and Tilly wrote in 1974 after the mass protests and explosive strikes of the late 1960s and early 1970s. Industrial conflict was central to the social science agenda. In the 1990s, globalization has forced the labor movement and scholarly interest in same into retreat. France 1968 was highly cogent to the reader of 1978. The radicalized coal village and job-conscious pit steward will seem irrelevant to many modern readers.

However, some of the problem is of the authors' own making. The prose is frankly turgid. Long summaries of secondary literature drone on with the point only being made after pages of wandering. The statistical write-ups are amateurish. (Say what you will about North American positivism, *AJS* and *ASR* are models of clear exposition of econometric results.) Worse yet, some key analyses are deeply flawed. Particularly weak is the centerpiece of the book, which reports a quantitative model of strike determination in English coal mines 1927–38 that integrates analytical themes introduced throughout the manuscript. It is based on an

oversaturated equation with an N of 169, 16 predictors, and many pairs of collinear variables. Findings for similar variables come out opposite in sign. The equation is a mess, and Church and Outram practically abandon discussion of its implications in the later chapters of the work.

Nevertheless, what remains in this book is often remarkable and sets a lot of sociological thinking about class conflict on its head. They acknowledge the roles of all the standard predictors of strike activity: business cycles, unionization, political opportunities, and so on. However, they argue that these factors are excessively macrosocial. Their data show strikes were concentrated in miniature waves that occurred in a specific small set of firms. Generally, other firms in the same labor market were not affected by these incidents. Furthermore, these firms were not permanently militant. They had 5–10-year bouts of militancy with quiescence both before and afterward. This strongly suggests that strikes were generally driven by firm level rather than community or national dynamics. This is not what would be predicted from current sociological models, which emphasize cycles, national politics, and the role of communities as a whole.

What were these firm-level dynamics? In an ingenious analysis, Church and Outram contrast pairs of firms with similar structural characteristics but differing militancy. What differentiates the strike-prone from the strike-averse was not union activity, nor the social organization of miners beyond the workplace, but rather the presence of particularly combative and aggressive managers. Strike waves started with the arrival of a new, nasty manager, and they dissipated after the manager was removed. This parallels orthodox “reorganization of the labor process” accounts, since capitalist offensives often included proactive managerial attempts to use technology to lower wages or increase output. However, popular managers were able to reorganize production with the consent of the labor force, while control-oriented or incompetent managers often generated strike waves in the absence of fundamental change. What seemed to matter was managerial militancy, a factor that could be extremely person specific. Such an argument may seem tautological, since one infers “bad attitude” from the presence of strikes themselves. However, the archival data on the actual managers involved provides modest independent confirmation for their position.

There are other niceties in the book, such as a surprisingly convincing attempt to resuscitate the Kerr-Seigel model and an excellent application of Tillyian models of resource mobilization to the problem of strike extension across firms. However, the identification of firm-specific strike clusters and the linking of these to management activity is by far the most novel and interesting contribution. Sociologists may have to be more cautious about predicting class conflict just from the properties of workers and give more attention to the exact microdynamics of sequences of authority/subordinate strategic interactions.

Expanding Class: Power and Everyday Politics in Industrial Communities, The Netherlands, 1850–1950. By Don Kalb. Durham, N.C.: Duke University Press, 1998. Pp. ix+339. \$64.95 (cloth); \$21.95 (paper).

Marc W. Steinberg
Smith College

Class analysis is alive and well; so Don Kalb convincingly demonstrates in his superb analysis of economic and social transformation in the province of North Brabant. Rejecting both economic and cultural determinisms, he offers an approach based in a relational and historical materialism and a narrativist methodology. For Kalb, class analysis focuses on the contingent ways in which regimes of accumulation create local and particular path-dependent possibilities for class formation and hegemony. Class processes are a structured totality creating fields of force, spatially and temporally bounding the development of social networks, political alliances, and the cultural bases of everyday life. Importantly, Kalb insists that class analysis involves the intersection of modes of production and modes of life, arguing that household structure, gender processes, the cultural politics of everyday life and the spatial dispersion of populations combine with the labor process in determining the contingencies and contradictions of class formation and relations.

Kalb illuminates his approach through a detailed comparative analysis of the development of two regimes of accumulation in the central and southeastern Brabant. The industrial region as a whole was characterized by the slow transformation of protoindustrial production based in subsistence agriculture and outwork into one specializing in low-wage, low valued-added mass production for global markets. Kalb takes on the daunting task of explaining paths of divergent development and the *absence* of sustained class conflict in these areas.

In Central Brabant, Kalb charts how the development of shoe production from this protoindustrial base led to solidarity structures among outworkers based in Catholic unionism and a cross-class alliance on social and political issues. By the turn of the century, production was divided between “superexploitative” production centered in households and female labor and large paternalistic factories focusing on skilled male labor. The latter group, along with local clergy and liberal factory owners, created a cross-class alliance cemented by an ideology of social Catholicism. They attempted both to combat the evils of “unfair competition” among small producers and its resultant low wages and also to civilize outworkers by preaching temperance. This alliance stifled the rise of socialist unions and politics and created a local civilizing coalition. However, this regime was undone by 1910 with the advent of cheaper technology, allowing for the rise of small factories that threatened the dominance of the large entrepreneurs and dissolving the alliance. The hegemony of Catholic unions no longer spoke to the daily strife of the new factory workers, yet in its wake no other moral discourse was available. The

result was a highly charged strike whose failure ultimately led to long-term acquiescence.

In the manufacturing area centered in the town of Eindhoven, Kalb focuses on an alternative developmental path to working-class dependency and incorporation based in a production regime of flexible familism. In this system, wages were pegged to household economies anchored in the authority of male workers but crucially dependent on the contributions of dependent daughters. This provided a large and flexible low-wage labor force, first exploited by cigar manufacturers who relied on low-capital export strategies. Increasingly, the area became dominated by the huge plants of Philips, which initially produced light bulbs and eventually inexpensive radios for the global market. Philips's mass production relied on skilled male metal and glass workers complemented by heavily feminized and regimented unskilled assembly. The latter workforce was largely comprised of daughters of Philips's male workers, which allowed for the simultaneous reproduction of household wage strategies and gendered forms of factory discipline. The company instituted industrial paternalism by taming and incorporating the semi-autonomous solidary structures of skilled workers, while relying on family socialization to prepare girls for its despotic assembly regimes.

Kalb analyzes how the local path of economic and political development created working-class fragmentation. Large liberal entrepreneurs in central Eindhoven allied with workers dispersed in outlying communities against the petty bourgeoisie who exercised control over an unregulated and exploitive housing market. Alternatively, this conservative petty-bourgeoisie unsuccessfully sought to cement alliances with workers concerning factory exploitation. This fragmented local politics, spatial dispersion of working-class families in adjacent communities, and Philips's hegemonic paternalism and flexible familism stifled the growth of cohesive and independent working-class activism.

Philips surmounted periodic problems in labor supply by expanding social policies anchored in the provision of housing. Skilled workers were recruited nationally, and many settled in the respectable enclave of Philipsdorp with other technical and supervisory workers. When faced with limits on its local female labor and an increasingly disruptive workforce in the late 1920s, the company responded by erecting Drents Dorp and recruiting unskilled families for its expanding radio production. In this fashion, it successfully recreated flexible familism. Within spatially segregated communities, skilled and unskilled workers lived out the culturally constructed commonsense that Philips engineered into its variegated labor regimes, corporate housing, and standards of social citizenship.

Kalb's analysis is methodical and detailed throughout, carefully parsing out particular conjunctures crucial for each local developmental path. On occasion, his detailed excursions into everyday life, especially when he draws on oral histories, become a bit attenuated from his more encompassing and integrated analyses. Kalb's analysis of the reproductive dynamics of Philipsism would also be bolstered if he more fully demon-

strated how the workplace bureaucratic regimes and cultures, particularly for unskilled assembly, meshed with his detailed analysis of family life and the ideology behind flexible familism. In his more general discussions of class analysis, readers might also wish that Kalb had provided a clearer indication of how his approach can be used to analyze workers' counterhegemonic practices in work and everyday life.

Those looking for a programmatic strategy or positivist recipes for class analysis should be forewarned that this is not what Kalb is offering. Instead, he artfully demonstrates that the best class analysis requires the hard labor of detailed historical, comparative, and cultural analysis. Moreover, his analyses deftly illuminate the ways in which class analysis is necessarily dependent on and linked to the study of other regimes of domination without tendentious debates over causal primacy. Everyone interested in class analysis should read this volume as an exemplary defense of historical materialism, one that is at once cognizant of its roots but that charts a new course for future work.

Reconstructing City Politics: Alternative Economic Development and Urban Regimes. By David L. Imbroscio. Thousand Oaks, Calif.: Sage Publications, 1997. Pp. xxii+210. \$42.00 (cloth); \$18.95 (paper).

Robert A. Beauregard
New School for Social Research

Urban political economists have never been shy about borrowing ideas. Karl Marx has served them well for perspectives on the economy, Robert Dahl and Floyd Hunter (political scientists) have helped them think about political power in the city, and notions of "community" have been drawn from a long line of sociologists beginning with W. Lloyd Warner and Robert and Helen Lynd.

Much of this work was crystallized in John Logan and Harvey Molotch's *Urban Fortunes* (University of California Press, 1987) and in the parallel development of urban regime theory by such political theorists as Clarence Stone and Stephen Elkins. The counterweight to their work is Paul Peterson's *City Limits* (University of Chicago, 1981).

Reconstructing City Politics sits squarely in this tradition, though boldly extending it into the prescriptive realm. David Imbroscio's basic premise is that a consensus exists regarding the "empirical nature" of central city politics. It is called urban regime theory. At its core is a corporate-centered, growth-oriented approach to economic development orchestrated by public and private elites.

This belief frees him to ask to what extent urban regimes foster political inequality and thereby subvert liberal democracy, that is, a democracy that protects individual liberties. The answer is that it does, and this realization motivates the remainder of the book.

Essentially, Imbroscio sets himself the task of institutional redesign,

replacing the political economy of urban regimes with an approach that delivers economic growth but preserves liberal values. To this end, he evaluates three alternative models of economic development: an entrepreneurial-mercantilist model focused on small business development, a community-based strategy arranged around community development corporations and community banks, and a municipal enterprise strategy that favors public ownership. Drawing on secondary material and brief case studies of St. Paul and Pittsburgh, he describes each in depth. He then asks whether they have the potential to be economically effective, withstand legal challenges, and be politically feasible.

He concludes that although potentially effective, each of the strategies faces formidable ideological and political barriers. Nevertheless, he sees signs of resistance to the dominant model and thus remains hopeful.

Imbroscio's dissection of regime theory is well done, and his presentation of the alternative models is thorough and informative. Moreover, the basic intent—revealing the normative inclinations that other urban political economists keep hidden—deserves praise. Yet, he never overcomes the nearly insurmountable problems of evaluation posed by his project. Urban regimes exist in innumerable variations, making distinctions between them and alternatives difficult, and he idealizes the alternatives in order to give them coherence. The direction of large-scale social change resists easy evaluation after the fact; it is twice as difficult prospectively. The result is a piecemeal evaluation of cobbled-together models and a blurring of the boundaries between theoretical potential and practical consequences. His "evaluation" is simply too unsystematic to be credible to even the mildest of skeptics.

Current political regimes in cities do exacerbate social and economic inequalities, stifle innovation, and diminish citizenship, among other ills. Political inequality is not the only normative issue. If Imbroscio's moral argument is to be convincing, it has to be set in a broader framework and made more sensitive to the variety of injustices, oppressions, and exploitations that characterize U.S. society.

When normative theory ends with assessment, though, it courts impotence and irrelevance. Our moral obligations extend beyond pointing to wrongs and offering alternatives. If we are going to take a prescriptive turn in urban political economy, then we should also think strategically about how regimes are changed through purposive and collection action. Institutional design needs to be joined with social mobilization.

Imbroscio deserves applause for casting light on an issue not yet focused on by intellectuals. Still, I doubt that many will follow his lead. Despite sometimes being very vocal about their values, most urban political economists are reluctant to dive headlong into moral debates, preferring the familiar and comforting realms of empirical description and explanation.

Finally, though sociological in its roots, urban political economy has seemingly abandoned the social arena. Civil society is absent, and the economic and political actors who force all others off the stage have little

depth. Often these “movers and shakers” are simply caricatures of economic and political logics. Other social theorists, by contrast, have noted the importance of trust and social networks to political power, the degree to which the economy is also and profoundly a realm of the social, and the ways in which history and culture (both localized and widely spread) influence what can be done, when, with whom, and where.

Imbroscio correctly claims that consensus enshrouds urban regime theory. The paucity of debate about empirical facts, however, does not mean that the peak of truth has been scaled. Rather, it is a sign that urban regime theory has run out of intellectual steam. In response, some theorists have turned to regulation theory, and Imbroscio has set sail onto the moral high seas. The moral path is worth exploring, but without a social imagination for guidance, few discoveries are likely to be made.

Control and Consolation in American Culture and Politics: Rhetoric of Therapy. By Dana L. Cloud. Thousand Oaks, Calif.: Sage Publications, 1998. Pp. xxiv+192. \$45.00 (cloth); \$19.95 (paper).

Robin Wagner-Pacifici
Swarthmore College

Dana Cloud, a professor in the field of speech communication, has written a book that vigorously critiques the current cultural role of that distinct rhetorical genre called “the therapeutic.” In fact, much of the book can be summed up in the following opening statement: “*Control and Consolation* is about how this rhetorical strategy of offering therapeutic consolation as a substitute for political and economic compensation has become a commonplace diversion from political engagement in contemporary American society” (p. xi). It soon becomes clear that Cloud sees the rhetoric of therapy infusing, infecting really, a multitude of cultural and social contexts in the United States—everything from media assessments of an ongoing war (the Gulf War) to avatars of contemporary feminism, to New Age movement conceptions of self and society.

Cloud has designed her book as a series of case studies of such discrete cultural events as the movies, *Thelma and Louise* and *Boyz N the Hood*, political speeches of the 1992 electoral campaigns, and network television’s coverage of the “yellow-ribbon” war. Supplementing these individual case studies with a theoretical description of the history and role of therapy as a cultural discourse, Cloud finds a generalized dampening of the collective, overtly political ethos in the United States as this post-’68 therapeutic phenomenon drives us all to a metaphoric cultural couch. As such, individuals or families, not the class structure or capitalism, are blamed for such social ills as poverty and crime. She writes: “The therapeutic refers to a set of political and cultural discourses that have adopted psychotherapy’s lexicon—the conservative language of healing, coping,

adaptation and restoration of a previously existing order—but in contexts of sociopolitical conflict” (p. xiv).

Much of what Cloud writes and many of her individual analyses seem plausible. Some of this same territory was charted with searing effectiveness as long ago as 1972, when Richard Sennett and Jonathan Cobb wrote their stunning book, *The Hidden Injuries of Class* (Vintage Books, 1972). There, before the full onslaught of the rhetoric of therapy, American workers were found to be living with a pervasive sense of personal failure to make it as a full-fledged citizen, that is, a professional, and to have little sense of how to analyze the structural constraints and dehumanizing features of a class-based capitalist society. They blamed themselves. And certainly we are under the thrall of a strange fetishization of the “family” as the cultural DNA of all life. This is not to be confused with a Freudian obsession with the gnarled and charged family of late 19th-century Vienna, on which Freud himself cast, in the words of Yeats, “a cold eye.” Finally, there is a noticeable decline in the kinds of political activism in the streets that characterized much of the 1960s—the loss of which Cloud laments.

However, something about this study seems forced, and many of the case studies seem crafted with too blunt an instrument. In my opinion, Cloud operates with very narrow definitions both of therapy and of politics. The rhetorical discourse of therapy (distinguished from individual therapy, which she allows can be useful as a prepolitical treatment of serious emotional problems) comes across as thin, superficial, and essentially compensatory. Here, it is as if she took the notion of self-help to be synonymous with therapy and does not engage with more intellectually adventurous ideas about the social unconscious such as those offered by the Frankfurt School. It is true that she is primarily making claims only about that manner of appearance of the therapeutic rhetoric dominant in our culture, but the very notion of therapy seems to collapse into that. The only exception to this, and it is a tantalizing one, is when Cloud analyzes Gloria Steinem’s autobiographical book, *Revolution from Within* (Little Brown, 1992), and finds a brief opening there to something beyond the conservative employment of a therapeutic rhetoric. She writes: “In her analogies between self-liberation and collective struggle, Steinem transgresses the ideological boundaries of liberal individualism, offering a new definition of selfhood and agency that can be inflected as either individual or collective” (p. 115).

Finally, the term “politics” in this book is restricted to class-based, collective movements of and for the working class, with revolution as the goal. This is asserted even as Cloud surveys an American landscape littered with racism, sexism, and environmental devastation. Thus, she rejects the theories and strategic interventions of New Social Movement theorists, with their recalibrations of politics in an age of contingent and fluid collective enterprises. And she does not consider other scholars, such as Paul Lichterman (*The Search for Political Community: American Activists Reinventing Commitment* [Cambridge, 1996]), who have devel-

oped complicated and subtle ethnographic readings of contemporary expressive or communitarian political activism.

While justifiably critical of the often privatizing and politically enervating effects of the therapeutic culture, *Control and Consolation* dismisses the political (antiwar) possibilities of such apparently therapeutic gestures as the yellow-ribbon campaign—no cannon fodder allowed.

Diversity in the Power Elite: Have Women and Minorities Reached the Top? By Richard L. Zweigenhaft and G. William Domhoff. New Haven, Conn.: Yale University Press, 1998. Pp. viii+215. \$25.00.

Aage B. Sørensen
Harvard University

The answer to the question in the subtitle of this book is yes. Though not very many have reached the top, the representation of minorities and women in the power elite has been increasing slowly.

The definition of the top employed by Zweigenhaft and Domhoff is the one used by C. Wright Mills in *The Power Elite* (Oxford University Press, 1956) with minor modifications (Congress is made part of the power elite). This means that the power elite is made up of directors and top executives of major corporations, members of the president's cabinet, generals and admirals, and senators and representatives.

Mills never mentioned women and minorities in his study of the elite. There were none to mention. Zweigenhaft and Domhoff find some and show that their number increases. Using biographical vignettes, Zweigenhaft and Domhoff give an impression of the social-class backgrounds and educational attainments of these new members of the power elite. For interpretations and elaborations, they rely on the research by others and on a large number of anecdotes and biographical details, many gathered from the media. There is much attention on the careers of specific individuals. There is less attention focused on pursuing a specific argument in depth.

Zweigenhaft and Domhoff present quantitative trends and qualitative and anecdotal evidence for access into the various elite locations by Jews, women, African-Americans, Latinos, and Asians. They also include a brief survey of the situation for gays and lesbians, using no quantitative evidence. Jews now are overrepresented in the elite. Women have made substantial progress, but their progress may not have been to the most influential positions, like CEO for major corporations. African-Americans also now are represented on corporate boards in increasing numbers, but they may not be as powerful directors as white males, because they are rarely CEOs themselves. African-Americans have done very well in the military, but Zweigenhaft and Domhoff wonder why the retired black generals are not joining corporations. Latinos and Asians are treated in less detail but with similar conclusions: there is progress but perhaps not

as much as it appears. Few are likely to be upset by Zweigenhaft and Domhoff's conclusions about the trends. Those looking for progress will be comforted, those looking for continued discrimination are provided with support too. Some will find all the vignettes and anecdotes a bit tiring in the end.

Zweigenhaft and Domhoff's last chapter is an interpretation of what has happened and what it means. They argue that behaving and being like the old members of the elite is an important qualification for getting into the elite. Education and social class is important. Those who arrive are disproportionately drawn for upper and upper-middle classes. The evidence is a bit impressionistic, since it is all based on the vignettes, but there is no reason to doubt the conclusion. Discrimination and color remain important. We find many light-skinned African-Americans and Hispanics in the elite, presumably more than in the population as a whole. All these conclusions are based on looking at only the elite and not at those who failed. They are believable conclusions and are supported by evidence from other research. However, it would be a great advance for elite studies such as this if causal hypotheses would be based on analysis of samples not selected on the dependent variable.

Readers probably would like very much to know the consequences of the diversification of the elite. However, this elite study is similar to most other elite studies. It is unable to show directly how and why it matters who is in the elite. With a couple of anecdotes as evidence, Zweigenhaft and Domhoff conclude that the diversification has no consequences for the behavior of the elite and has strengthened the elite and the underlying class system of America. The anecdotes show that women and minorities behave just like white males when they are members of the power elite. I find the generalization very plausible, but the evidence presented to support the generalization is not very solid.

Zweigenhaft and Domhoff's main conclusion is that class matters most for inequality of opportunity, except the continued existence of a deep racial and color line hurting African-Americans and dark Latinos. Other minorities will blend with non-Hispanic whites into a common pool stratified by class and education. I believe this is an important and correct conclusion to a readable and engaging book with much detailed information for those interested in the top.

Virtual Politics: Identity and Community in Cyberspace, edited by David Holmes. Thousand Oaks, Calif.: Sage Publications, 1997. Pp. viii+248. \$24.95 (paper).

Keith Hampton
University of Toronto

Virtual Politics challenges underlying perspectives in the study of computer mediated communication (CMC). The 12 essays in this collection

come from a variety of disciplines including architecture, history, politics, philosophy, comparative literature, and sociology. This book has two main problems. The first is that it does not take into account existing literature in CMC. Second, the text is overly burdened by postmodern discourse that may not be easily understood by a wider audience. Dividing the book into two sections, "The Self, Identity and Body in the Age of Virtual" and "Politics and Community in Virtual Worlds," David Holmes uses the introduction and first chapter to present the book's underlying perspective. He suggests that the Internet and cyberspace should not be viewed as tools that maintain social relationships, or achieve instrumental goals, but as closed environments that serve as a context for alterations to personal identity, relations with others, and community.

Cathryn Vasseleu and Nicola Green write complementary chapters that discuss different aspects of "presence" in virtual reality. Vasseleu examines the "virtual environment suit" and explores the relationship between corporeal existence and the mind/body experiences made possible in virtual reality. She concludes with a reminder that any escape from corporeal limits virtual reality (VR) allows is mediated by being bound to the very body it excludes. Green continues with the recognition, of what has often been neglected in the CMC literature, that individuals do not enter into cyberspace as clean slates but bring with them aspects of their lives that serve to both differentiate and produce inequalities.

Chris Chesher breaks from the flow of the previous two articles and critiques the construction of cyberspace as a spacial domain. Chesher theorizes about how information is stored and revealed based on the idea that computers are an "invocational media." Simon Cooper challenges the notion that imagination and creativity are emancipated through VR and addresses the importance of maintaining social relations outside of VR. One problem with Cooper's analysis is that he refers to VR as if it currently exists and is a widely experienced aspect of modern life. Descriptions of VR are recognized as fictional but are simultaneously taken as a "nevertheless accurate description" (p. 100) of spaces that do not truly exist for analysis. Paul James and Freya Carkeek conclude the first section with an analysis of how our relationships with others are losing meaning through the use of "increasingly disembodied modes of social engagement" (p. 107). This essay is an example of the problem inherent in viewing social relations that take place in cyberspace in isolation from other relationships in "real life." By ignoring existing literature that demonstrates CMC is only one of the many ways people maintain social relationships, and by negating the fact that social relationships vary both in strength and content, this value ridden essay draws conclusions that have been better examined through systematic research.

Michael Ostwald begins the second half of *Virtual Politics* with a unique analysis that I found to be the most interesting article in this book. Ostwald challenges our spatial perceptions of what defines a virtual space by identifying "virtual urban spaces" and exploring community on the boundary between marginal urban spaces of the "Cartesian" plane and

cyberspaces on the virtual plane. Michele Willson's chapter is the most complete discussion in the book on the notion of community in cyberspace. He also continues a discussion started by Holmes that recognizes the dual nature of virtual community in expanding global connectivity while inhibiting local interaction. What Holmes and Willson neglect in their argument is that CMC holds the prospect of "glocalizing" community relations by enhancing both nonlocal (global) and very local communities. The connectivity of CMC provides access to new virtual communities, but at the same time the location of the technology in the home facilitates access to local relationships. It is conceivable that by spending more time in the home people will become more familiar with those in their local "real space" environment.

Mark Nunes continues the discussion of the Internet as self-contained reality and analyzes the virtual realm using geographic metaphors where cyberspace is not an "undiscovered country" but "only the simulation of such, just like a planned treasure hunt" (p. 167). Patricia Wise provides a well-focused analysis of the feminist position within virtual space, arguing that women's modern experiences represent an advantage amongst cyber-citizenry and taking feminist activism into cyberspace. Christopher Ziguras's analysis of the New Age movement, their interpretation of "reality," and their spiritualization of technologies provides an interesting dialogue. The book concludes with Mark Poster's discussion of Cyberdemocracy. Poster explains the difficulties in analyzing the cultural aspect of the Internet from the postmodern, provides a detailed look at the Internet as a public sphere, and examines the notion of gender in virtual communities.

Like many who have written in this field of CMC, there is a general failure to recognize that this is the continuation of debate into the nature of community relations and the effects of technological change on community and society that is as old as sociology. In general, these essays would have benefited by incorporating more traditional sociology into areas such as urban and community sociology. This book is as likely to be used in philosophy as it is in sociology, and I suggest that it is most appropriate for those interested in the study of postmodern culture.

The Democratic Dilemma: Can Citizens Learn What They Need to Know?
By Arthur Lupia and Mathew D. McCubbins. New York: Cambridge University Press, 1998. Pp. xiv+282. \$64.95 (cloth); \$19.95 (paper).

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Boston College

The democratic dilemma refers to the mismatch between what democratic theory seems to call for in the concept of an informed citizen and the well-documented low level of information that most citizens have in actually existing democracies. Russell Neuman in his earlier treatment of

the topic called it *The Paradox of Mass Politics* (Harvard University Press, 1986) and quotes Bernard Berelson's statement of it in the late 1940s. Arthur Lupia and Mathew McCubbins are addressing a venerable problem in political communication and democratic theory.

Their effort here is to extend and develop an existing paradigm. The underlying information processing model is part of a broader family of rational actor models. As in other rational actor models, citizens are treated as active and reasonable choosers about when and what they learn. If they do not acquire certain information, it is basically because the costs of acquiring information often outweigh the positive benefits of acquiring it. Lupia and McCubbins extend this, arguing that aside from costs, the benefits of a lot of information may be zero or even negative. Just because people ignore a lot of information, it does not mean they cannot make reasoned choices when they need to.

Their attempt to develop this model involves a specification of *how* people process information, in the form of theorems with empirical implications that they test. Generally, the theorems are not terribly exciting. For example, in their elaboration of how people use what others say as a shortcut to the information they need, they specify the conditions for successful persuasion. Their findings, they conclude, "provide compelling support for our claim that people are selective about whom they choose to believe" and "respondents showed a tendency to follow the advice of speakers whom they perceived to be both knowledgeable and trustworthy and to ignore the advice of speakers who lacked these characteristics" (p. 201).

Their argument distinguishes information and knowledge. The latter is the ability to predict accurately the consequences of choices. Hence, it rests on information, but selectively, since most information does nothing to improve the accuracy of assessing consequences. "Information is useful," they argue, "only if it helps people avoid costly mistakes" (p. 6).

In general, those who work within this paradigm and are comfortable with its taken-for-granted assumptions should find the Lupia and McCubbins effort a useful extension. For the rest of us, there are the nagging, unaddressed questions. What exactly is meant by this unexamined concept of *information*? At one point, in passing, we are told that "information is data." My mind leaps to the 30-second television polispots by which most modern political campaigns are waged. How do images relate to data? How much media content is about factual claims, and how much is about interpretation and spin? Is it really meaningful to talk about facts as if they are divorced from the larger frames and scenarios in which they are embedded? If news is storytelling, then does information really mean a set of stories, and if so how do these stories relate to the ability to predict consequences?

Nor is the concept of knowledge really much clearer. In one of the examples the authors use, they say that "the more accurate prediction you can make about the incentive effects of a new tax policy, then the more knowledgeable you are about that topic" (p. 24). Are the conse-

quences I am trying to predict here about the impact of the incentives on the economy—about a societal level outcome? And, if so, why should I worry about whether it will stimulate the economy rather than the consequences for increasing an already high degree of inequality? Or am I assumed to be concerned about the consequences for my personal economic “interests” regardless of the societal consequences, or the consequences for my personal values of living in a society with the highest Gini index in the world? What are the “costly mistakes” that this knowledge of the incentive effects of a new tax policy will help me to avoid?

In the final chapter, the authors discuss institutional reforms for assuring that the information provided in a democracy will be useful for the knowledge that citizens need to make political decisions. I confess to a certain envy of the authors when they talk about increasing the penalties for politicians when they lie. They live in a world where facts are true or false, clearly distinguishable from interpretations, and independent of the frames in which they are embedded. In my less satisfactory world, politicians almost never lie but often mislead through innuendo, emphasizing facts selectively and out of context, and every political communication has an agenda it is serving. In a political world where spin is ubiquitous, the idea of penalties for lying seems to eliminate politics.

Every paradigm has concepts and assumptions that are unexamined, and Lupia and McCubbins are not personally responsible for those they inherited in choosing their information processing model. But there is no invitation here or effort to persuade those of different faiths, and this, inevitably, limits their potential audience to those who already believe.

Community of Citizens: On the Modern Idea of Nationality. By Dominique Schnapper. Translated by Séverine Rosée. New Brunswick, N.J.: Transaction Publishers, 1998. Pp. xiv+184. \$32.95.

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University of Wisconsin—Madison

The Community of Citizens is another of a growing number of attempts to save the nation and national solidarity from nationalism and ethnocentrism. In this study of “The Modern Idea of Nationality,” Dominique Schnapper, a leading French social theorist, argues that we are wrong to identify the nation with the celebration of ethnic and other inherited cultural loyalties. For nations, according to Schnapper, integrate individuals into a community of citizens by carrying us beyond the particular class, ethnic, family, and religious identities that persist in private life. This uniquely modern form of social integration, she argues, not only makes liberal democratic citizenship possible, it also sustains the basic “social bond” among individuals in an age in which religious and dynastic legitimations of communal life have disappeared (p. 166).

Schnapper goes so far in opposing the more familiar ethnocultural un-

derstanding of nationality as to declare that the idea of an ethnic nation is "a contradiction in terms" (p. 12). To become a nation, according to her, involves a process of abstraction from our received identities, a process that allows people with different religious and cultural heritage to identify with each other as citizens of a single political community. Ethnocultural groups may, from her point of view, be nationalistic, that is, they may demand the rights and respect due to nations, but they can never become nations themselves without abandoning most of what many hold dear.

Schnapper's understanding of the nation echoes a long intellectual tradition in France, one that stretches to the revolution. But it also resonates with the increasing popularity of the idea of the "civic nation" among contemporary students of nationalism. Like the defenders of the civic nation, Schnapper believes that she can uncover a model of a purely political form of national solidarity in the experience of Western liberal democracies. Where she differs from them is in the emphasis that she places on the particular histories of different liberal democracies and on their role in shaping civic solidarity. Most defenders of the civic nation, like Habermas in his defense of "constitutional patriotism," view the civic nation as a community of shared principle. Civic solidarity, for them, is the mutual loyalty felt by people who share a commitment to a certain set of ideas and institution. For Schnapper, however, this is far too abstract a picture of civic solidarity. "Any sense of belonging or collective idea can only be the product of a long common history, even if it is, more often, either wholly or partially invented" (p. 60). The problem then, as she sees it, is how to use common histories to bind citizens without privileging any of the particular attachments that national solidarity is supposed to transcend.

Schnapper resolves this problem by introducing the most original—and ultimately problematic—idea of her book. The particularity of liberal democratic nations, she argues, stems from the distinctive "political projects" that each develops as a means of integrating their citizens. The British, for example, saw themselves as the inventors of individual rights, the Americans of democracy, the French of national fraternity, and so on. It is only through the attachment to these historically contingent and distinctive projects that individuals come to transcend their diverse ethnic and religious heritage and form communities of citizens. "The integrative capacity of the nation," she concludes, "is defined by the relationship between the political project and the ethnic and social characteristics of populations organized into nationhood by the state" (p. 42).

The strong points in Schnapper's argument are a reflection of her emphasis on the processes of political socialization in modern liberal democratic nations. Most European and North American students of nationalism have focused on cultural, ethnic, economic, or religious sources of socialization, and they have thus missed these powerful, homogenizing political influences within the very nations in which they reside. As a result, they tend to think of the nation and nationalism as a "foreign"

problem, one that mostly afflicts Eastern Europeans and other “uncivilized” people. Schnapper brings western liberal democratic political experience back into the center of reflection on nationalism, as it should be.

The weak point in her argument is the tremendous weight that the idea of the “political project” has to bear in her understanding of national integration. This idea allows her to avoid the abstraction of the civic nationalist vision of political solidarity, on the one side, and the hostility-inciting particularism of the ethnocultural vision of community, on the other. But in order to unite us around these political projects, she must introduce an artificial distinction between the political and cultural contingencies of history. To what extent, for example, is the French language part of the French political project? In principle, not at all. But that project has been formulated, embellished, and passed on to later generations in French, the learning of which is the primary instrument of political socialization. The French political project thus resonates with particular cultural contingencies that have nothing to do, in themselves, with the revolutionaries’ celebration of fraternity. (Schnapper must also endorse the monumentalization and mythologization of national foundations in order to promote national political projects, something that she freely admits, noting that “all power emanates from the sacred” [p. 169].)

National political projects gain much of their emotional impact from their connection to these cultural contingencies. When the citizens of the first French republic answered the cry “*la patrie est en danger*,” were they really rising just to defend their partners in a revolutionary political project? Or did they associate this project with an image of an inherited cultural community, *la France*, whose territory had been violated by foreign invaders? I do not see how one can extricate national political projects from a broader stream of a community’s cultural heritage without engaging in the kind of overly abstract analysis that Schnapper rejects in the work of Habermas and other defenders of civic nationalism.

National integration is, as Schnapper argues, an extremely powerful force in modern lives, one that deserves considerably more attention than it has received. But it derives that force from a fusion of political socialization and cultural inheritance. The efforts of contemporary social and political theorists to pry apart these two elements of national integration may be morally edifying. But they will not, in the end, help do much to help us either understand or control the effects of nationalism on our lives.

Domesticating Revolution: From Socialist Reform to Ambivalent Transition in a Bulgarian Village. By Gerald W. Creed. University Park: Pennsylvania State University Press, 1997. Pp. xvi + 304. \$55.00 (cloth); \$19.95 (paper).

Chris Hann
University of Kent

Although some sections of this book—detailed descriptions of the functioning of a village canning factory and of agricultural pursuits from strawberries to cattle silage—will be read and appreciated only by a small community of *cognoscenti*, East Europeanist anthropologists, Gerald Creed has a message for wider social science audiences, and he deserves to reach them. First, he has news about the nature of Bulgarian socialist society before 1989, namely that it was very far from being the crudely repressive totalitarian system imagined in the West. Creed's demonstration of the success of the large rural population in "domesticating" socialism from the 1960s onward is perhaps the clearest and fullest application to any Eastern European case of Jim Scott's ideas about how peasants resist domination (though theoretical inspirations are lightly handled and neither Scott nor any other forerunner is followed closely). However, as the first decade of transition comes to an end, this book is likely to be appreciated even more for the insights it offers into the ways in which those same villagers have tried, rather less successfully, to domesticate transformation processes. Creed has been back many times to the village where he began work in the 1980s, and his microdocumentation of recent changes is both fascinating and depressing. The two parts of the book are well woven together: the account of socialism is crucial to understanding the ambivalences of recent years.

The introduction outlines the key themes concerning "the power of everyday practice to reconfigure the very conditions that inform it" (p. 3) and makes a persuasive case for the importance of studying rural transformation in Bulgaria. The very orthodox nature of this case distinguishes the Bulgarian story from those of countries like Hungary and Poland, better known to English readers. Indeed, though Creed does not claim such general validity, his analysis is highly relevant to the predicament of huge numbers of people in Russia and the Ukraine whose experiences during and after socialism have barely been charted at all by western social scientists so far. Chapter 1 provides more historical background locally and nationally and argues that there were sound "objective" reasons for collectivization, to address the problems of the countryside. However, the manner of its implementation largely obliterated an earlier cooperative tradition. Chapter 2 is called "Agricultural Reformations" and is the longest in the book. It includes detail on farm concentration processes and mechanization but also on the dialectic between villagers and powerholders that led to the emergence of popular and flexible institutions of personal plots and family contracting (the *akord* system). Chapter 3

shows that after the accomplishment of a demographic transition in the interwar period, the early decades of socialism were characterized by heavy outmigration, followed in turn by strenuous efforts to keep people in the village. There is good discussion of the motives for migration, including distaste for village life based in part on its heavy work demands. Some evidence for rising fertility in the last years of socialism is taken as support for the "domestication" thesis. Chapter 4 provides a rich discussion of socialist encouragement of rural industry through policies of regional self-sufficiency. Here we see how the new small factories have to adapt to the prior claims of agriculture, notably in releasing workers for farm work at the seasonal peaks. We see processes of interaction and domestication here too, as workers exploit conditions of labor shortage to negotiate better deals for themselves and to vote with their feet when not satisfied.

The ethnography in these chapters repeatedly drives home the importance of social relations, for example, in explaining how a factory comes to be established in a particular location. Chapter 5 is called "Informal Proliferations," and it pursues these points in the broad context of comparative debates about informal activities. Creed follows David Stark and others in distinguishing socialist informality from capitalist variants, where it is congruent with the main organizing principle of the economy. He looks not only at income generation but at a range of political and cultural factors; indeed, all he has to say about kinship, godparenthood, weddings, and housebuilding is subsumed in this discussion. He cautions against attaching undue "negative valence" to bribes and even to theft, showing through examples how stealing from the collective can have beneficial consequences for both informal and formal institutions. The former provides indispensable "lateral integration." Overall, "informal arrangements [are] the ultimate expression of socialism as a system of conflicting complementarity and a major mechanism for ameliorating socialist life" (p. 213). Yet the chapter closes on a much more negative note for which little evidence is provided: "while the integrations and adjustments achieved by informal routes were adaptive locally and empowered villagers, they were also detrimental to the system as a whole" (p. 217).

The last substantive chapter describes processes of land restitution in the 1990s, which have been greatly complicated by changes of government at the center. Some villagers took direct action (self-help) in protest against the legal morass and delays in decollectivization. However, the majority was highly critical of the liquidation of the old collective farm, and they showed their loyalty to collective institutions by flocking to join a new cooperative (it was named Dawn; Phoenix might have been even more apt). Even the villagers' insistence on receiving particular plots of land is interpreted as "another form of resistance to decollectivization" (p. 233). As inflation and unemployment rose and the role of subsistence agriculture intensified, villagers testified eloquently to their feelings of anger and frustration: "We don't have theft in Bulgaria; it's called privatization." Finally, a short conclusion addresses some of the underlying

causes of these attitudes. Creed is surely right to suggest that equality, both as empirical fact dating back to the presocialist period and as enduring cultural ideal, is a factor that helps to distinguish Bulgarian experience from some other cases. He is right to maintain that socialism has a particularly "robust history" in rural Bulgaria. I would only add that the tendency to repeasantization and the threats to "villagers' sense of self" (p. 275) are to be found almost everywhere else in the region as well.

In summary, this is a well-written, no frills, pioneering ethnography, which also makes an important theoretical and political contribution. Creed's knowledge of the field is extremely impressive, though it is a pity that he is not familiar with the studies assembled recently by Ray Abrahams (*After Socialism: Land Reform and Social Change in Eastern Europe* [Berghahn Books, 1996]), some of which offer comparable analyses. On occasion, the theoretical points are not brought as clearly as they might be (this is not to be confused with his insistence on recognizing ambiguity and nuances, which is wholly welcome). For example, Creed's data do not endorse the picture of "atomized" households in confrontation with the socialist state as suggested by István Rév, David Kideckel, and others, but this is not made explicit at the end of chapter 1. Similarly, while making good use of Caroline Humphrey's classic account (*Karl Marx Collective: Economy, Society and Religion in a Siberian Collective Farm* [Cambridge University Press, 1983]), I was not clear how Creed's concept of complementarity takes us further than her notions of "balance" and "compromise." His repeated identification of "conflicting complementarity" seems an awkward term for institutional interdependence and the unintended side effects of state policies. Finally, while Creed salutes the "ingenious achievement" of the villagers who domesticated socialism and who are now continuing their struggle to domesticate the transition, he does not fully explain how the informal activities that played so constructive a role in ameliorating socialism could nonetheless be detrimental to its "practical and ideological foundations" (p. 218). At times, he seems to fall into the trap of identifying socialism only with the formal rationality of the plan, the trap which all his ethnography counsels against. His statement that "in a horrible twist of fate, as socialism became more tolerable, it became less viable" (p. 277) does not seem warranted by his own evidence. True, he adds quickly that the system was actually destroyed by "spectacular attacks" from outside rather than through its internal contradictions and a democratic process of the Bulgarian people, but this important political point could perhaps be made more clearly. Despite these various small criticisms, this book makes an outstanding addition to the anthropological literature on Eastern Europe and to transition studies and the sociology of economic life generally.

The Rebirth of Politics in Russia. By Michael Urban, Vyacheslav Igrunov, and Sergei Mitrokhin. New York: Cambridge University Press, 1997. Pp. xiii+429. \$64.95 (cloth); \$24.95 (paper).

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The collapse of the Soviet Union triggered a major crisis in Soviet studies. For decades, static models versus evolutionary approaches to change structured the discipline. When the Soviet Union collapsed in an abrupt, revolutionary manner, Sovietological theories quickly became obsolete, and Soviet scholars were put on the defensive. Citing their failure to predict the collapse of the Soviet Union, social scientists from other subfields attacked the Sovietological community as a "ghetto" of atheoretical self-absorption, which needed to be destroyed. Soviet and now post-Soviet studies has yet to recover from these charges.

When placed in comparative perspective, Soviet studies has gotten a "bum rap" by being labeled a theoretical backwater. In fact, robust theorizing took place; the theories were just different from mainstream social science models because the case was different. Moreover, that Sovietological models did not predict a single, unexpected change in the stable system is not a failure unique to Soviet studies. Most of the robust models in political science seek to explain stability, not change—equilibria not disequilibria. These theories offer explanations for events (and nonevents) that occur frequently. Variables that precipitate change in equilibria are often treated as exogenous shocks to the model in question. Compared to theories of equilibria, theories seeking to explain change remain underdeveloped in all fields.

In the post-Soviet field, the paucity of theory development regarding change has produced a peculiar dichotomy in recent scholarship. Several important books have been written on post-Soviet Russia. These books essentially begin after the Soviet collapse and treat Russia like any other country in transition. At the same time, a few important studies have been published on the end of the Soviet Union. These books make the Soviet collapse the endpoint of their research. *The Rebirth of Politics in Russia*, written by Michael Urban with contributions from Vyacheslav Igrunov and Sergei Mitrokhin, is one of the first books that seeks to explain change and continuity in the Soviet/Russian political system both before and after the Soviet collapse. This book offers an explanation for the political processes that brought down the Soviet Union as well as an account of how these political changes in the Soviet period have influenced the emergence of Russia's new polity in the post-Soviet period. In tracing the rebirth of Russian politics through the Soviet period and into the post-Soviet period, the book makes explicit reference to the importance of path dependency for understanding Russia's new political system. As stated in the very beginning, "the institutional inheritance be-

queathed by communism to the new Russian polity would complicate the matter of rebirth in far more profound and ramified ways" (p. 3).

As the first major study to tackle head on the dangerous task of explaining this complex transformation, the book already qualifies as a must read for students of social change and scholars interested in Russian politics. However, there are several other reasons to read this book. Perhaps most important, *The Rebirth of Politics in Russia* introduces society and its organizations as critical players in this drama of regime change in the Soviet Union and then Russia. With few exceptions, most studies have cast this transition as an elite event. Urban and his coauthors tell us why this image is flawed. The book chronicles the evolution of nongovernmental associations, from the dissident groups in the 1960s and 1970s, to the "informal" movement in the early Gorbachev period, to the formation of national organizations in the early 1990s.

In tracing the role of societal actors in this transition, however, Urban, Igrunov, and Mitrokhin also pay close attention to decisions taken by state actors. For instance, the book rightly focuses on the role that elections played in stimulating the formation of social actors. In the postcommunist period, the authors then trace how state power defined and limited the development of political society. In constructing an explanation of political change by examining the interaction of state and society, this book provides the most comprehensive account in print to date of regime change in the Soviet Union and Russia.

It is not surprising that the authors convey a sense of intimate understanding of this history as Igrunov and Mitrokhin were both major figures in the dissident and informal movements. They worked together to create an archive of documents and publications of the informal movement—the richest set of documents on this period in the world. Both then helped to found the political organization, Yabloko, in 1993, and both now serve as deputies in Russia's new parliament, the State Duma. Through his close association with Igrunov, Mitrokhin, and dozens of other activists in Russia's informal movement, Professor Urban has enjoyed an intimate view to this transformative process as well.

Because two of the three authors are active politicians in Russia today, some might object to this book as a politicized account of this tumultuous period. It is not. Readers familiar with the Russian political landscape will recognize interpretations in this book as consistent with Yabloko political positions. However, this reviewer was more struck by the absence of a political line. This book is a serious academic study of one of the most important events of the 20th century. It is meticulously documented (their archive served them well), carefully argued, well-written, and serves no political agenda.

If there is one shortcoming to the book, it is the weak connection between the first theoretical chapter and the remainder of the book. In the opening chapter, the authors spell out an interesting set of hypotheses about the relationship between the state, forms of societal organization, and strategies of communication practiced by different social organiza-

tions. In the concluding chapter, cleverly titled, "Neither Dictatorship nor Democracy," the authors then reintroduce these categories. In the more empirical chapters in between, however, the typological construct fades into the deep background.

However, maybe this is not a weakness but a blessing in disguise. The story is too compelling to be interrupted by heavy-handed conceptualization. For this book, the authors may have made the right decision in separating out the theoretical and empirical chapters, as this organizational structure will make the book accessible to a wider audience.

The crisis in Soviet studies precipitated by the collapse of the Soviet Union will gradually give way to a truly exciting period of scholarship for this field. No longer constrained by poor data and unique theories, the transitions underway in the postcommunist world offer a historical testing ground for old theoretical approaches as well as an incubator of theoretical innovation. *The Rebirth of Politics in Russia* is a great example of this promising future.

Iconography of Power: Soviet Political Posters under Lenin and Stalin. By Victoria E. Bonnell. Berkeley and Los Angeles: University of California Press, 1998. Pp. xxii+363. \$48.00.

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This book eloquently demonstrates that combining political analysis, historical perspective, social psychology, and art history can better explain propaganda and political art than can any single discipline. Victoria Bonnell, a sociologist whose earlier work was in prerevolutionary Russian labor history, consistently moors her arguments to the concrete historical landscape. She links the changing imagery of posters to each of the regime's successive "master narratives" and social creeds: October Revolution, Dictatorship of the Proletariat, work ethic, gender roles, and the cults of Lenin and Stalin. She relates poster art to the "invented traditions" that encapsulated the narratives and fashioned model identities. The chapters address representations of workers, urban and rural women, leaders, and the "other" (foreign and domestic enemies), ending with the Stalinist apotheosis of 1945–53, constructed as a divine order of abundance and harmony. This is a physically heavy book, made of fine sturdy paper suitable for reproductions. It is replete with weighty arguments, learned allusions, and thick documentation. It employs the technical vocabulary of several disciplines. For all that, *Iconography of Power* is refreshingly jargon free and, except for occasional repetition, a delight to read. In scope, Bonnell's work goes beyond the early Soviet years examined in the excellent study of Stephen White, *The Bolshevik Poster* (Yale University Press, 1988).

Bonnell explores all the lines and corners of the "cultural diamond" (a

term from the sociologist Wendy Griswold's *Renaissance Revivals: City Comedy and Revenge Tragedy in the London Theatre, 1576–1980* [University of Chicago Press, 1986]), that is the study of cultural artifacts from the angles of creator, work, audience, and the social and political setting. Bonnell provides a wealth of information about the artists, the institutions that commissioned and critiqued their work, and the mechanisms of political control. The mission of poster artists was to match images to social identities mandated by the state. Artists had to balance that mandate with imagery that viewers would accept and understand; as the author puts it “to ‘speak the language’ of the viewer” (p. 7). As the political system hardened in the 1930s, political agendas outweighed other considerations that had been allowed in the 1920s—including the artists’ own expressive signature.

Bonnell meticulously searches the posters for signals about big social and political issues by critically appraising figures, faces, perspectives, background, composition, and color symbolism. Her readings of the blacksmith figure and the tractor are brilliant. Through all of this, she traces change over time and its relation to state policy. Among the most striking themes is the broadening of art’s central metaphors from proletarian man to Soviet citizen, with Stalin as the father of them all. The book offers replicas of posters in eight full-color plates and 92 black-and-white figures. With a hundred pictures at one’s disposal, the reader is given ample scope to follow and sometimes challenge the author’s analysis.

When speculating about visual meanings, Bonnell is modestly suggestive rather than dogmatic. This tone prevails also in her ideas about possible audience reaction to posters. Some evidence on how many people might have seen them is offered by careful reference to print-run figures and distribution techniques. Bonnell examines viewer expectation in the most sensible way, given the relative lack of memoir data on what people thought about the things they viewed. She embraces the notion that “viewing pictures is a learned experience, one that is cumulative over time” (p. xix). She does this by working through many layers of visual art traditions: Russian Orthodox, French Revolutionary, and European socialist iconography. Cautiously, imaginatively, and rigorously, Bonnell tries to figure out what different kinds of viewers brought to the moment when they saw posters. To assist in this, she invokes intertextual relations of posters to statuary, festivals, films, theater, and fiction.

Using printed sources and other conventional historical materials, the author analyzes the motivations and confirms the tight control that party leaders had over the outpouring of political and social messages through cultural artifacts. Though there can always be doubt about how Soviet citizens actually saw the world in the 1930s and 1940s, there can be little doubt about how the authorities wanted them to see that world—Soviet life, the regime, and the outer darkness of “capitalism” and “fascism.” As the author puts it, the purpose of Soviet political art of that era “was to provide a visual script, an incantation designed to conjure up new modes

of thinking and conduct, and to persuade people that the present and the future were indistinguishable" (p. 14). This attractively produced and intelligently written book will stimulate discussion, arouse theoretical interest, and excite students who are lucky enough to read it.

Communists and National Socialists: The Foundations of a Century, 1914–39. By Ken Post. New York: St. Martin's Press, 1997. Pp. xi+222. \$65.00.

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After the "end of history," it is no small undertaking to attempt to update Marx by revisiting the Russian and the Nazi revolutions. Drawing on world systems analysis, Ken Post writes early 20th-century history to re-write theory for the revolution we still await. The resulting tale is instructive, though not always in ways the author intends. His book is largely a study in the limitations and contradictions of Marxism, which in the final analysis appear more serious than those Marxists ascribe to capitalism.

The historical questions are clearly articulated: Why did revolution occur in czarist Russia and not a more developed capitalist state, and why did Hitler rather than socialism capitalize on regime crisis during the depression in Germany? Neither question is novel, nor are the answers unfamiliar. It is their presentation in the terminology of periphery (Russia) and core (Germany) and over against the theoretical expectations of socialist contemporaries that frames their relevance to a critique of early 20th-century Marxism. Post rationalizes revolution on the periphery in terms, first, of the asynchronous character of economic and political developments in Russia, which was at once incorporated into global capitalism and relatively underdeveloped and, second, in terms of the existence of a revolutionary party. Neither condition was obtained in Germany, where a more complex social capitalism proved resistant to socialism despite economic and political crisis and where communism was not an "organized revolutionary force with its own terrain and space" (p. 78). Post therefore concludes that revolution on the periphery, not at the core, was historically appropriate.

From similar premises, non-Marxists have constructed theses of distinct national trajectories (notably the German *Sonderweg*), which explain divergent responses to the pressures of war and economic crisis. Such theses also recognize the vulnerability to revolution of states caught in asynchronous development, particularly when engaged in war. Post, however, also seeks to explain socialist misreading of events in Russia and Germany. He identifies the blind spot as early Marxism's economic determinism, the belief that economic contradictions would destroy capitalism. Marxist theory consequently downplayed the importance of poli-

tics, culture, and nationalism: "the creators of real dialectical thought thus ended up with a totally undialectical historicist inevitability" (p. 174). They also falsely assumed that socialism could proceed to victory from within the parliamentary system. When in the 20th-century social capital spawned core and periphery capitalisms and took predominantly bourgeois democratic form, Marxist theory found itself badly outstripped by history.

This argument, while generalized and again not new, could prove a point of departure for rethinking Marx. Historically, however, it sits awkwardly with Post's criticisms of German communism (KPD) in the '20s. Post reprimands the party for squeamishness about bloodshed, for failure to work out a viable revolutionary strategy, and for accommodation to the Weimar system. As a political force, it compared unfavorably to the NSDAP, which did succeed in creating a distinct as well as broad political space. Yet, if one concedes, as Post rightly does, that circumstances in Germany militated against socialist revolution, one does not require communism's fumbblings to explain the failure of revolution. The KPD's "version of Marxism" certainly caused them to misread Nazism, but that they could have outmaneuvered Hitler to gain power with a different version remains more than doubtful. Whatever "good sense" kept German voters from electing Nazis until the depression operated even more efficiently against communism. Thus, the failure of the party to build a "concrete revolutionary terrain" was indeed characteristic but also systemic. Historically, German communism could not have duplicated Nazism's accomplishment, not least because the triumph of Bolshevism in Russia horrified the entire German middle class and alienated much of the working class.

In light of Post's contention that "social capital on a global scale was sufficiently strong to survive major economic depressions, and so complex that it required a general war to create a revolutionary situation in one part of its periphery while none arose at its center" (p. 187), this story leaves prospects for socialist revolution elusive. Indeed, the book offers compelling evidence for the creative and adaptive capabilities of capitalism, organized into national blocs, thus able to channel its contradictions into war and flexible enough to accommodate a Hitler to preserve itself. By Post's own description, bourgeois democracy was and remains social capital's most effective political form, giving wide representation and allowing the "best informed and most sophisticated formulation of policies for managing the expansion and reproduction of social capital" (p. 194). Marxist theory and practice remain a distant rival, still inadequate to the challenge. From this perspective, Weimar's socialists appear prescient rather than unimaginative in choosing constitutional politics over revolution. Unlike Marxism's founding theorists, they recognized that "given a certain trajectory of capitalist development, the conjunctural preconditions for a true revolutionary situation were extremely hard to find" (p. 195). The moral of this tale is thus not promising for the Marxist project.

Democracy and Authority in Korea: The Cultural Dimension in Korean Politics. By Geir Helgesen. Richmond: Curzon Press, 1998. Pp. xiii+321.

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Geir Helgesen's *Democracy and Authority in Korea: The Cultural Dimension in Korean Politics* attempts to explain Korean politics from the "classical political cultural approach" pioneered by American political scientists such as Sidney Verba and Lucian Pye. The book's main theme is to identify the roots of Korean political culture and the influence of those roots on Korean politics, especially the process of democratization, primarily based on two large surveys of political and national consciousness conducted in Korea by the author in 1990 and 1995. Helgesen's text also examines the process of political socialization and discusses future prospects for Korean democracy.

Helgesen regards "deep-rooted familism," based on Shamanism and Confucianism, as the guiding principle of Korean political culture. This familism encouraged "psychological dependency," "we-collectivism," and "filial piety," hindering the establishment of Western-style democracy in Korea. This familism was preserved in Korean society through "moral education" aimed to "promote nationalism, anti-Communism, and general ethical norms for the daily life of the people" (p. 155). As a result, Korea's traditional cultural values came to be deeply rooted in its politics, fostering a conflict with Korea's effort to democratize, win acceptance by the global community, and adopt more universalistic values. Finding that it is impossible to uproot traditional cultural values, Helgesen goes on to suggest the need for a Korean democracy that can better integrate indigenous values to the Korean political system, presumably in the form of "Confucian democracy."

I found this book highly informative and at times even provocative, offering many interesting survey findings largely unknown to English-speaking readers but which I cannot summarize here. The author also should be commended for his research efforts, especially conducting two large surveys and many valuable in-depth interviews supplementing the surveys. Nonetheless, some of his arguments are disputable.

First of all, the reader will be struck by the text's highly conservative picture of Korean political culture, illustrated by very negative attitudes found in the surveys, toward student activism and labor movements. I do not dispute the finding itself since Korea was no exception in making a "conservative turn" after the demise of the left in the 1990s, and recent revival of interest in Confucianism among Koreans attests to this point. Nonetheless, not only should the recent conservative shift in Korean politics be better contextualized, but there is great danger in speaking of a *single* Korean political culture. Student activism, a major force of Korean oppositional politics, commanded a high level of popular support in the 1980s, and if Helgesen's surveys had been conducted then he would have

found *minjung* ideology (a major Korean oppositional ideology that guided Korea's quest for democracy in the 1980s) to be a key part of Korean political culture. *Minjung* ideology, of course, could not be attributed to Confucian familism.

Second, as much as "politics are imbedded in culture," so is culture shaped by history and social structure. For instance, the author argues that regionalism could be seen as a natural outcome of Korean political culture, that is, its "we-group mentality," and thus as a "potential strength" (p. 207). This observation, however, can be misleading when taken out of the historical context in which Korean regionalism developed; it was not so much a natural growth of Korean political culture as an outcome of discriminatory policy by several authoritarian regimes. Similarly, that Koreans have "very little confidence in the political leadership" (p. 108) might derive as much from political culture as from the historical experience of the inability of Korea's political leadership to prevent colonization by the Japanese and, subsequently, national division upon liberation.

Third, the notion that democracy in Korea "was established by the US Military Government as a Western-style political system" (p. 94) is highly disputed. Bruce Cumings, an eminent Korea specialist, certainly does not think so (see his *The Origins of the Korean War* [Princeton University Press, 1981]); neither did the many Korean activists who fought for democracy in the 1980s, demanding national liberation from foreign influence, especially that of the United States, which they perceived not as democratizing but as integral to the establishment and maintenance of authoritarian rule in Korea. Various public polls conducted in the mid-to late 1980s likewise show that many Koreans viewed the rise of anti-Americanism in Korea to be the result of America's support of Korean dictatorship.

Finally, while the author's recommendation for a "Koreanized democracy" based on indigenous political culture, especially Confucianism, may sound appealing, this should be approached with caution. It was the dictatorial Park Chung Hee, who ruled in the 1960s and 1970s, who first advocated a "Koreanized democracy" (*han'gukjók minjujuui*), selectively incorporating Confucian elements such as respect for the authority, loyalty to the nation, and filial piety. Given such prior abuse of the notion of a "Koreanized Confucian democracy," I very much doubt that it would be a viable alternative to that of a "Western-style democracy" in Korea. All in all, the reader will find in this text many informative and valuable findings on recent trends of Korean political consciousness, but some of the arguments will be controversial and thus disputed.

Neoliberalism and Class Conflict in Latin America: A Comparative Perspective on the Political Economy of Structural Adjustment. By Henry Veltmeyer, James Petras, and Steve Vieux. New York: St. Martin's Press, 1997. Pp. xiv+258. \$49.95.

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The peculiar distinction of the Trotskyite persuasion in social analysis is to see potential capitalist crisis in every economic blip and potential proletarian revolution in every popular social movement, if only it were not betrayed. According to Henry Veltmeyer, James Petras, and Steve Vieux, Latin America is reaching the end, or may already have reached the end, of what they call its neoliberal "policy cycle" and hence is in crisis. Further, in their view, center-left governments in tandem with externally funded nongovernmental organizations are the most recent traitors to the Latin American working classes, who have been battered by the neoliberal restructurings carried out in the name of free markets and whose multifarious struggles against capital and the state could usher in a "new alternative economic model" (p. 227). Unfortunately for possible readers of this book, dubious reflections, such as these, overwhelm the occasional insights provided by the author's class analyses of imperialism and national politics.

Rather than the "comparative perspective" advertised in the subtitle, the authors offer a composite picture of Latin America from about 1975 to 1995, with Chile and Mexico furnishing the examples of conversion to neoliberalism, Bolivia furnishing the example of nefarious NGOs at work, and Brazil, the landless, and Mexico (the Zapatistas and El Barzon—the debtors' movement) furnishing the examples of potentially revolutionary masses in motion. If as a child (or an adult) you ever played with Mr. Potato Head, you will recognize the technique at work here: patchwork synechdoche. While it might be successful rhetoric to take a region's most brutal political figures, its most disastrous economic moments, and its most valiant oppositional movements to stand for the whole, this technique does not work terribly well as social science.

More's the pity, because Veltmeyer, Petras, and Vieux do in fact summarize and present some very useful if not particularly original materials on the region's changing class structure, at least through the 1980s. These data show serious to precipitous declines in real urban minimum wages (except for Chile) and in employee earnings as a percentage of value added in manufacturing. They show the sharply unequal distribution of income. They show the increase in both the number of billionaires and the size of their immense fortunes. The authors also show that these outcomes resulted from the "Washington consensus" on fiscal and monetary policy as enacted via Structural Adjustment Programs.

Still more useful, though again not original, is the authors' interpretation of the shift to neoliberalism as a policy in the first place (pp. 111–

13). First they review the causal contributions of (1) the ascendant Latin American capitalist class segment linked to export markets and overseas banks, (2) the collapse of Eastern bloc "socialism"—an accelerator perhaps, but hardly a cause, they correctly point out—and (3) the roles of the IMF and the World Bank. They then echo the position of most world-system analysts by arguing that "the strategic economic needs of the U.S. state" (p. 113) in its competition with Germany and Japan have been decisive. Regional bloc formation à la the European Union and the ASEAN pact has enabled the United States to offset part of its deficits with Asia and Europe, thereby buffering its decline in global competitiveness.

In the last third of the book, Veltmeyer, Petras, and Vieux turn to the politics of social movements. Their chapter 9 provides a useful chronology of the Movimento dos Sem Terra (Brazil) from its founding in 1984 to the summer of 1994, before the October national election. Yet, many analytical questions go unasked, let alone answered: which groups (renters, sharecroppers, squatters, proletarians) have benefited and to what degree; why did such a movement arise in Brazil and not elsewhere; how has this organization managed to define itself as a working-class movement?

Chapter 10, on Chiapas, is even thinner. It makes analytical arguments, but they are muddled or contradictory. The authors claim, for example, that the Zapatistas' existence punctures the "myth" that "popular guerrilla movements are no longer viable" (p. 200), when clearly the movement is barely surviving, thanks to its sympathizers and allies in the rest of Mexico and internationally. The authors assert that the Zapatistas "have demonstrated that revolutions are not products of competition between state systems" (p. 200)—this is a revolution?—while arguing on the following pages that the movement is a response to the "success of the neoliberal SAP" introduced by a government desperate not to be left behind, by a ruling class that must "pay for its entry into the First World" (p. 201). Again, the interesting analytical questions are neglected in favor of radical posturing: if in fact Chiapas is "*representative* of wide-reaching socioeconomic changes that have polarized the countryside" (p. 201, emphasis theirs), why is it virtually the only region with a new (quasi-)armed movement? In their embrace of symbols of militant popular opposition, the authors have simply not done their homework in the burgeoning literature on Mexican rural politics in general and on Chiapas in particular.

In sum, for all the clarity with which Veltmeyer, Petras, and Vieux see certain broad contours of the emergent Latin American scene—increased U.S. (and other foreign) investment, heightened class polarization, shallow if not sham democratization, working-class fragmentation and disorganization—their Trotskyite imaginary leaves them ill-prepared to understand the future. Far from decaying, neoliberalism is here to stay for the medium term. One can reasonably hope that over the next 20 years, popular movements, perhaps in tandem with hemispheric Keynesianism from above, will lead to deeper democratization and a reversal of polarization. Envisioning and thereby encouraging armed revolutions is irresponsible nostalgia, faithful to Trotsky's political dreams but not to his intellectual acumen.

Empirics of World Income Inequality¹

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This article employs a common general formula for inequality indexes to answer several basic questions about intercountry income inequality in recent decades: Has inequality across nations increased or declined (and why have earlier studies yielded mixed results)? Have different rates of population growth played a significant role in the trend? Have large nations dominated the trend? Are the results robust over different inequality measures and different income series? Two findings stand out. First, different rates of population growth in rich and poor nations played the predominant role in determining change in the distribution of per capita income across nations. Second, the centuries-old trend of rising inequality leveled off from 1960 to 1989. The dependency theory thesis of a polarizing world system receives no support.

The Industrial Revolution produced a stunning increase in the income disparity between nations. At the beginning of the 19th century, average incomes in the richest nations were perhaps four times greater than those in the poorest nations. At the end of the 20th century, average incomes in the richest nations are *30 times* larger—annual incomes of about \$18,000 versus \$600 (Summers, Heston, Aten, and Nuxoll 1994).

Is the income disparity between nations still increasing? The answer to that question is critical. Because of the great disparity in average income from nation to nation, it is intercountry inequality—not inequality within nations—that is the major component of total income inequality in the world today. A recent sociological study (Korzeniewicz and Moran 1997, table 2) estimates that inequality across countries accounts for *over 90%* of current world income inequality as measured by the Gini index. Other studies give lower estimates, but all agree with Berry, Bourguignon, and Morrisson (1983*b*, p. 217) that “it is clear that the level of world inequality

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is . . . primarily due to differences in average incomes across countries rather than to intra-country inequality.”

Although I begin with the question of whether intercountry inequality is still rising, the analysis does not end there. My aim is to provide the foundation for a general sociological literature on intercountry income inequality by *getting the facts right* about its key dimensions: Whether increasing or declining, is the trend in intercountry inequality due to differential economic growth across nations or to differential population growth across nations? Which countries contribute most to change in intercountry inequality? Are results robust over different inequality measures and income series? Although sociological studies of these important issues are rare,² there is no good reason for sociologists to continue to shy away from studying intercountry inequality. Careful income estimates are available for over 100 nations, which constitutes a near-universe of the world's population (Summers et al. 1994), and convenient methods have been developed for analyzing income inequality for aggregates (Firebaugh 1998). The time is ripe for systematic sociological research on intercountry inequality.

CONVERGENCE THEORY

Convergence Theory in Economics

The convergence issue—the issue of whether national economies tend to converge or diverge over time—has been a central concern in economics over the past decade. Many economists are keenly interested in the convergence issue because recent debates over the nature of economic growth turn on it. The traditional view, based on neoclassical growth theory (Solow 1956), is that national economies will tend to converge because of the principle of diminishing returns to capital and labor. As rich industrial nations begin to experience diminishing returns, poorer nations (who are farther from the point of diminishing returns) will tend to catch up as they industrialize. DeLong (1988, p. 1138) puts it this way in the *American Economic Review*: “Economists have always expected the ‘convergence’

² Despite the overriding importance of intercountry inequality to global inequality, sociologists have focused instead on *within*-country income inequality (Cutright 1967; Weede and Tiefenbach 1981; Bollen and Jackman 1985; Hoover 1989; Nielsen and Alderson 1997). There are three apparent reasons for this focus. First, intranational inequality is more amenable to state policy. There are no international organizations with the muscle to force significant income transfers from rich to poor nations. Second, intranational inequality can be studied with cross-sectional data, whereas the study of intercountry inequality requires longitudinal data. Third, the psychic costs of inequality likely are greater for inequality within nations, since feelings of relative deprivation presumably derive more directly from local comparisons.

of national productivity levels. The theoretical logic behind this belief is powerful. The per capita income edge of the West is based on its application of the storehouse of industrial and administrative technology of the Industrial Revolution. . . . The benefits of tapping this storehouse are great, so nations will strain every nerve to assimilate modern technology and their incomes will converge to those of industrial nations."

The new "endogenous growth theory" (Romer 1986; Lucas 1988) challenges the convergence thesis by arguing that in today's world the principle of diminishing returns can be overcome by specialized inputs made possible by research. The new theory has had a powerful impact in economics. By questioning neoclassical growth theory, endogenous growth theory has reopened the debate about convergence and prompted a new generation of growth studies in economics. Economic research on national convergence is booming. In the words of Robert Solow, one of the founders of neoclassical growth theory, there is a "wildfire revival of interest in growth theory . . . [that] shows no signs of petering out" (1994, p. 45).

One popular test for convergence is to regress economic growth rate on initial economic level for some sample of nations. A negative slope indicates convergence (poorer nations tend to grow faster) and a positive slope indicates divergence. Though many cross-national studies find evidence of *conditional* convergence—that is, the initial economic level has a negative slope when control variables are added (Barro 1991; Barro and Sala-i-Martin 1992; Mankiw, Romer, and Weil 1992)—the recent consensus in economics is that the world as a whole has exhibited no tendency toward *unconditional* convergence in recent decades (Quah 1996; Jones 1997), where "unconditional" refers to the absence of control variables.

The Polarization Thesis

Although sociologists have no formal theory of intercountry income inequality to rival convergence theory in economics, prior sociological studies of intercountry inequality have used world system and dependency perspectives to predict increasing cross-nation polarization and thus rising intercountry inequality (see Korzeniewicz and Moran [1997] for an overview). World system theory begins with the premise that there are identifiable strata in the world economic system. The polarization thesis adds the argument that there are powerful forces driving these strata apart. Although polarization theorists sometimes identify intermediate strata (e.g., the "semiperiphery"), the perspective tends to be dualistic, placing greatest emphasis on the mechanisms by which nations at the top become richer at the expense of nations at the bottom.³

³ As Korzeniewicz and Moran (1997) note, the intellectual roots of current polarization theory can be found in Marxist theories of imperialism (Lenin 1939), in the Prebisch-

Even though world system theorists usually tell a divergence story, there is nothing inherent in a world system perspective that would rule out convergence stories. Dependency theory, by contrast, is incompatible with convergence stories; removing the polarization thesis from dependency theory would hollow out the theory. Dependency theory is a theory of world stratification—of why some nations are so rich and others are so poor. The dependency theory thesis—that the development of core nations and the underdevelopment of peripheral nations are complementary processes—is inherently a polarization thesis. Core nations enrich themselves *at the expense of poor nations* because core nations differentially benefit from core-periphery economic exchange. Thus in dependency theory the principle of differential benefits from exchange (Mandel 1975; Bunker 1984)—not the principle of diminishing returns—is the main-spring for trends in intercountry inequality. The principle of differential benefits from exchange implies a “growing gap between core and periphery” in the world economy as a whole (Chase-Dunn 1975, p. 720). “A picture of unequal development emerges in which the core becomes progressively more developed while peripheral development is hindered as a result of its relationship to the core” (Peacock, Hoover, and Killian 1988, p. 839). In effect, then, dependency theory argues that Marx’s principle of uneven development applies to the world economy as a whole rather than to groups within individual industrial nations (Chase-Dunn 1975).

Because polarization is fundamental to the dependency argument, the trend in intercountry income inequality in recent decades provides a critical test of the relevance of dependency theory to the current era. If rich nations benefit more from international exchange, *and if this exchange is the primary source of national income differences* (as dependency theory insists), then so long as rich and poor nations continue to engage economically we can expect national incomes to continue to diverge. If intercountry income inequality is not increasing, then adverse dependence effects either do not exist or are overshadowed by other forces. Either way, dependence effects do not pack the punch assumed in dependency theory.

If intercountry income inequality *is* increasing, it is important to determine if the increase is due to the faster population growth of poorer nations in recent decades. Income is measured as income per capita. If income per capita has grown more slowly in poorer nations in recent decades, that slower growth might be due to the swelling of the young

Singer theory of deteriorating terms of trade for primary products (Prebisch 1950; Singer 1950), and in the world system (Wallerstein 1974, 1979) and dependency (Frank 1966; Cardoso 1977) formulations of the 1960s and 1970s. Although these theories offer different reasons for expecting disparity between the strata, according to Korzeniewicz and Moran (1997, p. 1004) they “all would agree that world income inequality becomes more pronounced over time.”

(nonworking) population in poor nations quite apart from any effects of international economic exchange. In analyses of trends in intercountry income inequality, then, it is important to distinguish *population-induced polarization* from *dependency-induced polarization*—a theme I return to below.

CROSS-NATIONAL EVIDENCE

Cross-national studies of convergence appear at first glance to present a mishmash of conflicting results. In this section I show that consistent findings do emerge when the studies are sorted carefully. I will also show that sociologists should not be too hasty to use the findings from economics to reach conclusions about trends in world income inequality because economists and sociologists are asking different questions.

It is useful first to place the convergence studies in historical perspective. At the outset of the Industrial Revolution average income in the richest nations was perhaps four times the average income in the poorest nations (Maddison 1995, chap. 2). Average income in the richest nations and poorest nations now differs by a factor of about 30. Over the long haul, then—from the late 18th century through much of the 20th—national incomes diverged. No one disputes that.

The more vexing question is what has happened since about 1960. Some studies conclude that there has been little or no change in intercountry inequality in recent decades (Berry et al. 1983a; Peacock et al. 1988; Schultz 1998) whereas other studies conclude that national incomes have continued to diverge (Jackman 1982; Sheehey 1996; Jones 1997; Korzeniewicz and Moran 1997).

There are three keys to making sense of these findings. The first key is weighting. Studies that do not weight generally find divergence, whereas studies that weight generally find very little change in intercountry inequality over recent decades. The second key is whether or not the national income data have been adjusted for “purchasing power parity” (PPP—elaborated in a subsequent section). The use of unadjusted data results in spurious divergence (Schultz 1998). The third key is China. Weighted studies that exclude China are suspect.

Studies of Unweighted Convergence

Table 1 summarizes key convergence studies from economics, sociology, and political science. In each of these studies the dependent variable is per capita income. Note that the income measure of choice is based on purchasing power parity; among recent studies only Korzeniewicz and Moran (1997) rely exclusively on income estimates that are based on the foreign exchange method.

TABLE 1

SUMMARY OF MAJOR STUDIES OF NATIONAL INCOME CONVERGENCE*

Study	Data and Method	Conclusion
Unweighted by population: Jackman (1982, table 1, fig 1)		
Abramovitz (1986)	Income growth rate, † 1960–78 ($N = 98$), regression of rate on initial level 1870–1979 income, ‡ coefficient of variation; 16 industrial nations (from Maddison 1982) Same historical data as Abramovitz (1986), but uses regression	Divergence with inverted-U pattern Long-run convergence among rich nations Long-run convergence among rich nations
Baumol (1986)	Income growth rate, † 1960–85 ($N = 98$); regression of rate on initial level	Divergence
Barro and Sala-i-Martin (1992, table 3, fig. 4) ...	Income growth rate, † 1960–88 ($N = 107$ non-OPEC nations) 1960 and 1990 income ($N = 74$); ‡ SD of logged income	Divergence with inverted-U pattern Divergence for world but rich nations converge
Sheehy (1996, table 2) ...	1950–77 income ($N = 124$); ‡ Gini, Theil, mean log deviation, Atkinson	No overall trend
Jones (1997, tables 2, 3) .		
Weighted by population: Berry et al. (1983a)		
Peacock, Hoover, and Kil- ian (1988, figs 1, 2) ...	1950–80 income ($N = 53$); ‡ Theil	No overall trend, with convergence within world system strata and divergence between strata
Ram (1989): Table 1	1960–80 income ($N = 115$, excludes China); ‡ Theil§	Divergence
Table 2	1960–80 inequality trend ($N = 21$); regression of overall Theil on mean world income, 1960–80, excludes China in the Theil§	Inverted-U pattern
Korzeniewicz and Moran (1997)	1965–90 income ($N = 121$); ‡ Gini, Theil	Divergence, especially in 1980s
Schultz (1998)	1960–89 income ($N = 120$); ‡ Gini, variance of logged income, Theil	No trend for purchasing power parity income; divergence for foreign exchange (FX) rate income

* Because the object of this study is change in intercountry inequality, the table is restricted to studies of *unconditional* convergence, a term that refers to the absence of control variables. In regression analysis, unconditional convergence is examined by regressing growth rate of income per capita on initial level of income per capita. Conditional convergence is examined by adding control variables.

† Income estimates are based on foreign exchange rates

‡ Income estimates are based on purchasing power parity (PPP).

§ The significance of excluding China in weighted analyses is addressed in the text. I do not note the unweighted analyses (top panel) that omit China because the omission of China hardly matters in those studies

|| Income estimates are based both on foreign exchange rates and on purchasing power parity.

The top panel of table 1 summarizes studies that do not weight nations by size and the bottom panel summarizes studies that do. I begin with the studies in the top panel. One of the earliest reliable studies of cross-national convergence is Jackman's (1982) study of the relative income growth rates of 98 nations from 1960 to 1978. Jackman found an inverted-U pattern for the relationship between income growth rate and initial income—a pattern that was subsequently replicated in studies using different income measures and longer time periods (e.g., Summers and Heston 1991, table 4; Sheehey 1996). Despite this faster growth in the middle of the distribution, there is *overall* divergence because growth rates tend to be higher for the richest nations than for the poorest nations. Subsequent research has replicated the divergence finding as well (Barro and Sala-i-Martin 1992, table 3 and fig. 4; Sheehey 1996, table 2; Jones 1997, tables 2 and 3).

In short, when each national economy is given the same weight—the sort of convergence that interests economists because it bears on endogenous growth theory—there is an inverted-U pattern in which nations in the upper middle of the distribution tend to exhibit the fastest rates of income growth and those at the lower end of the distribution tend to exhibit the slowest rates of growth. The upshot is that national economies are diverging for the world as a whole even though there are convergence “clubs” (e.g., there is evidence of income convergence among Western European nations; see Abramovitz 1986; Baumol 1986; Jones 1997).

Studies of Weighted Convergence

Although it is *weighted* national convergence that bears most directly on sociologists' interest in world inequality (see below), evidence on weighted national convergence is relatively scarce. In sharp contrast to the large and growing literature on unweighted convergence, the empirical literature on weighted convergence across nations consists of just a handful of studies.

The early study by Berry et al. (1983a) remains one of the best of these studies. Based on a large sample of nations containing most of the world's population, Berry et al. conclude, first, that economic growth in China was the most potent force equalizing world incomes from 1950 to 1977 and, second, that there was no clearcut trend in intercountry income inequality from 1950 to 1977.

Remove China, then, and the data will show weighted divergence—precisely what Ram (1989) found for 1960–80 with China removed. Include China and the data will show no overall trend in intercountry income inequality in recent decades—precisely what Peacock et al. (1988) and Schultz (1998) found, replicating the main conclusion of Berry et al. (1983a). So the studies are quite consistent: Weighted by population, the

data show no underlying trend in intercountry income inequality over recent decades; remove China, and the data show rising inequality.⁴

Only one key finding remains to be explained: Korzeniewicz and Moran's (1997) anomalous finding of rising intercountry inequality despite their inclusion of China. Schultz (1998) provides the key to the puzzle. Schultz presents two sets of findings, one for income data based on purchasing power parity (PPP) and one for income data based on foreign exchange (FX) rates (the type of income data used by Korzeniewicz and Moran). Intercountry income inequality rises for the FX income series but not for the PPP income series.

The important lesson to be learned from Schultz's (1998) two sets of findings is that researchers should not rely on official exchange rates when studying trends in relative national incomes. Though early studies in economics used FX estimates because PPP estimates were unavailable, PPP-based income is now the industry standard (in addition to the studies listed in table 1 above, see Barro 1991; Mankiw et al. 1992; Levine and Renelt 1992; Quah 1996). The rationale for the switch to PPP income measures will be elaborated later.

To summarize: When each national economy is given the same weight, the data indicate national divergence. Yet weighted studies find stability (the weighted studies that find divergence do so because they exclude China or use dubious income data).⁵ So the issue turns on weighting: Do we want to give nations or individuals equal weight?

⁴ Careful readers who compare this literature summary table with a similar table in Korzeniewicz and Moran (1997, table 1) might be puzzled by several inconsistencies. The most important inconsistency involves the findings of the Peacock et al. (1988) and Ram (1989) studies. Contrary to the Korzeniewicz-Moran table, Peacock et al. (1988, figs. 1 and 2) found no overall trend, and Ram (1989, table 1) found divergence.

⁵ Table 1 above summarizes key studies of *unconditional* convergence. Scattered throughout the social sciences are dozens of cross-national regression studies of economic growth that include initial income as a regressor and thus provide evidence (often unwittingly) on *conditional* convergence. Because these studies most often are focusing on an issue other than convergence, the sign of the coefficient for initial income often escapes notice. Typically that sign is negative, which provides support for conditional convergence (see, e.g., Firebaugh 1983; Barro 1991; Barro and Sala-i-Martin 1992; Mankiw et al. 1992; Levine and Renelt 1992; Sheehy 1996, table 3; for contrary evidence, see Quah [1996]). This negative *partial* effect does not imply that intercountry inequality is declining, since it is *unconditional* convergence that bears on trends in intercountry inequality. Finally, to complete the picture, Levy and Chowdhury (1994) estimate that aggregate income (*total* income, not per capita income) converged cross-nationally at the rate of about 0.6% annually from 1960 to 1985. The convergence of aggregate income does not tell us very much about intercountry inequality, however, because the growth of aggregate incomes has been largely a function of nations' population growth in recent decades. When population

Weighted versus Unweighted Convergence

Sociologists and economists are interested in intercountry convergence for different reasons. The stimulus for many economists is theoretical, to test theories of macroeconomic growth. Very often for economists, then, each nation represents one unit (one economy) and, in typical analyses, economic trends in Luxembourg count just as much as economic trends in China, even though China has nearly 3,000 times more people. By contrast, sociologists generally study intercountry income inequality because of what it can reveal about income inequality for the world as a whole (Korzeniewicz and Moran 1997), so sociologists are interested in whether there is intercountry convergence in the case where *individuals, not nations*, are given equal weight. Thus most sociologists are interested in *weighted* convergence.

To appreciate the point that it is weighted national incomes that determine the level of world income inequality, suppose we had income data for every individual in the world. Then we could calculate total world inequality using some summary measure such as the Theil index. It is important to note that we could partition this measure of total inequality into a “within-nation” and a “between-nation” component (Allison 1978): World inequality = between-nation inequality + within-nation inequality. As noted earlier, students of world inequality agree that between-nation inequality is by far the larger component.

The partitioning of world inequality into between- and within-nation inequality is analogous to the familiar ANOVA partitioning of variance as total variance = between-group variance + within-group variance:

$$\sum_j \sum_i (X_{ij} - \mu)^2 / N = \sum_j n_j (\mu_j - \mu)^2 / N + \sum_j \sum_i (X_{ij} - \mu_j)^2 / N, \quad (1)$$

where j indexes group, i indexes individual, and μ denotes mean. Observe that the between-group component, $\sum_j n_j (\mu_j - \mu)^2 / N$, is *weighted by group size* (n_j), so group effects in ANOVA are effects based on the *equal weighting of individuals*. The partitioning of total *inequality* is governed by the same principle: the between-nation component in the inequality identity (total = between + within) is a *population-weighted* component. In studying intercountry inequality, then, sociologists should weight nations by their population size because—as Korzeniewicz and Moran (1997) note—the objective is to gain leverage on total world inequality.

Weighting is likely to matter a lot in the case of intercountry inequality because nations vary so much in population size. Large nations such as

grows significantly faster in poorer nations than in richer ones—as in recent decades—aggregate incomes converge.

TABLE 2
THE 1960-89 TRENDS IN INTERCOUNTRY INCOME INEQUALITY WEIGHTED VERSUS
UNWEIGHTED RESULTS

YEAR	AVERAGE WORLD INCOME*		INEQUALITY (VarLog)	
	Weighted	Unweighted	Weighted	Unweighted
1960	2,277	2,294	.91	.74
1965	2,660	2,729	1.04	.84
1970	3,118	3,266	1.08	.90
1975	3,426	3,761	1.11	.96
1980	3,835	4,303	1.07	1.02
1985	4,059	4,421	.96	1.08
1989	4,367	4,826	.96	1.18
1960-89 change (%)	+92	+110	+5	+59

SOURCE.—Summers et al (1994)
NOTE —Real gross domestic product per capita is given in constant U.S. dollars (variable RGDPCH in the Penn income series, ver. 5.6), *N* = 120 nations containing 92%–93% of the world population
* Average per capita income for the 120 nations, in constant U.S. dollars. “Weighted average” indicates that the national means are weighted by population size

China and India affect the weighted measure but have little effect on the unweighted measure, and the reverse is true for small, rich nations such as Luxembourg and Norway.

To verify the importance of weighting, table 2 presents the weighted and unweighted trends in intercountry inequality from 1960 to 1989 (I use 1989 as the endpoint because the dissolution of the Soviet Union interrupts the income series at that point). I use variance of logged income (VarLog) because it is the inequality measure most often used in economic studies. Table 2 reports the results for five-year intervals.

The difference between the weighted and unweighted results is striking. The unweighted results confirm economists’ findings of divergence. But when nations are weighted by size, intercountry inequality increases monotonically until 1975 and declines thereafter; as a result, there is little net change in inequality from 1960 to 1989.

A CONVENIENT METHOD FOR STUDYING INTERCOUNTRY INEQUALITY

In this section I explain the common general formula for inequality indexes that I will use in the analysis of intercountry inequality. The common formula is a powerful tool for studying intercountry inequality because it provides a convenient way to compare the contributions of China,

of India, of the United States, and so on, to trends in intercountry inequality as measured by each of the standard inequality indexes.

My method is based on the insight that inequality measures are weighted functions of X/\bar{X} (the ratio of X to the mean of X) calibrated to zero when the ratio is 1.0 for all units. Even though this insight is implicit in prior econometric work (see formulas in Atkinson [1970], Cowell [1977], and Shorrocks [1980]), neither economists nor sociologists have fully appreciated the leverage it provides in analyzing inequality trends. This paper illustrates one way to exploit the insight.

It is instructive to begin with the fundamentals. By definition a quantity X is equally distributed over the N units in a population when $X_i = \bar{X}$ for all $i = 1, 2, \dots, N$. Assuming X —a nonnegative quantity—exists in the population (so it is not zero), the equivalent *ratio definition of equality* is $r_i = 1.0$ for all $i = 1, 2, \dots, N$, where $r_i = X_i/\bar{X}$. I hereafter refer to r_i as the “income ratio,” since in this study X is income.

Inequality is the absence of equality. Thus inequality occurs when r_i does not equal 1.0 for all i . The greater the average departure of the r_i from 1.0, the greater the inequality.

Inequality indexes are based on this notion of “average departure of the r_i from 1.0.” Briefly put, inequality indexes reflect the average disproportionality of the units’ shares of X . Unless a unit’s income ratio is exactly 1.0, it has a disproportionate share of income—either a disproportionately large share ($r_i > 1$) or a disproportionately small share ($r_i < 1$). So the deviation of r_i from 1.0 reflects disproportionality for a given unit. Because average *deviation* from 1.0 is always zero (positive and negative deviations offset), one must first transform the deviations to some function of *distance* from 1.0 (e.g., one might square the deviations) before taking the average. Inequality indexes measure “disproportionality” or “unequal share” as a function of distance of income ratios (r_i) from 1.0. As we will see, inequality indexes differ because they employ different functions of distance.

Regardless of the distance function used to measure it, disproportionality is zero when all the income ratios are 1.0 (equality). For that reason an inequality index—whatever its distance function—is zero in the case of equality. Hence inequality indexes can be described concisely as *functions of income ratios that are calibrated to zero for equality*.

It is important to note that inequality indexes are *population-weighted* functions of income ratios that are calibrated to zero for equality. In the case of individuals, the units are the same size so they are weighted equally. In the more general case, however, the units differ in size so they are not weighted equally. To underscore the switch to the general case of different-size units, I replace the i subscripts with j subscripts, consistent with the convention that j indexes “group.”

A general expression for inequality indexes (I) is

$$I = \sum_j p_j f(r_j), \quad (2)$$

where $r_j = X_j/\bar{X}$, the income ratio for the j th unit; $p_j = n_j/N$, the population share of the j th unit; f = the functional form used to measure disproportionality; and, $\sum_j f(r_j) = 0$ when $r_j = 1$ for all j (see app. A).

Equation (2) is a key finding. Observe that the population shares (the p_j) and the income ratios (the r_j) do not depend on the inequality index used. It follows from equation (2) that *inequality indexes differ only because they employ different functions of the income ratios*. Those functions are as follows for four popular indexes (V^2 , Theil, VarLog, and Gini, respectively):

$$\begin{aligned} v_j &= f(r_j) = (r_j - 1)^2, \\ t_j &= f(r_j) = r_j \log(r_j), \\ l_j &= f(r_j) = [\log(r_j) - E[\log(r_j)]]^2, \\ g_j &= f(r_j) = r_j(q_j - Q_j), \end{aligned} \quad (3)$$

where E is expected value, \log is the natural logarithm, q_j is proportion of total population in units poorer than unit j , and Q_j is proportion of total population in units richer than unit j (so $p_j + q_j + Q_j = 1$).

In short, the Gini index, the Theil index, the squared coefficient of variation (V^2), the variance of the logarithm, and other popular inequality indexes can be reduced to a simple common expression (eq. [2]). The expression of inequality indexes in this common form provides parallel calculations so results for the various indexes can be readily compared.

TREND IN INTERCOUNTRY INCOME INEQUALITY

The Korzeniewicz-Moran (1997) study provides a convenient point of departure for studying the trend in intercountry income inequality. As noted earlier, Korzeniewicz and Moran conclude that intercountry income inequality is rising. Because that conclusion fits nicely with a large body of sociological literature on world polarization, the study is likely to attract a good deal of attention among sociologists. Moreover, the finding seems plausible, given the growth spurt in world income in recent decades (Easterlin 1998): careful estimates (Summers et al. 1994) indicate that the world's per capita income, stated in constant U.S. dollars, almost doubled from 1960 to 1989 (from \$2,277 in 1960 to \$4,367 in 1989), and an increase of this magnitude certainly has enormous potential for destabilizing the distribution of income across nations. Have Korzeniewicz and

Moran uncovered an important trend that other weighted studies have missed?

The answer is no. The Korzeniewicz-Moran findings are based on the FX rate method, which is an unreliable method for comparing national incomes (e.g., Summers and Heston 1991; Horioka 1994). It is well documented that the use of official exchange rates exaggerates intercountry inequality (Ram 1979) and produces spurious divergence in intercountry inequality (Summers and Heston 1991, table 4; Schultz 1998). When industry-standard income data are substituted for the data used by Korzeniewicz and Moran, the rise in intercountry inequality disappears. What Korzeniewicz and Martin have demonstrated is not world polarization but the “dangers of using market exchange rates when making international comparisons” (Horioka 1994, p. 298).

To demonstrate these points, it is necessary first to summarize central issues regarding the comparison of income across nations.

Income Data

International comparisons of economic activity traditionally were obtained by using the FX rate to convert each country’s national account data to a common currency, usually the U.S. dollar. But FX rates are highly flawed calibrators of currencies for two reasons. First, many goods and services are not traded on the international market, so exchange rates are based on a restricted bundle of goods and services (Grosh and Nafziger 1986, p. 351). Because this failure to capture economic activity is especially acute for nonmonetized exchange in nonindustrial nations, FX measures of national income tend to miss significant economic activity in poorer nations. Second, FX markets are not totally “free” but are routinely distorted by government policy and speculative capital movement. As a result, exchange rates fail to reflect accurately the actual purchasing power parities (PPPs) of currencies.

To alleviate the deficiencies of FX-based income measures, several economists at the University of Pennsylvania spearheaded an ambitious effort to estimate national incomes using PPP to calibrate local currencies. Cross-nation parity for goods and services was determined through detailed studies of national price structures. As a result of those efforts, there is now an income series—the Penn series (Summers, Kravis, and Heston 1980; Kravis, Heston, and Summers 1982; Summers and Heston 1991; Summers et al. 1994)—that does not rely on FX rate. Even critics of the PPP measure concede that it represents a big improvement over the old FX measure (Dowrick and Quiggin 1997).

To appreciate the severity of the problem with using foreign exchange

rates to compare national incomes, consider the FX income estimates for China and Japan. The remarkable economic growth of China since 1978 (Nee 1991, fig. 1; Chow 1994; Mastel 1997) is reflected in the PPP income series, where China's income ratio jumps roughly 40% between 1975 and 1989. Incredibly, though, the FX-based World Bank income series used by Korzeniewicz and Moran fails to capture that growth; instead it indicates that China's growth rate lagged so far behind the rest of the world that the FX income ratio for China declined by a whopping one-third from 1970 to 1989 (from .139 to .090).

The FX estimates for Japan are just as misleading. Though Japan experienced brisk economic growth through the 1970s and 1980s (Tachi 1993; Argy and Stein 1997), per capita income in Japan at the end of the 1980s still fell well short of incomes in the richest nations in the West (Horioka 1994). Yet FX-based income estimates place Japan's 1989 per capita income *above* per capita incomes in many rich Western nations (12% higher than Sweden and 16% higher than the United States; see World Bank 1993).

How do FX income estimates become so distorted? The Japanese case is illustrative. The use of foreign exchange rates to compare incomes leads one to conclude that Japanese per capita income as a percentage of U.S. per capita income rose from 67% in 1985 to 121% in 1988 (Horioka 1994, table 1). Obviously an increase of this magnitude in just three years would have been nothing short of miraculous. In fact this stupendous increase is "nothing more than a statistical illusion" (Horioka 1994, p. 297) caused by the too-rapid appreciation of the yen from 238 yen to the dollar in 1985 to 128 yen to the dollar in 1988. As Horioka (1994, table 1) demonstrates, more realistic measurement indicates that the Japan/U.S. income ratio rose only marginally over those three years, from 0.74 in 1985 to 0.76 in 1988.

In addition to the evidence that official exchange rates yield implausible income estimates for specific nations such as Japan and China, there are critical theoretical reasons for using PPP-based estimates when comparing incomes across nations (Summers and Heston 1980, 1991; Grosh and Nafziger 1986). Though Korzeniewicz and Moran (1997, p. 1011) state that the FX rate method "provides a better relational indicator of *command over income*" (emphasis in original), to the extent that exchange rates bear on command over income, they do so *in the world marketplace*—a largely hypothetical concept in the workaday world of the vast majority of the world's population. For the vast majority of the world's population, foreign-exchange-rate income is largely moot, since most of what is produced is not traded internationally. People face local prices, not international prices. This is not to deny that foreign-exchange-rate price can

TABLE 3
RESULTS FOR PPP-BASED VERSUS FX-BASED INCOME ESTIMATES

YEAR	PPP		FX			
	Theil	Gini	Theil		Gini	
			Nominal	Adjusted	Nominal	Adjusted
1965552	.560	.816	.762	.661	.643
1970548	.558	.826	.771	.666	.647
1975540	.555	.847	.775	.674	.650
1980531	.550	.878	.782	.681	.650
1985512	.539	.963	.835	.706	.663
1989526	.543	1.079	.900	.733	.683
1965–89 change (%)	–4.7	–3.0	+32.2	+18.1	+10.9	+6.2

SOURCE.—Summers et al (1994) for the PPP income data and World Bank (1993) for the FX income data

NOTE.—There are 120 nations in the PPP data set and 112 nations in the FX data set. The data sets contain both capitalist and socialist nations and all populous nations and cover over 90% of the world's population. Theil and Gini results are reported to allow comparison with results offered by Korzeniewicz and Moran (1997). Results for V^2 and VarLog lead to the same conclusions. Under FX, "nominal" uses exchange-rate income estimates as given and "adjusted" uses more realistic estimates of income trends in China and Japan (see text).

affect local price, but it is to say that an ox does not become half-an-ox when a nation decides to devalue its currency by half relative to the U.S. dollar.⁶

Trends for FX versus PPP Income Estimates

Table 3 reports the 1965–89 trend in intercountry inequality based on both PPP and FX income. I try to replicate the Korzeniewicz and Moran (1997) study as closely as possible. First, I rely on the same source for FX

⁶ Foreign-exchange-rate income pertains to the level of goods and services that an individual in a given nation could purchase *in international trade*. Purchasing power parity measures, by contrast, are designed to measure level of experienced income. By "level of experienced income" I refer to the level of goods and services that an individual in a given nation can purchase, faced with the price structure of that nation. Consider farmers, who constitute a substantial fraction of the population in most poor nations. Suppose a farmer has an ox. Because the ox has value—e.g., it can be used to pull a plow—it can be sold locally. The price that it would command is its local price. The ox also has a foreign-exchange-rate price, i.e., the price it could command *if* it were sold internationally. The local price is the relevant price to the farmer. As Summers and Heston (1991, p. 360) put it, "After all, residents of a country face their own prices, not international prices." The worth of an ox to a farmer is not halved when a nation decides to devalue its currency by half relative to the U.S. dollar.

income estimates—the World Bank (1993)—and I use the same population data. Second, I use 1965 as the starting point.⁷ Third, I use the inequality indexes they used, the Theil and the Gini (results for V^2 and VarLog are similar). Finally, to ensure that results do not vary because of sampling differences, both “samples” here represent a near-universe of the world’s people.⁸

The results vividly demonstrate the difference in the two income series. According to the PPP-based income estimates, intercountry inequality declined modestly from 1965 to 1989. Yet according to the FX-based estimates, intercountry income inequality shot up 32.2% based on the Theil and 10.9% based on the Gini. Korzeniewicz and Moran (1997, table 3) report similar results (increases of 38.2% based on the Theil and 12.5% based on the Gini). These results reinforce the warning of, among others, Summers and Heston (1991, p. 355) that “it really makes a difference if exchange rates are used rather than PPPs” so “the practice of using exchange rates as quick, easily obtained estimates of PPPs is invalidated” (p. 335).

To see if the misleading FX income estimates for China and Japan matter much, I estimated a second, adjusted set of FX-based trends in intercountry income inequality (table 3, cols. 4, 6). These results are based on the same FX income data as before, except that I use better income

⁷ Again, I use 1989 as the endpoint because the dissolution of the Soviet Union interrupts the income series at that point. I did not attempt to extend the series past 1989 by using a weighted average of the republics’ income estimates to reaggregate the Soviet Union because reliable income estimates are difficult to obtain for some of the former Soviet republics.

⁸ There are 120 nations in the PPP data set and 112 nations in the FX data set. The data sets contain both capitalist and socialist nations and all populous nations. Both data sets cover over 90% of the world’s people. The 120 nations are Algeria, Angola, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde Island, Central African Republic, Chad, Comoros, Congo, Egypt, Gabon, Gambia, Ghana, Guinea, Guinea Bissau, Ivory Coast, Kenya, Lesotho, Madagascar, Malawi, Mali, Mauritania, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Reunion, Rwanda, Senegal, Seychelles, Somalia, South Africa, Swaziland, Togo, Tunisia, Uganda, Zaire, Zambia, Zimbabwe, Barbados, Canada, Costa Rica, Dominican Republic, El Salvador, Guatemala, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Panama, Puerto Rico, Trinidad and Tobago, United States, Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Guyana, Paraguay, Peru, Suriname, Uruguay, Venezuela, Bangladesh, China, Hong Kong, India, Indonesia, Iran, Israel, Japan, Jordan, South Korea (Republic of Korea), Mongolia, Myanmar, Pakistan, Philippines, Saudi Arabia, Singapore, Sri Lanka, Syria, Taiwan, Austria, Belgium, Cyprus, Czechoslovakia, Denmark, Finland, France, West Germany, Greece, Iceland, Ireland, Italy, Luxembourg, Malta, Netherlands, Norway, Portugal, Romania, Spain, Sweden, Switzerland, Turkey, United Kingdom, Soviet Union, Yugoslavia, Australia, Fiji, New Zealand, Papua New Guinea, and Tonga.

TABLE 4
TRENDS BASED ON FOUR INEQUALITY INDEXES

Year	Average World Income*	V^2	VarLog	Theil	Gini
1960	2,277	1.36	.91	.517	.540
1965	2,660	1.42	1.04	.552	.560
1970	3,118	1.36	1.08	.548	.558
1975	3,426	1.30	1.11	.540	.555
1980	3,835	1.29	1.07	.531	.550
1985	4,059	1.28	.96	.512	.539
1989	4,367	1.34	.96	.526	.543

SOURCE.—Summers et al (1994)

NOTE — $N = 120$ nations. V^2 is the coefficient of variation, squared

* Mean per capita income for the 120 nations in constant U.S. dollars

ratio estimates for China and Japan. Using more defensible income ratios (based on PPP) for *just those two nations* reduces the observed increase in the Theil and the Gini by over 40%.

In short, Korzeniewicz and Moran found divergence because they used a dubious income measure. Lest there be any doubt that the FX data yield a specious increase in intercountry inequality, it should be noted that a recent technical analysis of the PPP data used here (Dowrick and Quigen 1997) concludes that the PPP data are, if anything, biased *in favor of polarization*. If so, then my failure to replicate the Korzeniewicz-Moran polarization result using PPP income cannot be dismissed on the ground that the use of PPP income as the yardstick stacks the deck against the polarization thesis.

If the FX income estimates tell the wrong story about recent trends in intercountry inequality, what is the right story? I now use PPP income estimates to answer that question.

Trends in Four Inequality Indexes

Table 4 reports 1960–89 trends in intercountry inequality based on PPP income for 120 nations. For all four inequality indexes, intercountry income inequality was about the same in 1989 as it was in 1960: it fell slightly according to V^2 and rose slightly according to the other three indexes. If we start at 1965 instead of 1960, all four indexes declined somewhat. Nevertheless, the big story here is not change, but lack of it. The distribution of income across nations remained remarkably stable over a period of substantial income growth in the world.

In terms of the pattern of change in inequality, three of the four indexes— V^2 , Theil, and Gini—give parallel results: Inequality peaked in the mid-1960s, declined monotonically from 1965 to 1985, and increased somewhat from 1985 to 1989. The variance of the logs, by contrast, indicates that inequality peaked in 1975. These patterns appear to be robust, since Schultz (1998, table 1) reports similar patterns for VarLog, Theil, and Gini based on an earlier version of the Penn income series (he does not report results for V^2).

The variance of the logs gives different results because it is the most sensitive of the four indexes to change at the lower end of the income distribution. By logging income, VarLog compresses higher incomes more than lower incomes; so a \$100 increase has a greater effect among poor nations than it does among rich nations. The Theil index and VarLog both use logged values for income, but the Theil index mutes the effect of logging by weighting $\log(r_i)$ by r_i . Of the four indexes, then, VarLog is the most influenced by income change at the lower end of the income distribution. It is interesting that, when we weight the inequality index to place more emphasis on income trends for the poorer nations (as in VarLog and the Atkinson index given below), we find that intercountry income inequality has declined since 1975.

ACCOUNTING FOR THE STABILITY IN INTERCOUNTRY INCOME INEQUALITY

Why did intercountry inequality remain stable in the face of the near-doubling of the world average income? The common expression for inequality indexes (eq. [2]) is a good place to begin in addressing this question. By expressing inequality indexes in their common form as functions of income ratios and population shares, it is then straightforward to (i) decompose the change in each index in a standardized way to determine the contributions of changing income ratios (economic convergence or divergence) versus changing population shares for the nations; and (ii) quantify the contribution of *each nation* to the overall change in inequality.

Changing Income Ratios versus Changing Population Shares

By decomposing the trend in the inequality indexes we solve the mystery of why unweighted studies find economic divergence whereas weighted studies find no increase in intercountry inequality. Unweighted convergence studies capture the effect of changing income ratios but miss the effect of changing population shares. Weighted studies capture both. The decomposition reveals that the income ratios are in fact diverging, ex-

plaining why economists typically find divergence in their unweighted studies; yet change in population shares across countries offsets that divergence, so *weighted* intercountry income inequality does not increase.

Appendix B, table B1 reports the decomposition results. All four indexes confirm that differential economic growth boosted intercountry inequality whereas differential population growth reduced intercountry inequality. In other words the inequality-enhancing effect of diverging income ratios was blunted by the slower population growth of the richer nations.

So we have an answer to the stability question: intercountry income inequality was stable because the divergence in the income ratios (indicating faster per capita income growth, on average, in richer nations than in poorer ones) was offset by the slower population growth of richer nations. To conceptualize this phenomenon, consider national populations distributed along an X-axis of income ratios. The percentage of the world's population living in nations at the upper tail of the distribution (such as the United States) was smaller in 1989 than in 1960, and this narrowing of the upper tail offset the inequality-enhancing effect of the stretching of the X-axis as income ratios diverged (see fig. 1).

What If Population Growth Rate Had Not Varied across Nations?

Suppose population growth rate had been the same for all nations from 1960 to 1989. Under that circumstance, what would the trend in intercountry inequality have been? The answer appears to be simple: constant population growth rates across nations imply constant population shares, so $\Delta p_i = 0$ and the second and third terms in the decomposition ($\sum_j \Delta p_j f[r_{j1}] + \sum_j \Delta p_j \Delta f[r_j]$), the "changing population shares" and "joint

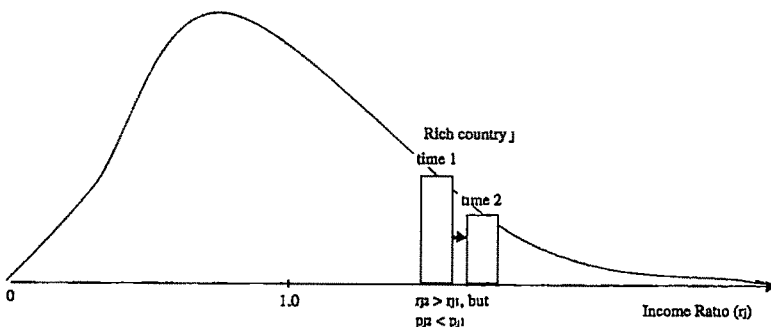


FIG. 1.—How divergence of income ratios ("stretching" of the X-axis) is offset by population shares for rich nations.

effect" components in table B1) go to zero. Because only the "changing income ratios" component remains, and that component is positive in table B1, the decomposition results suggest at first glance that intercountry income inequality would have increased if population growth rate had not varied across nations.

Upon reflection, though, it is not clear that the income ratios would have diverged if population had in fact grown at the same rate in rich and poor nations, since the divergence of the income ratios itself (app. B, table B1) could have been due to the faster population growth of the poorer nations. The key here is to remember that the income ratios are based on per capita income. Income per capita tends to grow more slowly in fast-growing populations because of the effect of rapid population growth on the ratio of dependents to workers. For this reason alone income ratios could diverge when (as here) poorer nations have faster rates of population growth.

In short, income ratios could diverge across nations simply because of the swelling of the young nonworking population in poor nations, quite apart from any endogenous growth or dependence effects. To distinguish population-induced polarization from other types of polarization, I removed the dependents from the denominator, that is, I examined the trend for income *per worker*. If the income ratios in table B1 are diverging only because of the faster growth of the dependent population in poorer nations, then the income ratios should no longer diverge when dependents are removed from the analysis—which is precisely what happens when dependents are removed from the denominator. If we decompose the income *per worker* trend in the Theil, for example, we find that the *income ratios* converged slightly ($-.028$) from 1960 to 1989; compare this result with the result for income *per capita*, where the income ratios *diverged* ($+.136$ for the Theil; see table B1). The other indexes give similar results. These findings support Sheehey's (1996) conclusion that the faster population growth of poorer nations accounts for the divergence in national income ratios found in the unweighted convergence studies in economics.

Hence *all the important dimensions of the 1960–89 trend in intercountry income inequality are related to differences in the population growth rates of richer and poorer nations*. First, the effect of changing population shares offsets the inequality-enhancing effect of changing income ratios. Second, the inequality-enhancing effect of changing income ratios is *itself* the apparent product of population processes: the income ratios did in fact diverge, yet this stretching of the X-axis represents spurious *economic* divergence in the sense that the stretching is rooted in population processes (faster growth of the nonworking population in poorer nations) rather than in economic processes. With regard to the recent trend in inter-

country income inequality, then, the headline story is the ascendant effect of national differences in population growth rates.

Offsetting Effects of Influential Nations

This study, up to this point, makes several contributions. First, it shows how research on intercountry income inequality (the dominant component of total world income inequality) can go astray either by confusing weighted and unweighted convergence or by using dubious income estimates based on foreign exchange rates. Second, the study describes a common formula for inequality indexes that demystifies the indexes and facilitates the study of inequality. Third, the study shows that the trend in *weighted* intercountry inequality was basically flat from 1960 to 1989 because divergence in the income ratios was offset by a decline in the population share of richer nations. Fourth, the study shows that the faster population growth of poorer nations also accounts for the divergence in national income ratios found (a) in studies of the *unweighted* inequality trend and (b) in the income ratios component of the weighted inequality trend. This section of the article adds to knowledge about the trend by identifying the nations that matter most for the weighted trend and by describing the effect those nations have on that trend.

Equation (2) tells us that a nation's contribution to inequality depends on the nation's income ratio and population share. It follows that a nation's contribution to *change* in intercountry inequality is determined by *change* in the nation's income ratio and population share. Intercountry inequality is reduced when a nation's income ratio moves toward 1.0 and increased when a nation's income ratio moves away from 1.0.

Based on changes in income ratios and population shares we expect Japan, India, the United States, and China to play major roles in the movement of intercountry inequality from 1960 to 1989. Japan's income ratio skyrocketed over this period, from 1.3 in 1960 to 3.1 in 1989. Because the ratio moved away from 1.0, Japan will boost intercountry inequality over the period. India will also boost inequality over this period because of faster-than-world-average population growth (India's share of the world population increased from roughly 14% to roughly 16%) and slower-than-world-average economic growth (which moved India's income ratio further below 1.0). India's faster-than-average population growth boosts inequality because India lies in the left-hand tail of the income distribution.

The income ratios for China and the United States, by contrast, moved *toward* 1.0—in China because of faster-than-world-average economic growth and in the United States because of slower-than-world-average growth. The income ratio for China changed from 0.25 to 0.31. Though

TABLE 5

LEADING CONTRIBUTORS TO CHANGE IN INTERCOUNTRY INCOME INEQUALITY, 1960-89

Nation	Δr_i	Δp_i	V^2	VarLog	Theil*	Gini*
Increased inequality the most:						
Japan	+1.84	-.008	.125	.053	.068	.043
Soviet Union	+.72	-.016	.058	.050	.043	.036
India	-.06	+ .019	. . †	.040	.026	.057
Italy	+.80	-.006	.029
Reduced inequality the most:						
United States	-.21	-.012	-.189	-.054	-.071	-.040
China	+.06	-.003	-.018	-.087	-.055	-.082
Indonesia	+.14	+.003	-.015	-.014
United Kingdom	+.03	-.007	-.016	-.016

SOURCE—Summers et al. (1994)

NOTE— $N = 120$ nations* Centered using $r_i - 1$ (see app. C)

† Not in top three for this inequality index

this increase might appear to be relatively modest, it is likely to have a major impact on global inequality because of the sheer size of China's population. In the United States, economic growth and population growth over the period both lagged behind world averages. The result is a declining income ratio and a declining population share for the United States, thus reducing intercountry inequality.

Results.—The insight that inequality indexes can be expressed as functions of income ratios and population shares (eq. [2]) provides the necessary analytic leverage for quantifying the contributions of each nation to change in intercountry inequality (see app. C for technical details). In table 5 I report the results for change in intercountry inequality, 1960-89, for each of the four indexes. To save space I report the most influential countries only (the nations that most increased, and those that most reduced, 1960-89 intercountry inequality).

The four indexes tell the same story though they differ somewhat on the details. Based on the Gini index, India boosted inequality the most, followed by Japan and the Soviet Union, and China reduced inequality the most, followed by the United States and Indonesia. The Theil index identifies the same six nations but ranks them somewhat differently. Based on VarLog, Japan and the Soviet Union boosted inequality the most, and China and the United States reduced inequality the most. Two nations—Japan and the United States—are identified by V^2 as dominating the trend, with Japan boosting intercountry inequality and the United States reducing it.

So we now have another answer to the question of why intercountry

inequality remained so stable during a period of substantial income growth: the effects of the big nations offset. The inequality-enhancing effect of brisk economic growth in Japan and sluggish growth in India was blunted by the inequality-reducing effect of rapid economic growth in China and slower-than-world-average growth in the United States over this period.

OVERWEIGHTING POORER NATIONS

As this article makes plain, inequality indexes are averages (average disproportionalities). Because an inequality index is an average, the trend in an inequality index might miss offsetting changes in an income distribution. That point is pertinent here because some studies of intercountry inequality (e.g., Peacock et al. 1988) note an offsetting pattern of convergence and divergence. If there is convergence at some regions of the income distribution and divergence at other regions, then one's conclusion about overall inequality might be sensitive to the weight given to the different regions of the income distribution.

In this section I show what happens to the 1960–89 trend when poorer nations are given more weight in calculating inequality. The decision to give more weight to poorer nations follows from the welfare principle that income increases at lower ends of the income distribution produce greater welfare benefits than do income increases at the upper end of the income distribution. Atkinson (1970) demonstrates that the following index is consistent with the welfare principle:

$$A = 1 - (\sum_j p_j r_j^{1-\epsilon})^{1/(1-\epsilon)}, \quad \epsilon > 0, \quad (4)$$

where (as before) p_j is the population share and r_j is the income ratio.

The Atkinson index (A) does not refer to a single index of inequality but rather to a family of measures that depend on the parameter ϵ . The parameter ϵ determines the relative sensitivity of A to transfers at different points in the income distribution: the larger the ϵ , the greater the weight given the lower end of the income distribution.

To appreciate the effect of the reweighting, consider two nations of equal population, one rich and one poor, with income per capita differing by a factor of 10 for the two nations. Suppose we want to increase the per capita income of the poor nation P by \$1 at the expense of the rich nation R . By varying ϵ we specify how much we are willing to reduce income per capita in R in order to add \$1 to the per capita income of P : From equation (4) it follows that we are willing to sacrifice up to 10^ϵ income per capita in R in order to add \$1 to the per capita income of P (Cowell 1977, p. 45). For example, $\epsilon = 0$ implies a willingness to take \$1 from R to enrich P by \$1; in other words, we are "inequality averse," so

TABLE 6
TRENDS BASED ON ATKINSON MEASURES

YEAR	RELATIVE WEIGHTING OF LOWER END OF DISTRIBUTION				
	$\epsilon = 0.1$	$\epsilon = 0.6$	$\epsilon = 1.1$	$\epsilon = 1.5$	$\epsilon = 2.5$
1960051	.274	.430	.512	.618
1965055	.296	.466	.552	.658
1970054	.299	.472	.560	.666
1975054	.299	.476	.566	.673
1980053	.292	.465	.554	.662
1985051	.277	.439	.524	.635
1989052	.282	.444	.528	.639
1960-89 change (%)	+2.0	+2.9	+3.3	+3.1	+3.4

SOURCE.—Summers et al (1994)

NOTE.— $N = 120$ nations. The larger the ϵ , the greater the weight given the lower end of the income distribution. Larger values of ϵ reflect greater aversion to inequality (see text)

we believe that total welfare is increased when income is transferred from a richer to a poorer unit. Values of $\epsilon > 0$ imply a willingness to accept a net income loss in order to reduce inequality: $\epsilon = 0.5$ implies a willingness to reduce R 's income by \$3.16 to increase P 's by \$1 (a net income loss of \$2.16); $\epsilon = 1.0$ implies a willingness to accept a loss of \$9 (\$10 from R , \$1 to P); $\epsilon = 2.0$ implies the willingness to accept a loss of \$99 to transfer \$1 to P ; and so on.

I report results for ϵ for 0.1, 0.6, 1.1, 1.5, and 2.5 (table 6). Schwartz and Winship (1979, p. 31) cite studies suggesting that ϵ should be between 0.5 and 0.75, but I report a broader range to demonstrate the robustness of the results for intercountry income inequality. It should be noted that $\epsilon = 2.5$ is an extreme reweighting in favor of the lower end of the distribution, since $\epsilon = 2.5$ implies the willingness to accept a net income loss of \$315 to enrich the poorer nation by \$1 in the example above where the two nations differ by a factor of 10.

Reweightings in favor of poorer nations does not affect our conclusions. All five values of ϵ repeat the story of little change in intercountry income inequality from 1960 to 1989. Inequality in 1989 is slightly higher than it was in 1960 but lower than it was in 1965. For the five ϵ -values, intercountry inequality peaked in 1975 (except $\epsilon = .1$, where 1965 is the peak), declined from 1975 to 1985, then increased somewhat from 1985 to 1989. This pattern of increase from 1960 to 1975 and decline thereafter (except 1985-89) most closely resembles the pattern exhibited by VarLog. This similarity is not surprising because, of the four popular indexes, VarLog gives the most weight to poorer nations.

DISCUSSION AND IMPLICATIONS

This study employs a powerful tool—a common general formula for inequality indexes—to get the basic facts right about intercountry inequality over the 1960s, 1970s, and 1980s. This effort to get the facts right yields two big stories. One big story—not found in the usual world system or endogenous growth theory accounts of intercountry inequality—is the overriding importance of differential rates of population growth on intercountry inequality. A second big story is that the centuries-old trend of increasing intercountry income inequality has stalled at least since 1960.

The Centrality of Population Change

Population grew more rapidly in poor nations than in rich nations from 1960 to 1989. Intuition suggests that this difference is likely to affect change in the distribution of per capita income across nations because, after all, change in *per capita* income is a function of changing income *and* changing population.

The findings confirm intuition in this instance. Differences in nations' rates of population growth played a central role both in producing divergence in income ratios across nations and in muting the effect of that divergence. First, with regard to the muting effect of population growth, the slower population growth rate of the rich nations reduced weighted intercountry inequality by narrowing the right-hand tail of the income distribution. This narrowing of the right-hand tail offset the inequality-enhancing effect of divergence in the income ratios. The net result was the plateau in weighted intercountry income inequality over the 1960s, 1970s, and 1980s.

Second, the divergence of the income ratios was *itself* the result of population processes. Classical economists such as Malthus ([1798] 1960), John Stuart Mill ([1848] 1923), Ricardo ([1817] 1962), and Smith (1776) viewed economic growth as a "race between increases in the population and capital stock" (Dorfman 1991, p. 577). If economic growth is a race between increases in population and capital stock, then at this point in history poor nations—with their more rapid rates of population growth—are inherently disadvantaged. It is well documented, for example, that the rapid population growth in poor nations has resulted in a high ratio of dependents, and by definition dependents add directly to the denominator but not the numerator of income per capita.⁹

⁹ Malthus's famous prediction is that population growth will outrun economic growth, so the rapid population growth of poor nations should produce *negative* growth rates for per capita income. The argument here is less extreme—I assume only that nations with fast-growing population tend to experience *slower* growth rates for per capita income (slower growth rates can be positive, of course). If population growth is faster

The *per worker* results support this line of reasoning. The income ratios diverge when income per capita is used but not when income per worker is used. This finding is consistent with Sheehy's (1996) claim that the effect of population growth through age structure accounts for the income divergence economists usually find in their unweighted studies (see Crenshaw, Ameen, and Christenson [1997] for further supporting evidence). In other words, per capita income ratios *are* diverging, but the divergence is *population induced*. Upon close inspection, then, neither the unweighted studies of economists nor the weighted studies of sociologists support theories of continuing economic divergence in today's world. The weighted studies do not because they find no net divergence. The unweighted studies do not because the divergence they find apparently arises from the age-structure effect of the more rapid population growth of poorer nations rather than from either dependence effects or endogenous growth effects.

The Intercountry Income Inequality Plateau of 1960–89

It is well documented that, since about the mid-1970s, income inequality *within* the United States has risen after a long period of decline (Fischer et al. 1996; Nielsen and Alderson 1997).¹⁰ This phenomenon has been dubbed "the great U-turn" (Harrison and Bluestone 1988). Less appreciated is the pause in the long-run trend of rising intercountry inequality. This pause spans at least the 1960s, 1970s, and 1980s.

The discovery of a "great plateau" in the historical trend has important implications for our understanding of trends in world income inequality. One implication is that if income inequality across individuals has been increasing sharply for the world as a whole, as Korzeniewicz and Moran (1997) conclude, then the increase must be due to increases *within* nations. To cause the sort of increase in total world inequality that Korzeniewicz and Moran describe, the within-nation increase would need to be of colossal proportions because most of the total world income inequality is between, not within, nations.

A second implication of the plateau is that intercountry income inequal-

in poorer nations (as it is) *and* if rapid population growth slows per capita income growth (as it apparently does), then differential population growth contributes to the divergence of national income ratios

¹⁰ "Intercountry income inequality" in this section refers to weighted data. The great plateau observation nonetheless also applies to the unweighted data: again, the divergence in income ratios observed in the unweighted data is "spurious" (in the sense that the divergence appears not to be based on economic processes) so, when income *per worker* is used to remove the age structure effect, the unweighted data also plateau.

ity does not inevitably rise (or fall) with rising world income. Inter-country inequality was about the same in 1989 as it was 30 years earlier, and an important challenge for future studies is to determine why intercountry inequality remained so stable in a period when the world's average income shot up so rapidly. During a period of such great potential for destabilizing the distribution of income across nations, why did the variance neither increase nor decline? This study provides one part of the answer: offsetting trends in the most populous nations. The inequality-enhancing effects of rapid economic growth in Japan and sluggish economic growth in India were blunted by the inequality-reducing effects of rapid economic growth in China and slower-than-world-average population and economic growth in the United States over this period.

Finally, the discovery of the great plateau in weighted intercountry income inequality adds to the clamor for new sociological theories of national development (Gereffi 1989; Firebaugh 1992; Firebaugh and Beck 1994). Stable variance in the distribution of logged income across nations in a period of active core-periphery exchange calls into question fundamental assumptions sociologists have made about the impact of international exchange on national development. If the benefits of core-periphery movement of goods and capital in fact accrue primarily to rich nations and if this differential benefit is in fact the principal cause of intercountry income divergence (as dependency theory appears to claim), then it is hard to explain why the long-standing trend toward intercountry divergence was interrupted during an era of active core-periphery exchange.

APPENDIX A

A Common Formula for Inequality Indexes

Probably the most popular measures of inequality are the Gini index (G), Theil's index (T), the coefficient of variation (V), and (in economics especially) the variance of the logarithms (VarLog). In this section I express these indexes as functions of r_i . These expressions verify that the indexes are all functions of the average distance of the r_i from 1.0 and that they differ only because they employ different functions of "average distance."

I use V , which is the standard deviation divided by the mean, to illustrate how these expressions are derived:

$$\begin{aligned}
 V &= \sigma/\mu, \\
 \rightarrow V^2 &= \sigma^2/\mu^2 \\
 &= (1/N)\sum_i (X_i - \mu)^2/\mu^2 \quad (i = 1, 2, \dots, N) \\
 &= (1/N)\sum_i (r_i - 1)^2,
 \end{aligned} \tag{A1}$$

where \sum denotes summation and μ is the mean of X . From equation (A1) it is obvious that V^2 is the average of the squared deviation of the income ratios (the r_i) from 1.0.

By similar algebraic manipulation it is possible also to express the other inequality indexes as functions of the r_i . The functions are

$$T = (1/N)\sum_i (X_i/\mu)\log(X_i/\mu) = (1/N)\sum_i r_i \log(r_i), \quad (\text{A2})$$

where \log is the natural logarithm, and

$$G = (2/N^2)\sum_i r_i [i - (N + 1)/2], \quad (\text{A3})$$

where the units are ranked in ascending order on X so i is the unit's *rank* on X (see Firebaugh [1998] for the deviation of [A3]). The bracketed part, $i - (N + 1)/2$, is the difference between the unit's rank and the average rank, $(N + 1)/2$.

$$\text{VarLog} = (1/N)\sum_i \{\log(r_i) - E[\log(r_i)]\}^2, \quad (\text{A4})$$

where \log is the natural logarithm and $E[\log(r_i)]$ is the expected value (mean) of the logged income ratio. Thus the variance of the logarithms is also a function of r_i .

Expressing the indexes as functions of r_i reveals that they have the common form $I = [\sum_i f(r_i)]/N$, that is, inequality indexes are means of $f(r_i)$. This insight will prove useful in decomposing the trend in intercountry income inequality.

Grouping cases with the same values on X yields this convenient common expression for inequality indexes, where $p_j = n_j/N$, the population share of the j th group:

$$I = \sum_j p_j f(r_j), \quad (\text{A5})$$

subject to the constraint $\sum_j f(r_j) = 0$ when $r_j = 1$ for all j .

Because inequality indexes can be expressed as $\sum_j p_j f(r_j)$, and because p_j and r_j do not depend on the index used, it follows that inequality indexes differ only because they employ different functions of r_j , as follows:

$$\begin{aligned} v_j &= f(r_j) = (r_j - 1)^2, \\ t_j &= f(r_j) = r_j \log(r_j) \\ l_j &= f(r_j) = \{\log(r_j) - E[\log(r_j)]\}^2, \\ g_j &= f(r_j) = r_j(q_j - Q_j). \end{aligned}$$

The expressions for v_j (squared coefficient of variation), t_j (Theil), and l_j (VarLog) follow directly from equations (A1), (A2), and (A4), respectively; g_j follows from the Gini formula for grouped data given by Blau (1977), where q_j is proportion of total population in units poorer than unit j and Q_j is proportion of total population in units richer than unit j (so $p_j + q_j + Q_j = 1$).

APPENDIX B

Decomposition Results

To decompose change in inequality as shown in table B1, I adapt a standard three-component formula for decomposing weighted means (Kitagawa 1955; Firebaugh 1997, pp. 27–28). The adaptation yields

$$\Delta I = \sum_j p_{j1} \Delta f(r_j) + \sum_j \Delta p_j f(r_{j1}) + \sum_j \Delta p_j \Delta f(r_j), \quad (\text{B1})$$

where ΔI denotes change in an inequality index, r_j is the income ratio for the j th unit, p_j is j 's population share, Δp_j is $p_{j2} - p_{j1}$ (the change in j 's population share from time 1 to time 2), and $\Delta f(r_j)$ is $f(r_{j2}) - f(r_{j1})$, the change in j 's income ratio as a function of v_j , t_j , l_j , or g_j (see eq. [3] above and see Firebaugh [1998]).

In plain English, change in an index of income inequality is the sum of three factors.

1. *Differential economic growth*.— $\sum_j p_{j1} \Delta f(r_j)$, the contribution of changing income ratios while holding population shares constant;
2. *Differential population growth*.— $\sum_j \Delta p_j f(r_{j1})$, the contribution of changing population shares while holding income ratios constant; and
3. *Joint effect*.— $\sum_j \Delta p_j \Delta f(r_j)$, the joint contribution of changing income ratios and changing population shares.

The effect of population growth on change in inequality depends on where the population growth occurs along the income axis. When nations at the tails of the income distribution grow faster than those in the middle of the distribution, differential population growth boosts inequality; when

TABLE B1

DECOMPOSITION OF 1960–89 CHANGE IN INTERCOUNTRY INCOME INEQUALITY

COMPONENT	INDEX			
	V^2	VarLog	Theil*	Gini*
Changing income ratios252	.189	.136	.0946
Changing population shares ..	-.205	-.103	-.074	-.0732
Joint effect	-.071	-.034	-.030	-.0190
1960–89 change	-.025	.052	.031	.0024

SOURCE — Summers et al (1994)

NOTE — $N = 120$ nations

* These indexes were centered using $r_i - 1$ (see app. C). Centered Theil increases by .031 (from 1.0345 in 1960 to 1.0654 in 1989).

populations grow faster in middle-income nations, population growth reduces inequality. The decomposition method used here reflects that fact. When population is growing faster for nations at the tails of the income distribution, the positive Δp_j 's are concentrated in the tails (where $f(r_{j1})$ is larger) and the negative Δp_j 's are concentrated in the middle (where $f[r_{j1}]$ is smaller), so $\sum_j \Delta p_j f(r_{j1}) > 0$ (recall that $f[r_{j1}]$ is always positive). When population is growing faster for middle-income nations, the negative Δp_j 's are concentrated in the tails, so $\sum_j \Delta p_j f(r_{j1}) < 0$.

However, the method is ill-suited for determining the effect of changing population share for a *specific nation*. Because $f(r_{j1})$ is positive, $\Delta p_j f(r_{j1})$ is positive when Δp_j is positive and negative when Δp_j is negative. For a specific middle-income nation, this result is backward: inequality is *reduced* by faster than average population growth ($\Delta p_j > 0$) in a middle-income nation and *enhanced* by slower than average population growth ($\Delta p_j < 0$). (I am indebted to David Lam for prompting me to clarify these points.)

APPENDIX C

Ranking Nations' Contributions to Inequality and to Change in Inequality

Because inequality is measured as the sum of $p_j f(r_j)$, an individual unit contributes $p_j f(r_j)$ to an inequality measure. Hence the contribution of units to inequality can be ranked based on $p_j f(r_j)$ —or so it would appear.

This ranking method works in a straightforward manner for V^2 and for VarLog but not for the Theil and Gini indexes. (Do not confuse the objective here—determining a unit's contributions to inequality and change in inequality—with the discussion in the final paragraph of app. B, where the issue is how to *decompose* a unit's contribution to change in inequality. Decomposing a unit's contribution to change in inequality is not straightforward for any of the measures, as app. B explains.) Consider intercountry income inequality. The ranking method works for V^2 and VarLog because $p_j v_j$ and $p_j l_j$ are positive values that are larger for nations at either end of the income distribution than for nations in the middle, a fact in line with the concept of inequality. In the case of Theil and Gini, by contrast, the use of $p_j t_j$ and $p_j g_j$ does not work in a straightforward manner to rank nations because it violates the basic principle that (size constant) nations at either extreme contribute more to inequality than do nations in the middle. That principle is violated for nations on the left side of the income distribution: $p_j t_j$ and $p_j g_j$ are *negative* for poor nations, and the poorer the nation, the more negative the value.

My solution is to "center" t_j and g_j by substituting $r_j - 1$ for r_j as follows:

$$\begin{aligned} t_j^* &= f(r_j) = (r_j - 1) \log(r_j), \\ g_j^* &= f(r_j) = (r_j - 1)(q_j - Q_j). \end{aligned}$$

It can be shown that $\sum_j p_j g_j^* = \sum_j p_j g_j$, that is, the centering procedure does not change the value for Gini. Thus the Gini itself can be calculated with or without centering. In the case of Theil, $\sum_j p_j t_j^* = \sum_j p_j t_j - \sum_j p_j \log(r_j)$, that is, centering adds a constant to the value for Theil. Thus the centered Theil should be used only to *rank* the contribution of individual units to inequality and to change in inequality; it should not be used to estimate the overall level of inequality.

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Their Brothers' Keepers? Orthodox Religionists, Modernists, and Economic Justice in Europe¹

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Through analyses of national surveys of 21 European countries and Israel, the authors test the conventional wisdom in Europe that modernists are to the left of the religiously orthodox on economic justice concerns. Modernists are more individualistic than the orthodox in seeing individuals, not a deity, as responsible for their fates and as the ultimate moral arbiters. The authors hypothesize that modernists are also economically individualistic in believing that the poor or jobless, not the community or state, should solve their own problems. The authors find that on economic concerns, modernists are far more likely to be to the right of the orthodox than to the left.

INTRODUCTION

The conventional wisdom in Europe is that religious traditionalists are to the political right of religious modernists (Almond, Sivan, and Appleby 1991*b*, p. 476; Arnold 1990, p. 186; Bréchon 1996; Coleman 1992, p. 85; Conradt 1986, p. 134; Harding, Phillips, and Fogarty 1986, p. 64; Kirschenbaum 1993; Mayer 1995; Mossuz-Lavau 1992; Percheron 1982; Safran 1991, p. 81; Soper 1994; Szawiel 1993; Talin 1995; Taylor 1985). So straightforward is the logic linking religious beliefs and political

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stances that it is rarely stated: conservative religious beliefs go with conservative political beliefs and progressive religious beliefs with progressive politics. The activism of secular communists and socialists on behalf of the working class and of traditionalist Christians in opposing legalization of abortion and divorce gives credence to the established view. The different positions staked out by socialist, social democratic, and communist parties, on the one hand, and by Christian-democratic parties, on the other, are taken as further testament to the accuracy of the common wisdom about modernists and religious traditionalists.

In this article, we propose an alternative theory linking religious cosmologies to a specific set of attitudes—those concerning economic justice. Our argument synthesizes the literature on religious cosmologies with that on attitudes toward economic equality—two subdisciplines of sociology that have had little impact on each other. We argue that what theologically distinguishes modernists (including both believers and secularists) from the religiously orthodox is their greater individualism. Modernists are individualistic in that they see individuals themselves, not a deity, as responsible for their destinies and as the ultimate judges of what constitutes moral action. Individualism in the economic realm manifests itself in the belief that individuals, not the state or larger communities in which they live, determine their own economic outcomes. For economic individualists, the solution to poverty or inequality is not more government spending on welfare programs or more private charity but greater individual effort by those on the bottom to improve their lot. We argue that modernists, as theological individualists, are more likely than the religiously orthodox to be individualistic with respect to concerns of economic justice, taking relatively egalitarian positions on such issues.

We test our hypothesis on data for 21 European countries and Israel, a country with strong European roots. Through analyses of national surveys of countries with a wide range of religious regimes, we find support for our hypothesis that modernists are to the right of the religiously orthodox on issues of economic justice, not to the left as is commonly assumed.

THEORETICAL BACKGROUND

Religious traditionalists are associated with right-wing politics in Europe, according to many observers. Arnold (1990, p. 186) notes the propensity of fundamentalist Catholics to support "right-wing, reactionary, and even repressive political regimes." In a similar vein, Coleman (1992, p. 86) observes that "Catholic integralist thinkers lent ideological support to the protofascist regimes of . . . Franco in Spain, Marshall Pétain in France and Salazar in Portugal." Talin (1995, p. 67) sees Catholic fundamentalism in France as linked with conservative political stances, "some of them

close to right-wing extremism." Conradt (1986, p. 134) observes that both Protestants and Catholics in West Germany with a strong attachment to their churches support the center-right religious party, while secularists support the parties of the left. Fundamentalist Protestantism in Northern Ireland, according to Taylor (1985, p. 241), is closely associated with ultra-conservative politics. And, writing about Israel, Kirschenbaum (1993, p. 190) comments on the right-wing nationalism of Gush Emunim, an ultra-Orthodox Jewish movement.

Empirical studies of the effect of religious belief on self-placement on a left-right scale (Bréchon 1996; Harding et al. 1986; Szawiel 1993) and on voting (Mayer 1995; Mossuz-Lavau 1992; Percheron 1982; Szawiel 1993) have also suggested that religious traditionalists are to the political right of theological modernists. Yet none of these studies allows the attitudes of the orthodox or modernists on issues of economic justice to be distinguished from their cultural attitudes.

While the cultural attitudes of the religiously orthodox and modernists are beyond the scope of the present article, careful studies have found secularists to be more libertarian than believers on cultural issues of abortion, sexuality, religion in state-supported schools, and divorce in a number of European countries (Hayes and Hornsby-Smith 1994; Hayes 1995; Blaschke 1995; Cesareo et al. 1995). In our own analyses, we find that in most European countries modernists are more libertarian than the religiously orthodox in their attitudes toward abortion, birth control, sex outside marriage, and prayer in public schools (details available on request; for a case study of Italy, see Davis and Robinson 1999). Nonetheless, it is incorrect, as our results will show, to infer from the relative cultural progressiveness of modernists that they are also relatively progressive on economic issues. Moreover, as important as issues of abortion, sexuality, and religious education are, debates on the problems of inequality, poverty, unemployment, and the solutions to these, such as taxation regimes, social welfare programs, aid to the unemployed, and government jobs programs, are at the core of contemporary European politics. We argue that theological modernists, relative to the religiously orthodox, are egalitarian on such economic issues. Although we limit our analyses to economic attitudes, our theoretical model accounts as well for the widely documented libertarianism of modernists on cultural issues.

In discussing the effect of religious cosmology on attitudes toward economic equality, we begin with a distinction introduced by James Davison Hunter (1991) between the religiously orthodox and progressives. The *orthodox* are those who believe that God is the ultimate moral arbiter of right and wrong, good and evil, that the word of God as recorded in sacred texts is inerrant and timeless, and that God takes an active role in people's everyday lives. In the God-centered and God-directed moral universe of

the orthodox, there are absolute moral standards that hold true for all times and all people. As Hunter (1991) points out, for many of the orthodox, sacred texts, such as the Torah, Bible, or Koran, are taken as the revealed word of God and as true, word for word. Roman Catholic traditionalists may find absolute moral authority in Church teachings or papal encyclicals, in addition to or instead of sacred texts (Alexander 1985; Arnold 1990; Coleman 1992; Pace 1995). *Progressives* believe that humans are the ultimate arbiters of right and wrong, that morality is an evolving, ongoing quest that must be judged in its historical and cultural context, and that individuals make their own fates (Hunter 1991, pp. 44–45). In the human-centered and human-directed universe of progressives or *modernists*, as we prefer to call them, morality is contextual and requires a hermeneutic approach to sacred texts or church pronouncements, if these are referred to at all.² What is right and wrong is determined by each individual for himself or herself, taking into account the specific circumstances under which the judgment must be made. Modernists include religious liberals as well as secularists. Modernism, as we use the term, refers to a cosmology that exists in all faith traditions, not narrowly to the modernist split in Roman Catholicism at the end of the 19th century.

It has become commonplace for scholars on both sides of the Atlantic to comment on the rapid secularization of Europe, as evidenced by dramatic declines in religious service attendance, church membership, and belief in God (Abbruzzese 1995; Acquaviva [1961] 1979; Bruce 1995; Davie 1990; Dekker 1995; Dobbelaere 1986; Dogan 1995; Giorgi 1992; Martin 1978; Sundback 1995; Wilson 1982; but see Morra 1988; Hervieu-Léger 1990; Stark and Iannaccone 1994; Lambert 1995). Yet communities and attendant movements of the religiously orthodox—often begun in an effort to resist secularization and modernization—are found in all European countries and Israel. A few examples will suggest the range of orthodox expression. *Comunione e Liberazione* in Italy is an antimodernist movement seeking to restore the authority of the pope in the Italian state (Abbruzzese 1989; Cavallaro 1976; Cipriani 1989; Kepel 1994; Zadra 1991). In France, Roman Catholic participants in the Charismatic Renewal movement, like many “born again” Protestants in the United States, value a close personal relationship with God and interpret the Bible literally (Arnold 1990, p. 176; Talin 1995, p. 70). In the same country, followers of Archbishop Marcel Lefebvre’s integrist movement (*Fraternité St. Pie X*) initiated the first ma-

² We prefer the label “modernist” for this cosmology because it avoids the political connotation that may be associated with “progressive.” We prefer Hunter’s (1991) label, “orthodoxy,” to “religious conservative,” which may also be interpreted politically, and to “fundamentalist,” which some would limit to the split that developed in 19th-century British and American Protestantism.

major schism in the Roman Catholic Church in the 20th century in seeking a return to pre-Vatican II practices (Arnold 1990, p. 177; Coleman 1992). In the Netherlands, Orthodox Reformed Christians and Evangelical Christians are strictly orthodox in their adherence to biblical literalism and inerrancy (Dekker and Stoffels 1993). Protestant Restorationists in Great Britain, fundamentalist in their views of the Bible, strive to bring about the "kingdom of God" on earth (Walker 1987, p. 201). Reverend Ian Paisley's Ulster Protestants in Northern Ireland combine biblical fundamentalism with ethnic nationalism and anti-Catholicism (Almond, Sivan, and Appleby 1991a, p. 420), and Gush Emunim, an ultra-Orthodox Jewish movement in Israel, uses the Bible to justify its territorial claim for a greater Israel (Almond et al. 1991a, p. 419; Wieseltier 1990, p. 194).

As we noted above, many scholars have commented on the link between such organized movements, or the religiously orthodox more generally, and the political right. Our alternative argument focuses on differences between the moral cosmologies of religious traditionalists and modernists and how these manifest themselves in beliefs about economic justice.³ Before presenting our argument, we note that the orthodox belief in the timeless authority of Scripture or the teachings of the church does not inevitably lead to either egalitarian or inegalitarian economic views. Sacred texts and church teachings have been subject to multiple interpretations, and such interpretations have served as "master frames" (Snow and Benford 1992) of social movements to promote equality as well as of movements and efforts to maintain class, racial, and gender hierarchies (see, e.g., Billings 1990). The Old and New Testaments of the Bible, which are perhaps most widely accepted by the orthodox as a source of moral authority, "could mean different things to different people at different times, in different circumstances" (Hill 1993, p. 5).

In his detailed analysis of the history of the social (i.e., economic) doctrine of the Roman Catholic Church, as recorded in such documents as Leo XIII's encyclical *Rerum Novarum* of 1891, Pius XI's *Quadragesimo Anno* of 1931, and the Vatican II Council of the early 1960s, Burns (1990, 1992) finds that to maintain autonomy from the state, the Church "subordinated socio-political issues to more purely religious and moral issues," such as those of abortion, birth control, and sexuality (1990, p. 1123).⁴ Burns notes, further, that the Church maintained a degree of "ambiguity and flexibility of doctrine on socio-political issues, which avoids specific policy commitments" (1992, p. 193) and made no attempt to bind Roman

³ See Davis and Robinson (1996a, 1996b, 1996c, 1997, 1999) for discussions of the effect of moral cosmology on issues of sexuality, reproductive rights, schooling, and gender roles.

⁴ See O'Brien and Shannon (1992) for the texts of the Vatican documents.

Catholic clergy or lay people to the social doctrine with the threat of excommunication (1992, pp. 45, 202; see also O'Brien 1968, pp. 13–18, 22).⁵ Burns concludes: "By being vague in their social pronouncements, popes could maintain ideological independence while avoiding conflict with powerful secular forces. They could make (admittedly weak) attempts to reach out to the working class without being specific enough to alienate conservative Catholics of the middle and upper classes. For the sake of the Church's reputation and the desire to reach out to poorer and reformist Catholics, popes could claim that social justice was a high priority when in fact it was not" (1992, p. 194).

Ambiguity has been the rule in the Church's social doctrine, allowing orthodox Roman Catholics, who may look to Church teachings as a source of moral authority as much as or instead of the Bible, to find support for inegalitarian economic views in the Church's insistence upon the absolute right to private property and in its rejection of the idea of class conflict or to find support for economic egalitarianism in the Church's advocacy of the right of workers to form labor unions and in its emphasis on the obligation of the propertied classes to provide a living wage to workers (Giammanco 1989, pp. 88–89).

If the orthodox are not pushed in a specific economic direction by their reliance on sacred texts and church teachings as a source of moral authority, what can explain the position of the orthodox and modernists on concerns of economic equality? The answer, we believe, lies in the inherent individualism of theological modernism. The modernist moral cosmology, with its human-centered universe and individually determined fates and moral standards, is individualistic at its core. While our arguments are about the modernist and orthodox cosmologies as these manifest themselves in individuals in any society, much of what has been written about modernism as a historical stage applies as well to the beliefs of individual modernists. For Bell (1996, p. 16), "The fundamental assumption of modernity . . . is that the social unit of society is not the group, the guild, the tribe, or the city, but the person." Individualism—"the heart of modernity" according to Abbruzzese (1995, p. 220)—entails a rational, calculating, self-interested individual, who makes his or her own choices among the various alternatives in the marketplace, including the marketplace of religious ideas. As Bruce (1995, p. 427) writes, "Modernization has seen the

⁵ In Italy, e.g., Abbruzzese (1995, pp. 213–14) observes that "the church is at a dead-end: if it takes a stand in solving social problems, it will lose political support. On the other hand, if it endorses modernization it will eventually exclude itself from any intervening role in this world. . . . At the societal level, it is forced into silence out of fear of political implications whereas at the cultural level it risks permanently losing contact with a society that believes mainly in a happy ending."

individual grow in confidence, from a creature as subordinate to his gods as he was to his political masters, to one asserting the right to make choices in ever-expanding spheres of behavior, then insisting on the right to define reality, and finally, because the definitions arrived at by any large number of people clash, asserting relativism (what is true for you and what is true for me may be very different things) as the political attitude.”

Individualism leads the modernist to a faith that is “privatized” (Acquaviva 1979, pp. 47–48), to “a more personal type of faith . . . religion which is ‘à la carte’ and a rejection of the doctrine of guilt” (Lambert 1989, p. 58). In a similar vein, Abbruzzese (1995, p. 214; see also Berzano 1995) writes of the contemporary Roman Catholic Church’s attempt to accommodate itself to modernism by reducing “as much as possible the idea of punishment, replacing it with a . . . message of individual freedom and self-realization.” For Wilson (1976, pp. 265–66; 1982), secularization and the individualism that accompanies it results in a “decline of community”—in interpersonal relations based less on ties and obligations to larger communities and less on charity toward others than on individual rights and self-interest (see also Bruce 1995, p. 428).

Bender (1978) notes that dichotomous sociological conceptualizations, such as orthodoxy versus modernism, communitarianism versus individualism, and community versus society, suffer from two problems. First, while it is common to apply one of the two patterns to an entire society (e.g., capitalist economies are individualistic), it is unlikely that any society exhibits only one pattern.⁶ Bender (1978, pp. 32–34) points out that Tönnies ([1887] 1988), himself the creator of the *Gemeinschaft/Gesellschaft* dichotomy, recognized that, in any given society, both patterns exist. With regard to orthodoxy versus modernism, the fact that movements of the religiously orthodox thrive in “modern” European societies (see above) attests to this fact. Second, the assumption of a linear historical progression from traditionalism to modernism, from community to society, and so on, does not fit the historical record of Europe or America (Bender 1978; see also Dobbelaere 1986, p. 116). Our conceptualization assumes that (1) orthodoxy/modernism (and communitarianism/individualism) is a matter of degree, a continuum along which individuals are arrayed, rather than a dichotomy (see Davis and Robinson 1996a, 1997) and that (2) both types of moral cosmology are likely to be present among the individuals in any given society. Further, our model makes no assumption of linear processes of secularization, modernization, individualization, or loss of community.

We argue that modernists, as theological individualists, are also indi-

⁶ See Turner (1983) for a review of theories linking individualism and capitalism.

vidualistic in their beliefs about economic equality. Economic individualism entails the conviction that individuals themselves, and not the larger communities or social structures within which they live, deserve the credit or blame for their economic fortune or misfortune (Feagin 1975). Paralleling the modernists' conception of individually determined fates and moral choices, for economic individualists, "the operating assumption is that people themselves are in charge and that their destiny is largely theirs to decide. . . . Being in control of their own destinies and masters of their fate, people can be held to account for how they exercised that control. Some people, as evidenced by their economic success, have exercised their freedom wisely. Others, who have failed to succeed, have no one to blame but themselves, since it was their decision to have opted otherwise" (Eisinga, Felling, and Lammers 1993, p. 69).

If, in the economic individualist's eyes, the individual is responsible for his or her own success or failure, then the solution to poverty, unemployment, or inequality is not more government spending or private charity to improve the lot of the poor, jobs programs to provide work for the unemployed, or redistribution of wealth from the rich to the poor but greater individual effort on the part of those on the bottom to pull themselves up. In contrast to the communitarianism of the traditionalist, the economic individualism of the modernist entails little obligation to provide government aid or private charity to people who are seen as having only themselves to blame for their economic failure.⁷ If economic individualists believe that anything should be done about poverty and inequality, they are likely to favor greater rewards for individual effort, responsibility, and productivity as a way to stimulate the poor to improve themselves. We hypothesize:

HYPOTHESIS 1.—*Modernists are more economically individualistic or inequalitarian than the religiously orthodox.*

Although cultural attitudes are not our focus here, we note that the theological individualism of modernists may also explain their greater openness to freedom of individual choice in matters of abortion, sexuality, reproductive rights, religious education, and appropriate roles for women and men in the workplace and home (see, e.g., Hayes and Hornsby-Smith 1994; Hayes 1995; Blaschke 1995; Cesareo et al. 1995). Fleishman's (1988) use of the label "individual liberty" for this cluster of cultural attitudes

⁷ See also Hart's (1992, p. 83) distinction between individualistic and communal modes of thought on economic life. Hart's book, which is based on in-depth interviews with a small number of American Christians, suggests the complexity of the relationship between religious and economic thought—a complexity that is, unfortunately, difficult to capture with analyses of survey data.

points to the individualism or libertarianism that underlies such positions on cultural issues.⁸

While our hypothesis on economic issues goes against the conventional wisdom in Europe, few of the studies that have found orthodoxy to be linked to the political right considered issues of economic justice. In this article, we focus specifically on attitudes toward economic equality—whether the government should reduce the gap between rich and poor, whether the government should provide jobs for the unemployed, whether there should be greater rewards for individual initiative, and whether workers should be paid the same regardless of their productivity and efficiency. While many earlier studies focused on a single country or a small subset of Western European countries, we conduct our analyses on data for 21 European countries and Israel. In our earlier research, we found that modernists are more economically individualistic than the orthodox in the United States, a country in which Protestantism is the largest faith tradition (Davis and Robinson 1996a, 1997).⁹ The inclusion of most European countries, as well as Israel, in these analyses allows us to test the generalizability of our hypothesis to a wide range of religious regimes—from predominantly Roman Catholic, Eastern Orthodox, Protestant, or Jewish countries through mixed regimes to countries in which only a minority of the population expresses any religious affiliation. Six Eastern European countries are included in our study—Bulgaria, East Germany, Hungary, Poland, Romania, and Slovenia—allowing determination of whether our argument holds in populations that lived under planned economies for more than 40 years and where religious groups in many cases played a crucial role in challenging and ultimately overthrowing communist regimes (Kepel 1994; Kurtz 1990–91; Tomka 1995).

DATA AND MEASURES

Data

The data that we use to test our hypothesis come from two sources. The European Values Systems Study Group (EVSSG) conducted surveys in 22 European countries from 1990 to 1993 (primarily in 1990), using national random sampling in some countries and quota sampling in others. Data on

⁸ See Davis and Robinson (1999) for a study of how religious cosmology relates to cultural and economic attitudes in Italy.

⁹ For other U.S. studies with similar findings, see Johnson, Tamney, and Halebsky (1986), Tamney, Burton, and Johnson (1989), Jelen (1990, p. 124), and Regnerus, Smith, and Sikkink (1998).

our key dependent and independent variables are available in all countries except Poland and Switzerland. When suggested by the EVSSG, the data for specific countries have been weighted to ensure representativeness of their target populations. The countries analyzed here include: Austria ($N = 1,460$), Belgium ($N = 2,794$), Bulgaria ($N = 1,034$), Denmark ($N = 1,030$), East Germany ($N = 1,336$), Finland ($N = 588$), France ($N = 1,002$), Great Britain ($N = 1,484$), Ireland ($N = 1,000$), Italy ($N = 2,018$), Hungary ($N = 999$), the Netherlands ($N = 1,017$), Northern Ireland ($N = 304$), Norway ($N = 1,239$), Portugal ($N = 1,184$), Romania ($N = 1,103$), Slovenia ($N = 1,035$), Spain ($N = 4,146$), Sweden ($N = 1,047$), and West Germany ($N = 2,100$), where N signifies number of persons.

The second data set analyzed here was gathered from 1990 to 1993 (primarily in 1991) by the International Social Survey Programme (ISSP). Under the auspices of the ISSP, 12 Western and Eastern European countries conducted random surveys of their populations. We also include ISSP data for Israel in our analyses, both because Israel is a country with strong links to Europe and because our arguments should apply to the predominantly Jewish population of this country as well. Data for all countries have been weighted to ensure nationally representative samples when this was deemed necessary by the original investigators. The 13 ISSP countries analyzed here include: Austria ($N = 984$), East Germany ($N = 1,486$), Great Britain ($N = 1,257$), Hungary ($N = 1,000$), Ireland ($N = 1,005$), Israel ($N = 991$), Italy ($N = 983$), the Netherlands ($N = 1,635$), Northern Ireland ($N = 838$), Norway ($N = 1,506$), Poland ($N = 1,063$), Slovenia ($N = 2,080$), and West Germany ($N = 1,346$). Eleven countries are included in both the EVSSG and ISSP studies. For these countries, we present analyses based on both data sources in order to take into account all available information. For some analyses the samples for all countries in the EVSSG or ISSP studies are pooled into a single sample. Since the sample sizes vary from country to country, we weighted the sample for each country proportionate to its population size in creating the pooled samples.

Measures

Religious orthodoxy.—Our indices of religious cosmology in the two data sources are shown in the appendix table A1. In both indices, the items contrast the absolute, timeless moral standards and God-centered, God-directed universe of the orthodox with the relativist, circumstantial, or contextual moral standards and human-centered, human-directed universe of modernists. The scales are, of necessity, composed of different items in each data set, although one item, "Life is meaningful only if God exists," is very similar. The item on the belief in a "personal God" as op-

posed to a “spirit or life force” (EVSSG) and the item on belief in a “God who concerns Himself with every human being personally” (ISSP) indicate belief in a deity who personally oversees the everyday activities of humans and is available to each of them for guidance—the God of the Old and New Testaments in Dogan’s (1995, p. 407) words (see also, Harding, Phillips, and Fogarty 1986, p. 49).

In the ISSP data, respondents are asked about the divine inspiration and inerrancy of the Bible. Religiously orthodox Roman Catholics may rely on Church teachings or papal encyclicals as a source of moral authority instead of, or in addition to, Scripture (Alexander 1985; Arnold 1990; Coleman 1992; Pace 1995). Some scholars similarly have argued that there is no equivalent in Judaism to fundamentalism in Protestantism because there is a strong interpretative Talmudic tradition in Orthodox Judaism, and the Torah, or Jewish Bible, is rarely taken as literally true (Kirschenbaum 1993; Wieseltier 1990). Nonetheless, in response the question on biblical inerrancy, 23.7% of Catholics and 24.3% of Jews, as compared with 10.6% of Protestants, in the ISSP pooled data agreed with the statement that “The Bible is the actual word of God and it is to be taken literally, word for word.” Moreover, the reliability of the scale in predominantly Roman Catholic countries and in Israel is increased by inclusion of the item on the Bible, and the scale correlates much the same with dependent variables regardless of whether this item is included.

In each data set, the scale of religious orthodoxy ranges from 4 to 40 (the four individual items are scored so that each ranges from 1 to 10).¹⁰ Low scores indicate modernism, while high scores indicate religious orthodoxy. In the pooled samples combining data for all countries, factor analyses revealed that the four items in each data set load on the same factor, with eigenvalues of 2.28 in the EVSSG data and 2.78 in the ISSP data. Cronbach’s alpha, a measure of scale reliability, is .70 in the EVSSG data and .85 in the ISSP data.

Although the scales of religious orthodoxy in the EVSSG and ISSP data sets are comprised of different items, analyses of the 11 countries for which data are available from both sources show that the correlation across countries of their average scores on the two orthodoxy scales is .993 ($P < .001$), suggesting that the two scales measure the same underlying concept.

Dependent variable: economic individualism.—The EVSSG and ISSP data sets each contain two items that we use to measure economic individualism (see table A2 in the appendix). The four items on economic individualism, which we analyze separately because neither set of two items forms a reliable scale, capture different aspects of the political de-

¹⁰ We scored the items from 1 to 10 because one of the EVSSG items ranged from 1 to 10.

bate on economic justice in Europe. The ISSP items measure support for state intervention on behalf of the poor and jobless—key planks in the platforms of political parties of the left across Europe. The EVSSG items measure support for efforts by employers, not just the government, in rewarding individual effort, efficiency, and productivity—the “merit” systems or incentive programs stressed by some European parties of the right and center. Note that for all four items the high pole is the economically individualistic or inegalitarian response, so that from our hypothesis we expect negative correlations of orthodoxy with economic individualism.

Control variables.—In some of our analyses, controls are introduced in assessing the effect of religious orthodoxy on economic attitudes. Two of the controls concern other aspects of religion. *Religious service attendance*, widely used in European studies to indicate religiosity, is coded in each data set to reflect the respondent's estimate of the number of times she or he attends religious services in a year.¹¹ *Faith tradition* is measured by a dummy variable series consisting of Protestant, Catholic (including both Roman Catholic and Eastern Orthodox), Jewish, other, and no religious preference (omitted category). Not all of these categories are present in every country. While it is possible, of course, to distinguish countries where Roman Catholicism is the largest faith tradition among those who express a religious preference (Poland, Ireland, Italy, Portugal, Spain, Austria, Belgium, Slovenia, Hungary, the Netherlands, and France) from those where Eastern Orthodoxy is the largest faith (Bulgaria and Romania), neither the EVSSG or the ISSP data allow such a distinction to be made among individuals. Sociodemographic variables are also controlled in these analyses. *Education* is measured in years of schooling. *Income* is household (family in the ISSP) income, with midpoints representing each category.¹² *Age* is measured in years. *Sex* is a dummy variable, with women coded “1” and men “0.”¹³

Statistical Analysis

We use OLS regression techniques to estimate the effect of religious cosmology on economic individualism. We assume that sex, age, education,

¹¹ For an analysis of several different dimensions of religiosity in France, see Michelat (1990). Religious service attendance is not available in the ISSP data for Slovenia and Israel.

¹² In the EVSSG data, household income before taxes was asked. In the ISSP data, before-tax family income was asked in Austria, Great Britain, Northern Ireland, Hungary, Ireland, Israel, Norway, and the Netherlands, while after-tax family income was asked in East and West Germany, Italy, Poland, and Slovenia.

¹³ For theoretical discussions of how sex, age, education, and income affect attitudes toward economic justice see Robinson and Bell (1978), Bell and Robinson (1980), Robinson (1983), and Davis and Robinson (1991).

TABLE 1
MEANS AND SDs OF ORTHODOXY SCALES BY COUNTRY, EVSSG AND ISSP

COUNTRY	EVSSG			ISSP		
	Mean	SD	N	Mean	SD	N
Northern Ireland	29.37	9.31	304	27.91	8.24	802
Poland				26.38	8.17	1,021
Ireland	28.56	8.94	997	25.80	7.90	999
Italy	25.65	10.21	1,999	23.33	9.63	982
Romania	25.40	8.47	1,103			
Portugal	24.78	9.94	1,180			
Spain	22.96	9.53	4,122			
Austria	21.18	8.00	1,460	18.97	9.23	970
Great Britain	21.14	10.38	1,481	18.70	8.67	1,137
Finland	21.08	9.53	559			
West Germany	20.19	9.74	2,066	18.76	9.12	1,303
Belgium	20.11	10.10	2,789			
Israel				19.25	10.87	967
Slovenia	18.93	8.85	1,032	17.84	9.30	1,942
Norway	18.66	10.84	1,115	17.20	9.44	1,340
Hungary	18.55	10.85	998	17.58	11.23	977
Netherlands	18.35	10.17	1,012	16.64	9.85	1,560
France	18.08	9.98	1,002			
Bulgaria	17.01	9.02	990			
Sweden	15.29	9.20	997			
Denmark	14.87	8.79	1,016			
East Germany	14.63	10.79	1,326	12.87	8.63	1,448
Pooled samples	21.03	10.40	27,673	20.38	9.84	15,494

income, religious service attendance, faith tradition, and religious cosmology are causally prior to economic attitudes. While religious cosmology is itself an attitude or belief, we assume that this provides an overarching moral framework from which individuals derive positions on specific economic issues (see, e.g., Wuthnow 1988, p. 219; Hunter 1991, p. 46).

RESULTS

The Religious Landscape of Europe

The 21 European countries and Israel are ranked according to the religious orthodoxy of their populations in table 1. On scales that range from a low of 4 (indicating extreme modernism) to a high of 40 (extreme orthodoxy), the average for these countries is 21.03 in the pooled EVSSG data and 20.38 in the pooled ISSP data. While it has become commonplace to

comment on the extent to which European countries are secularized, the European average leans only slightly in the direction of the modernist pole. Countries vary widely, however, in the orthodoxy of their citizens, from relatively modernist countries, such as East Germany, Denmark, Sweden, and Bulgaria, to relatively orthodox ones, such as Northern Ireland, Poland, Ireland, and Italy. Nonetheless, the standard deviations indicate, as we assumed, that there is a range of moral cosmologies among individuals in every country and that no country could reasonably be classified as "modernist" or "orthodox."

Since the dominant faith tradition of a country is related to the level of orthodoxy/modernism of its population, we show in table 2 the distribution of faith traditions (including and excluding individuals with no religious preference).¹⁴ Predominantly Roman Catholic countries—Poland, Ireland, Italy, Portugal, and Spain—are the most orthodox, while the most modernist countries include some predominantly Protestant countries—Denmark, Sweden, and Norway—and several of the former communist-bloc countries—East Germany, Bulgaria, Hungary, and Slovenia, in some of which large percentages of the population report no religious affiliation (65.0%, 67.5%, 41.8%, and 28.4%, respectively). Northern Ireland, where Protestants make up roughly two-thirds of the population and Roman Catholics one-third, has the most orthodox population, perhaps because religious polarization, reinforced by economic and political divisions (O'Dowd 1993), has led both Protestants and Roman Catholics to staunchly cling to the tenets of their faith. France, where Roman Catholicism is the dominant religion, is ranked considerably lower than other predominantly Roman Catholic countries. As Giorgi (1992, p. 652) points out, France's departure from the Roman Catholic norm may be a vestige of the persistent anticlericalism that followed "the first real secular Revolution of modern times" and of France's prominence in the Renaissance and the Enlightenment (see also Kokosalakis 1993, p. 144). Israel ranks in the middle of the 22 countries in the orthodoxy of its population.

Religious Cosmology and Economic Individualism

We hypothesized that modernists, as theological individualists, should take more economically individualistic or inegalitarian positions than the religiously orthodox. Since many of the scholars who concluded, to the contrary, that modernists are relatively progressive did so based on *bivariate* relationships of measures of religiosity or orthodoxy with attitudes or

¹⁴ Since for Jews the label "Jewish" is perhaps even more an ethnic identification than a religious one, the high percentage of Israelis identifying themselves as Jewish may overstate the extent of identification with the Jewish faith.

TABLE 2

FAITH TRADITION BY COUNTRY (%)

COUNTRY	ALL RESPONDENTS					EXCLUDING RESPONDENTS WITH NO PREFERENCE						
	None	Catholic*	Protestant	Jewish	Other	Total	Catholic*	Protestant	Jewish	Other	Total	N
Northern Ireland	9.7	29.0	60.7	0	.7	100.1	32.1	67.2	.0	.7	100.0	300
Poland	2.9	96.3	.0	0	.8	100.0	99.2	.0	.0	.8	100.0	1,054
Ireland	3.9	93.1	2.9	0	.1	100.0	96.9	3.0	.0	.1	100.0	998
Italy	18.4	80.3	.2	.0	1.0	99.9	98.5	.2	.0	1.3	100.0	2,008
Romania	5.9	90.5	3.4	.0	.2	100.0	96.1	3.9	.0	.2	100.2	1,103
Portugal	27.7	70.4	.3	2	1.4	100.0	97.4	.4	.2	2.0	100.0	1,184
Spain	14.4	84.5	.4	.0	.7	100.0	98.8	.5	.0	.8	100.1	4,130
Austria	16.5	76.2	6.4	2	.6	99.9	91.3	7.7	.2	.7	99.9	1,460
Great Britain	42.4	8.6	47.4	.2	1.4	100.0	14.9	82.3	.4	2.5	100.1	1,479
Finland	11.8	.3	85.0	1.2	1.7	100.0	.4	96.3	1.4	1.9	100.0	586
West Germany	10.7	45.0	43.5	.0	.7	99.9	50.5	48.7	.0	.8	100.0	2,094
Belgium	32.1	65.7	.6	.2	1.3	99.9	96.8	1.0	.3	2.0	100.1	2,773
Israel	1.0	.3	.1	98.0	.6	100.0	.3	.1	99.0	.6	100.0	988
Slovenia	28.4	69.3	.0	.0	2.3	100.0	96.7	.0	.0	3.3	100.0	1,031
Norway	10.0	.8	87.3	.1	1.8	100.0	.9	97.0	.1	2.0	100.0	1,239
Hungary	41.8	42.5	12.7	.1	2.8	99.9	73.1	21.9	.2	4.8	100.0	999
Netherlands	49.6	29.7	16.8	.0	3.9	100.1	58.9	33.3	.0	7.8	100.0	1,013
France	38.6	57.7	1.3	.1	2.3	100.0	94.0	2.1	.2	3.7	100.0	1,000
Bulgaria	67.5	24.6	1.3	.0	6.5	99.9	75.9	3.9	.0	20.2	100.0	1,023
Sweden	.2	.6	77.0	0	22.2†	100.0	.6	77.1	.0	22.3†	100.0	1,047
Denmark	8.4	.7	89.4	0	1.5	100.0	7	97.7	.0	1.6	100.0	1,030
East Germany	65.0	17.2	17.4	.1	.3	100.0	49.0	49.7	.4	.9	100.0	1,335

NOTE.—Based on EVSSG data, except for Poland and Israel, which are based on ISSP data

* Includes Eastern Orthodox and Roman Catholic.

† In Sweden, Swedish Missionary League, Orebro Mission, Salvation Army, Evangelical, Orthodox, and Muslim were coded as "other" by the original investigators

voting behavior, we begin with zero-order analyses that are comparable to theirs.

The zero-order correlations of scales of religious orthodoxy with economic individualism are given for each country in model 1 of table 3. In 13 of the 22 countries, modernists are significantly more individualistic (or inegalitarian) than the religiously orthodox on at least half of the items measuring economic individualism (two out of four or one out of two, depending on the number of items available).¹⁵ (Note that we expect negative correlations under our hypothesis). While the correlations are generally modest, the pattern of association is clear. Countries in which modernists tend to be relatively individualistic include Poland, Ireland, Italy, Romania, Portugal, Austria, West Germany, Israel, Slovenia, Norway, Hungary, Bulgaria, and East Germany. Had we instead hypothesized, following the conventional wisdom in Europe, that modernists are more egalitarian than the orthodox, in only four countries—Spain, the Netherlands, France, and Denmark—would half or more of the correlations with economic individualism have been significantly positive.

In table 3, model 2, we show the standardized regression coefficients for orthodoxy/modernism, controlling for other variables that might be expected to affect economic justice attitudes—education, income, sex, age, frequency of religious service attendance, and faith tradition.¹⁶ Inclusion of the last two control variables presents a particularly rigorous test of the effect of orthodoxy. Religious service attendance is often used in European studies as a measure of religiosity, and since the dummy variable series for faith tradition includes the category “none” as the reference category, we would expect this variable also to parallel our orthodoxy measure. Michelat (1990), for example, finds that several dimensions of religiosity are highly correlated in France. Controlling for sociodemographic and religious variables reduces the associations of orthodoxy and economic attitudes considerably in most countries. The R^2 s in many countries are also modest, as other studies of economic attitudes have found (e.g., Form and Hanson 1985; Knoke, Raffalovich, and Erskine 1987). Nonetheless, half or more of the coefficients of modernism with economic individualism are significant in 10 countries—Poland, Ireland, Italy, Romania, Portugal, Austria, West Germany, Israel, Norway, and Bulgaria.¹⁷ In another four

¹⁵ Since we have predicted direction (i.e., that modernists are more economically individualist or inegalitarian than the religiously orthodox), we use one-tailed tests of significance.

¹⁶ We report standardized coefficients both because they are comparable with the correlation coefficients in model 1 and because we make no prediction as to differences between countries in the effect of orthodoxy.

¹⁷ An Israeli commentator on an earlier draft of this article observed that religiously orthodox Jews tend to be poorer than secular Jews in Israel and thus are more likely

countries—Northern Ireland, Hungary, Great Britain, and East Germany—there is weaker evidence in support of our hypothesis: modernism is significantly related to one of the four items measuring economic individualism.

Had we instead tested the conventional theory that modernists are relatively egalitarian, in only two countries—Denmark and France—would that hypothesis have been supported (i.e., at least half the coefficients of modernism with economic communitarianism are significant). We were unable to find anything in the literature or in our own analyses to suggest why Danish modernists are relatively egalitarian while their Norwegian counterparts are relatively inegalitarian. Sundback (1995), for example, generally treats the Scandinavian countries as cut from the same cloth, noting only that Denmark is more secularized than Norway, as we also find. Regarding the egalitarianism of modernists in France, Percheron (1982, p. 8) observes that “left and right in France today coincide in large part with the ‘red’ and ‘white’ France of the Revolution,” the red referring to the anticlerical revolutionary Republicans and the white to Catholic loyalists. Berger (1987) also traces this association to the Revolution but notes that the Catholic right has begun to break up and that some integralist Catholics have shifted to the left.

Thus, in this stringent test, we find that modernists, where they differ from the religiously orthodox, are far more likely to be to the right than to the left of the orthodox on matters of economic justice. In the countries where modernists tend to be relatively inegalitarian, moral cosmology is often one of the most important determinants of economic justice attitudes (coefficients for control variables are not shown in table 3 to conserve space). In Italy, Poland, Israel, and Portugal, modernism is the most important factor in opposition to government intervention to reduce the income gap, more important than income, education, sex, age, religious service attendance, and faith tradition. In Bulgaria, modernism is the key factor in the belief that more efficient or productive people should be paid more. In Austria, Italy, and Poland, modernism is the most important determinant of opposition to government provision of jobs to those who need them.

The tendency for theological modernism to be linked to economic individualism that we found in our earlier study of the United States, where Protestantism is the largest faith, is generalizable to other countries, including those in which Roman Catholicism, Eastern Orthodoxy, Judaism, or Protestantism predominate. To assess further the generalizability of

to be recipients of state social programs. This might give orthodox Jews an economic interest in government intervention to reduce inequality and provide jobs. The control for income in our model should take into account any such tendency.

TABLE 3

CORRELATION COEFFICIENTS AND STANDARDIZED REGRESSION COEFFICIENTS FOR ECONOMIC INDIVIDUALISM AS REGRESSED
ON RELIGIOUS ORTHODOXY

COUNTRY AND MODEL	EVSSG			ISSP		
	Greater Incentives for Individual Effort	Productive People Should Be Paid More		Government Should Not Reduce Income Gap	Government Should Not Provide Jobs for All	
	Coefficient	N	Coefficient	N	Coefficient	N
Northern Ireland:						
1. No controls006	298	.141	298	-.019	752
2. With controls054	292	.147	292	-.012	744
R^2048		.058		.107	134
Poland:						
1. No controls					-.205***	906
2. With controls					-.177***	894
R^2085	.066
Ireland:						
1. No controls012	991	.023	981	-.156***	967
2. With controls056	985	-.006	976	-.095**	963
R^2074		.054		.092	.093
Italy:						
1. No controls	-.009	1,915	-.050*	1,848	-.103***	967
2. With controls020	1,849	-.019	1,781	-.141***	965
R^2051		.042		.081	.064
Romania:						
1. No controls	-.144***	1,087	-.114***	1,077	-.162***	974
2. With controls	-.052	1,074	-.062*	1,063	-.134***	971
R^2104		.043			

Portugal:					
1. No controls	1,153	-.167***	1,122	-.065**	
2. With controls	1,078	-.078*	1,052	-.049	
R^2130		.020		
Spain:					
1. No controls	3,904	.005	3,815	.033	
2. With controls	3,414	.024	3,323	.011	
R^2036		.018		
Austria:					
1. No controls	1,439	-.097***	1,351	-.003	917
2. With controls	1,410	-.034	1,326	-.017	913
R^2048		.005		.050
Great Britain:					
1. No controls	1,459	-.026	1,433	-.041	1,047
2. With controls	1,438	.002	1,413	-.087**	1,038
R^2062		.040		.100
Finland					
1. No controls	543	.014	503	-.017	
2. With controls	539	.065	501	-.012	
R^2068		.036		
West Germany:					
1. No controls	1,974	-.010	1,836	.013	1,191
2. With controls	1,934	.038	1,805	-.020	1,168
R^2033		.018		.053
Belgium:					
1. No controls	2,682	-.023	2,606	-.019	1,253
2. With controls	2,525	-.006	2,456	-.018	1,230
R^2049		.030		.061
Austria:					
1. No controls	1,439	-.097***	1,351	-.003	917
2. With controls	1,410	-.034	1,326	-.017	913
R^2048		.005		.050
Great Britain:					
1. No controls	1,459	-.026	1,433	-.041	1,047
2. With controls	1,438	.002	1,413	-.087**	1,038
R^2062		.040		.100
Finland					
1. No controls	543	.014	503	-.017	
2. With controls	539	.065	501	-.012	
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West Germany:					
1. No controls	1,974	-.010	1,836	.013	1,191
2. With controls	1,934	.038	1,805	-.020	1,168
R^2033		.018		.053
Belgium:					
1. No controls	2,682	-.023	2,606	-.019	1,253
2. With controls	2,525	-.006	2,456	-.018	1,230
R^2049		.030		.061

TABLE 3 (Continued)

COUNTRY AND MODEL	EVSSG			ISSP		
	Greater Incentives for Individual Effort		Productive People Should Be Paid More		Government Should Not Reduce Income Gap	
	Coefficient	N	Coefficient	N	Coefficient	N
Israel:						
1. No controls						955
2. With controls						861
R ²						
Slovenia:						
1. No controls	-.203***	975	-.078**	970	-.112***	1,847
2. With controls	-.045	903	-.038	898	.006	1,805
R ²165		.028		.088	
Norway:						
1. No controls001	1,107	-.065*	1,078	-.143***	1,205
2. With controls033	1,063	-.084*	1,034	-.064*	1,142
R ²066		.057		.075	
Hungary:						
1. No controls	-.146***	952	-.176***	960	-.102***	960
2. With controls000	939	-.171***	948	-.057	956
R ²140		.063		.067	
Netherlands:						
1. No controls099	1,004	.061	979	-.011	1,475
2. With controls060	995	.140	969	.036	1,451
R ²076		.053		.064	

France:						
1. No controls	.110	983	.015	944		
2. With controls	.095	896	.004	861		
R^2	.072		.048			
Bulgaria:						
1. No controls	-.097***	953	-.147***	940		
2. With controls	-.046	893	-.142***	884		
R^2	.093		.044			
Sweden:						
1. No controls	-.048	986	.008	927		
2. With controls	-.046	878	.004	829		
R^2	.059		.054			
Denmark:						
1. No controls	.104	994	.045	966		
2. With controls	.113	975	.080	946		
R^2	.042		.053			
East Germany:						
1. No controls	-.106***	1,300	-.068**	1,299	1,343	1,422
2. With controls	-.104*	1,291	.002	1,290	1,326	1,403
R^2	.042		.014	.052	.036	.036

NOTE.—Zero-order correlation coefficients (model 1) and standardized regression coefficients (model 2) controlling for education, income, sex, age, religious service attendance, and faith tradition

* $P < .05$, one-tailed test.

** $P < .01$

*** $P < .001$

this pattern, we compare the Protestant and Roman Catholic populations in the mixed religious regimes of Northern Ireland, Great Britain, West Germany, Hungary, the Netherlands, and East Germany. In the Protestant but not the Catholic population of West Germany, modernists are significantly more individualistic than the orthodox on at least half of the items measuring economic individualism (see table 4). Indeed, in the Protestant community in West Germany, modernism is the single most important factor in opposition to government intervention to reduce inequality and in opposition to government jobs provision (details available on request). In Northern Ireland and the Netherlands, the expected pattern is more evident among the Protestant population than among the Catholic population (one out of four standardized regression coefficients of modernism on economic individualism is significant for Protestants; see table 4). Among Dutch Protestants, modernism is the most important factor in opposition to government efforts to provide jobs to those who need them (details available on request). A survey of two orthodox Protestant groups in the Netherlands—Orthodox Reformed Christians and Evangelical Christians—found that the former did not differ from the general Dutch population (which was quite egalitarian in itself) in their position on reduction of class and income differences, but the latter were decidedly more egalitarian on this question (Dekker and Stoffels 1993). In the remaining three mixed religious regimes—Great Britain, Hungary, and East Germany—where modernism is related to one of the four indicators of economic individualism, there are no differences between Protestants and Catholics (not shown in table 4, details available on request). Thus, while in some mixed religious regimes the effect of modernism on economic individualism appears stronger among Protestants than among Catholics, this is not the case in all such regimes.

DISCUSSION AND CONCLUSION

Through analyses of data for 21 European countries and Israel, we tested the hypothesis that modernists are more economically individualistic or inegalitarian than the religiously orthodox. The conventional wisdom among European scholars is the reverse—that modernism is associated with the political left and orthodoxy with the right. We argued that at the core of the modernist moral cosmology is theological individualism—the belief that individuals make their own fates and must decide for themselves what is right and wrong. We reasoned that modernists, as theological individualists, are more likely than orthodox religionists to take individualistic stances on economic issues. Economic individualists hold each person, not the state or larger community, accountable for his or her own economic fortune or misfortune. The solutions to poverty, joblessness,

and economic inequality, in the view of economic individualists, must come from within individuals themselves and not from communitarian efforts such as social welfare programs, jobs provision, taxation of the rich, or private charity. Thus we hypothesized that modernists would be more likely than the religiously orthodox to oppose government intervention on behalf of the poor and jobless and to favor "merit" systems or incentive programs as a means of spurring the poor to improve themselves.

Our analyses of the effect of moral cosmology on economic attitudes support our hypothesis more than the conventional wisdom about religion and politics in Europe. In countries where the religiously orthodox differ from modernists in their economic beliefs, the orthodox are far more likely to be to the left of modernists than to the right. The link between modernism and economic individualism that we found in our earlier study of the United States (Davis and Robinson 1996a), a country in which Protestantism is the largest faith tradition, is generalizable to other countries, including countries in which the largest faith tradition is Roman Catholic (Poland, Ireland, Italy, Portugal, and Austria), Eastern Orthodox (Romania and Bulgaria), Protestant (Norway, Protestants in West Germany), or Jewish (Israel). The association of theological modernism with economic individualism appears to have no boundaries of faith.

Just as the association we hypothesized between theological modernism and economic individualism is not limited to a single faith tradition, neither does it appear to be limited to a single economic system—capitalism. The six Eastern European countries included in our study—Bulgaria, East Germany, Hungary, Poland, Romania, and Slovenia—present an especially interesting test of our hypothesis in view of their having experienced planned economies for over 40 years until just before our data were collected (Kepel 1994; Kurtz 1990–91; Tomka 1995). In the zero order, the religiously orthodox tend to be more egalitarian or communitarian than modernists on questions of economic justice in all six countries. This tendency holds up with controls for other religious dimensions and social characteristics in three countries—Poland, Bulgaria, and Romania (see table 3). That the religiously orthodox in Poland tend to be more favorable toward economic redistribution than modernists is especially surprising in view of the strong lay Catholic roots of the Solidarity movement that overthrew the communist regime in this country (Robertson 1989; Kepel 1994). The struggle of religious people against communist regimes may have had more to do with the restrictions on religious and political freedom imposed by some of these regimes or with their corruption of communist ideals than with the regimes' modest efforts at economic redistribution. It clearly is not religious traditionalists in the former communist countries who are pushing for unbridled capitalism.

TABLE 4
CORRELATION COEFFICIENTS AND STANDARDIZED REGRESSION COEFFICIENTS FOR ECONOMIC INDIVIDUALISM AS REGRESSED
ON RELIGIOUS ORTHODOXY

COUNTRY AND MODEL	EVSSG				ISSP			
	Greater Incentives for Individual Effort		Productive People Should Be Paid More		Government Should Not Reduce Income Gap		Government Should Not Provide Jobs for All	
	Coefficient	N	Coefficient	N	Coefficient	N	Coefficient	N
Northern Ireland—Protestants:								
1. No controls	-.004	179	.172	178	-.035	408	-.191***	397
2. With controls	.034	178	.107	177	-.036	407	-.118*	397
R ²	.019		.056		.058		.164	
Northern Ireland—Catholics:								
1. No controls	.035	84	.145	85	-.079	256	-.133*	256
2. With controls	.111	83	.189	84	-.023	256	-.092	256
R ²	.078		.071		.107		.075	

West Germany—Protestants:									
1. No controls	.023	853	.019	782	-.107**	537	-.109**	563	
2. With controls	.043	836	-.022	770	-.158***	531	-.156***	557	
<i>R</i> ²	.035		.024		.064		.054		
West Germany—Catholics:									
1. No controls	-.026	887	.008	837	-.090*	497	-.108**	525	
2. With controls	.023	872	-.001	823	-.066	486	-.113*	514	
<i>R</i> ²	.034		.011		.033		.063		
Netherlands—Protestants:									
1. No controls	.025	168	.077	165	-.001	279	-.173**	288	
2. With controls	-.027	166	.082	163	.043	275	-.165**	284	
<i>R</i> ²	.077		.022		.058		.068		
Netherlands—Catholics:									
1. No controls	.039	300	.128	293	-.079	318	-.058	329	
2. With controls	-.031	298	.098	291	-.056	315	-.021	326	
<i>R</i> ²	.042		.075		.058		.050		

NOTE.—Zero-order correlation coefficients (model 1) and standardized regression coefficients (model 2) controlling for education, income, sex, age, and religious service attendance.

* $P < .05$, one-tailed test

** $P < .01$.

*** $P < .001$

Our analyses call into question not only the conventional wisdom in Europe that religious orthodoxy is associated with the political right while modernism is associated with the left, but also the concept of "left/right" itself. As Bobbio (1996, pp. 1–16) notes, many European writers have commented on the increasing irrelevance of this concept. As we noted above, the conservatism of the religiously orthodox relative to modernists on such cultural issues as abortion, reproductive rights, sexuality, prayer in public schools, and gender roles has been documented in a number of European countries (Hayes and Hornsby-Smith 1994; Hayes 1995; Blaschke 1995; Cesareo et al. 1995). In this article, we find that the orthodox and modernists in many European countries line up in a seemingly opposite direction with respect to attitudes toward economic equality.

That individuals may position themselves differently on cultural and economic issues has been recognized for some time. Lipset (1981) distinguished between cultural conservatism and economic liberalism, the former referring to efforts to restrict freedoms with respect to sexuality, reproduction, and religious education and the latter to efforts to reduce economic inequalities (see also Klatch 1992). Fleishman (1988), in a factor analysis of attitudes in a national sample of Americans, identified two similar dimensions: "individual liberty," including sexuality and reproductive rights issues, and "economic welfare," involving issues of government spending on welfare and the cities and efforts to reduce the gap between rich and poor. Bobbio (1996, p. 60), in a recent review of the concept of "left" and "right," concludes that "the criterion most frequently used to distinguish between the left and the right is the attitude . . . to the ideal of equality"—that is, the economic dimension that we have examined here. Nonetheless, to locate individuals in political space, Bobbio argues that another dimension—"liberty/authoritarianism"—is needed. Although he does not apply this concept specifically to cultural issues, it would appear to extend to such issues.

What links the two dimensions identified by these authors, at least as they relate to religious cosmologies, is individualism. Modernists may be both relatively egalitarian on concerns of economic equality and relatively libertarian on issues of sexuality, reproduction, religious education, and gender roles (placing them on seemingly opposite ends of the two spectrums) because their positions on both dimensions are individualistic. The ideological configuration of economic and cultural individualism that we find among European modernists today has its precedent in classical liberalism, a European political philosophy that is not the same as the American political label of the same name. Contemporary modernist and orthodox positions on economic and cultural issues make sense in light of the struggles in Europe beginning early in the 19th century of orthodox Roman Catholics and Protestants against liberal political and social elites.

Liberalism coupled laissez-faire economic individualism with freedom of thought, speech, religion, and the press (Burns 1990, 1992; Shively 1997). It was arguably the struggle of socialists and social democrats against business elites and clerics beginning in the late 19th century that led to the dissociation of economic and cultural individualism and to the construction in its place of a "right" that is economically inegalitarian and culturally restrictive and a "left" that is economically egalitarian and culturally libertarian.

In separate studies of the United States and Italy, we found that modernists, relative to the religiously orthodox, are individualistic or inegalitarian on economic issues and are individualistic or libertarian on cultural issues (Davis and Robinson 1996a, 1999). In Italy, where we explored the political implications of these beliefs, we found that moral cosmology has no net effect on voting behavior, suggesting that neither modernists nor the orthodox can find a party representing both their economic and their cultural beliefs. We must leave to future researchers to determine how the cultural and economic attitudes of modernists and the orthodox express themselves in other political contexts. It is clear that state structures and party systems are critical in turning beliefs into votes and, ultimately, into policy (Lipset and Rokkan 1967). The centralized government structure and tight party system in Britain, for example, have not allowed Evangelical Christians the leverage over individual politicians that they have sometimes exercised in the United States (Bebbington 1994; Soper 1994). How much importance voters attach to their economic concerns as opposed to their cultural concerns, whether party platforms give voice to both economic and cultural agendas, and, if so, how they configure the two agendas undoubtedly also have much to do with how the economic and cultural individualism of modernists and the contrary inclinations of the orthodox play themselves out in the political arena.

APPENDIX

TABLE A1
MEASUREMENT ITEMS FOR RELIGIOUS ORTHODOXY

Item	Response	Scaling
European Values Systems Study		
Group:		
"Here are two statements which people sometimes make when discussing good and evil. Which one comes closest to your own point of view?"	Agree with statement B	1
	Disagree with both	5.5
	Agree with statement A	10
'A. There are absolutely clear guidelines about what is good and evil. These always apply to everyone, whatever the circumstances.'		
'B. There can never be absolutely clear guidelines about what is good and evil. What is good and evil depends entirely upon the circumstances at the time.' "		
"I am going to read out a list of statements about the meaning of life. Please indicate whether you agree or disagree with each of them 'Life is meaningful only if God exists' "	Disagree	1
	Neither	5.5
	Agree	10
"And how important is God in your life? Please use this card to indicate [any number between 1 and 10]—'10' means very important and '1' means not at all important."	Not at all important	1
	Very important	10
"Which of these statements comes closest to your beliefs?"	I don't really think there is any sort of spirit, God, or life force.	1
	I don't really know what to think	4
	There is some sort of spirit or life force	7
	There is a personal God.	10
International Social Survey Programme:		
"How much do you agree or disagree with each of the following statements? Right and wrong should be based on God's laws."	Strongly disagree	1
	Disagree	3.25
	Neither agree nor disagree	5.5
	Agree	7.75
	Strongly agree	10

TABLE A1 (Continued)

Item	Response	Scaling
"Which one of these statements comes closest to describing your feelings about the Bible?"	The Bible is an ancient book of fables, legends, history, and moral teachings recorded by man.	1
	The Bible is the inspired word of God but not everything should be taken literally, word for word.	5.5
	The Bible is the actual word of God and it is to be taken literally, word for word.	10
"Agree or disagree: 'To me, life is meaningful only because God exists.' "	Strongly disagree	1
	Disagree	3.25
	Neither agree nor disagree	5.5
	Agree	7.75
	Strongly agree	10
"How much do you agree or disagree with each one of the following? 'There is a God who concerns Himself with every human being personally.' "	Strongly disagree	1
	Disagree	3.25
	Neither agree nor disagree	5.5
	Agree	7.75
	Strongly agree	10

TABLE A2

MEASUREMENT ITEMS FOR ECONOMIC INDIVIDUALISM

Item	Response	Scaling
European Values Systems Study		
Group:		
"Now I'd like you to tell me your views on various issues. How would you place your views on this scale? '1' means you agree completely with the statement on the left, '10' means you agree completely with the statement on the right, or you can choose any number in between."	Incomes should be made more equal	1
	There should be greater incentives for individual effort	10
"Imagine two secretaries, of the same age, doing practically the same job. One finds out that the other earns \$50 a week more than she does. The better paid secretary, however, is quicker, more efficient and more reliable at her job. In your opinion, is it fair or not fair that the one secretary is paid more than the other?"	Unfair	1
	Fair	2
International Social Survey Programme.		
"On the whole, do you think it should or should not be the government's responsibility to provide a job for everyone who wants one?"	Definitely should be	1
	Probably should be	2
	Probably should not be	3
	Definitely should not be	4
"On the whole, do you think it should or should not be the government's responsibility to reduce income differences between the rich and the poor?"	Definitely should be	1
	Probably should be	2
	Probably should not be	3
	Definitely should not be	4

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Bending with the Wind: Strategic Change and Adaptation by Women's and Racial Minority Organizations¹

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This article provides an integrated analysis of social movement organizational change and survival based on the activities of national women's and racial minority organizations during 1955–85. Results demonstrate that core transitions in social change strategies are influenced in contradictory ways by the social movement environment. Older and more formalized movement organizations are more flexible, but the kinds of changes undertaken are not necessarily conservative. The benefits of transformation are limited, however, and organizational change increases the risk of failure with little evidence of a declining effect over time. In the long run, this shapes the organizational system in ways that potentially improve its legitimacy but may also limit the development of an infrastructure for future mobilization.

INTRODUCTION

When it was established in 1910, the National Urban League (NUL) characterized itself as a direct service agency operating with the express goal of improving the status of African-Americans through the provision of educational, economic, and social welfare services. By the early 1960s, as the Civil Rights movement took off at the national level, the NUL began to take a more activist stance. Despite initial reservations among the executive committee, the NUL became both a sponsor of and participant in the 1963 March on Washington. This marked the effective "transformation of the league from a social service agency to a civil rights organization without abandoning its historic commitments to the promotion of the economic and social welfare of black Americans" (Weiss 1989, p. 124).

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The National Woman's Party (NWP) was formed just three years after the NUL. In rather stark contrast, the NWP was a militant suffrage organization that picketed the White House and frequently provoked arrests at their demonstrations (Rupp and Taylor 1990). After women's voting rights were obtained, the organization shifted its efforts toward the single goal of passage of the Equal Rights Amendment. As Rupp and Taylor recount, although the National Woman's Party did not pursue militancy after suffrage was won, during its ERA mobilization in the 1940s and 1950s, it otherwise "followed the path laid down in the early years in every other way" (Rupp and Taylor 1990, p. 43). A changed political environment prompted a shift toward cultural activities that were centered in the Alva Belmont House in Washington, D.C., which provided a "feminist space" for activist women during a period of retrenchment. Today, the National Woman's Party remains committed to passage of the ERA and sponsors advocacy and cultural efforts toward this goal.

These two examples illustrate the importance of flexibility for social movement organizations and movement-affiliated associations in the face of changing circumstances and of being able to "bend with the wind" in order to survive (Powell and Friedkin 1987).² However, despite a tradition of research on structural and goal transformation among political and social organizations, and more recent work on the dynamics of change in organizational populations from an ecological perspective, little is known about which organizational attributes or what environmental circumstances facilitate change between diverse forms of social movement organization (see Powell and Friedkin [1987] and Barnett and Carroll [1995] for useful reviews of each line of research). Nor is it at all clear whether flexibility provides an adaptive edge in the context of highly variable political and resource environments or whether such circumstances promote increased activism or conservatism in social movement organizing styles. Such issues are critical, since processes of organizational change have important implications for the continued viability of organizational populations.

² I use the terms social movement organizations and movement-affiliated voluntary associations to differentiate between organizations that are committed to similar broad social change goals but adopt different organizing strategies to common ends. Social movement organizations comprise groups that use outsider methods of institutional challenge at least some of the time (Burstein, Einwohner, and Hollander 1995), as well as what McCarthy and Zald (1973) refer to as professional advocacy organizations (see also Jenkins 1987; Kriesi 1996). Movement-affiliated associations conform more closely to conventional models of voluntary activity, pursuing indirect means toward equality and group participation, for example, service provisions targeted at specific constituencies or cultural activities that seek to improve intergroup relations or collective empowerment without substantial political or institutional reform (Minkoff 1995).

In this article, I build on recent work in macro-organizational theory to develop an integrated analysis of social movement organizational change. Following the approach recommended three decades ago by Zald and Ash (1966), I explicitly analyze how the social movement environment and organizational attributes interact to influence the timing, direction, and consequences of organizational change by national women's and racial minority organizations during 1955–85. I address the following questions: First, how do changes in the social movement environment influence the rate of organizational change, and do external pressures promote conformity or activism in social change strategies? Second, what organizational features promote or inhibit flexibility in the face of changing circumstances, and what specific types of change do they influence? Third, are changes in core strategies adaptive or disruptive; that is, do they increase or diminish an organization's risk of disbanding? A corollary issue is whether conservative changes offset any liabilities associated with organizational transformation. Finally, does the timing of these changes or the kinds of organizations attempting them play an important role in determining organizational life chances? Taken together, these questions provide insight into what structural-organizational features situate social movements to successfully "bend with the wind" and continue their activities under changed circumstances and with new core organizational identities.

My focus is on changes in one dimension of organizational identity: dominant movement strategy. I conceptualize strategy in terms of the organization's primary activities: social protest, institutional advocacy, and service provision or cultural activities.³ Strategy represents what Hannan and Freeman (1984) characterize as a core feature of the organization that critically shapes its ability to mobilize support from members, sponsors, and authorities. These dominant strategies also represent a continuum of institutional challenge or conformity to established methods of social and political participation, which confer distinctive levels of legitimacy vis-à-vis established elites and the public (Minkoff 1994). Service and cultural

³ Protest organizations use outsider tactics or disruptive means—such as sit-ins, die-ins, and marches—to influence policies, public officials, or public opinion. Advocacy groups use routine means like lobbying and litigation to influence policies and public opinion in a variety of institutional settings, from the political-electoral arena to religious settings and the media (Jenkins 1987). Service organizations target private resources and services to the constituency, such as job training, community development programs, or shelters for battered women, while cultural organizations emphasize cultural and ideological activities such as sponsoring arts festivals, maintaining libraries or museums, and media production efforts. Service provision and cultural production are formally similar strategies, in that neither implies direct institutional confrontation or change.

organizations conform most closely to conventional models of associational activity by focusing their strategies on individual change and providing services and resources for social, economic, political, and cultural participation or empowerment. Advocacy organizations that emphasize policy reform and group representation are an intermediate form of institutional challenge. Groups that pursue social change through protest represent the most direct institutional threat, both practically and symbolically (Kriesi 1996; McAdam 1982, 1996). As McAdam (1982, p. 57) suggests, "the myriad tactics available to insurgents communicate varying degrees of threat to other organized groups in the political environment. . . . Even if used to pursue 'radical' goals, the former [institutionalized tactics] implicitly convey an acceptance of the established, or 'proper,' channels of conflict resolution." Changes along this dimension of organizational identity, then, are not undertaken without the potential for significant disruption in established routines, interorganizational relationships, and organizational legitimacy (Delacroix and Swaminathan 1991; Hannan and Freeman 1984).

The period covered by this research marks dramatic shifts in the context for voluntary action and activism, providing the opportunity to examine closely how environmental and organizational factors influence organizational change and the relationship between change and survival. By 1955, the southern Civil Rights movement was beginning to emerge as an organized force for change (McAdam 1982), initiating a "cycle of protest" later in the 1960s with broad significance (McAdam 1996; Minkoff 1997; Tarrow 1994). The various movements of the time—civil rights, women's, antiwar, New Left, and countercultural—were indicative of an open environment for social action that was (and is) unmatched in U.S. history. The legislative and policy efforts associated with Kennedy's War on Poverty, Johnson's Great Society, and civil rights reform likewise signaled significant changes in the legal, political, and social environment. With the 1968 election of Richard Nixon, the political opportunity structure began to close, leaving behind a more formally open legal environment (Burstein 1985) and an improved funding environment for interest groups and professionalized social movement organizations (Jenkins 1985, 1987; Walker 1991). At the same time, the 1960s culture of radicalism was replaced by increased political and social conservatism that continued throughout the 1980s (Gitlin 1987).

In the next section of this article, I elaborate testable hypotheses regarding the environmental and internal correlates of organizational change and the relationship between core changes in strategy and the prospects for organizational survival. After describing the study design, methods, and data, I test these hypotheses using event history methods. The results demonstrate that social movement organizations face contradictory pres-

tures for change from the environment and that the decision to alter their core identity places them at greater risk of failure. More established groups are better situated to take advantage of favorable opportunities for change, and although organizers may be able to institute organizational structures that enable them to weather external discontinuities, such investments in organizational maintenance may not be very effective. In the long run, the joint processes of adaptation and selection shape the organizational system in ways that potentially improve the legitimacy of the national social movement sector but may limit the development of an infrastructure capable of promoting and maintaining future mobilization.

THE PROCESS, CONTENT, AND CONSEQUENCES OF ORGANIZATIONAL CHANGE

Early studies of organizational change emphasize internal organizational politics in shaping and reshaping the goals, structure, and strategies of social movement and voluntary organizations. In such accounts, ideological conflict (Zald and Ash 1966), leadership succession and the routinization of charisma (Weber [1922] 1978), and such processes as goal displacement and the "iron law of oligarchy" (Michels 1957) provide the motor for organizational change as groups seek to insure the survival of the organization—whatever the cost. Such research also documents that organizational change among political and social organizations is likely to be conservative, although there are exceptions. One better-known example is the Townsend Movement (Messinger 1955), which was initially a radical change group advocating a guaranteed income for citizens over 65 but became little more than a social and recreational club after passage of the Social Security Act (the benefits of which most of its members could not claim). Helfgot's (1974) study of Mobilization for Youth represents a case where resource constraints and normative pressures first promoted the group's transformation from a service agency to a radical community action program and then later back to a manpower development agency that stressed personal adjustment via vocational training. Useem and Zald (1987) provide a contrasting example of the partial transition from established pressure group to social movement that pronuclear activists accomplished during the 1970s, largely in response to the successful mobilization of the anti-nuclear power movement.

This view of social movement organizations and voluntary associations as both flexible and adaptive in response to changes in the political and resource environment contrasts sharply with more recent attention to organizational change using macro-organizational perspectives, particularly ecological approaches. Early formulations of organizational ecology sug-

gest that organizations respond slowly to environmental threats and opportunities and that they are more likely to disband than adapt (Hannan and Freeman 1984). In a recent review of empirical studies, Barnett and Carroll (1995) find that a narrow majority of this work confirms the basic prediction of Hannan and Freeman's (1984) structural inertia theory. In particular, core changes, such as publisher succession or changes in the content or frequency of newspapers, service area shift among voluntary social service organizations, product changes by semiconductor manufacturers, and technological changes implemented by telephone companies, are associated with a higher risk of organizational mortality (see, e.g., Singh, Tucker, and Meinhard 1991). However, even within this research there is evidence that organizational change can be an adaptive response to environmental contingencies.

I conceptualize the process of social movement organizational change as an effort to respond to shifts in the social movement environment that alter the viability of different forms of activism. However, core changes are expected to be disruptive and re-expose recently altered groups to the "liability of newness" that stems from needing to reconstruct routines and relationships with the environment (Stinchcombe 1965; Delacroix and Swaminathan 1991; Amburgey, Kelly, and Barnett 1993). My premise is that "institutional changes that redefine the feasibility of alternative models of organizing, the impact of patterned variation in the resource environment, and environmental discontinuities that reinforce, and sometimes undermine, existing organizational competencies" (Brittain 1994, p. 360) are crucial in altering the openness of environmental space for the development of organizational forms. In particular, open social movement environments improve the feasibility of pursuing greater activism as officials and the public become more willing "to accept the legitimacy and value of the social movement and its organizational manifestations" (Zald and Ash 1966, p. 330). Restrictive environments, in contrast, present pressures to adopt less confrontational models of action, as organizations become more vulnerable to processes of "institutional channeling" that favor isomorphism with more established or legitimate organizational forms (DiMaggio and Powell 1983; McCarthy, Britt, and Wolfson 1991). At the same time, core organizational attributes such as structure, goals, and a repertoire of prior flexibility situate movement organizations differently in terms of their ability to successfully negotiate environmental variation and continue their objectives with a new organizational identity. Whereas characteristics associated with institutional legitimacy and organizational flexibility promote responsiveness to environmental contingencies and offset the disruptive effects of core change, organizational goals set an outer limit on flexibility and survival.

The Social Movement Environment

Three dimensions of the social movement environment are particularly salient with respect to the timing and feasibility of adopting alternative organizational identities: the political opportunity structure, protest cycle dynamics, and resource availability.⁴ Although there is little direct theorizing about the relationship between political opportunities and organizational change, it has been demonstrated that such factors as the presence or absence of political allies and shifts in the political balance of power are critical in shaping social movement activity and organizational dynamics across a range of constituencies (see Costain [1992], McAdam [1982], and Minkoff [1995] on feminist and black activism). Following from the view that openings in the political opportunity structure decrease the risks associated with collective political challenge (Tilly 1978; Tarrow 1994), favorable political conditions should not only diminish the need for social movement organizations to moderate their activities but also improve the prospects for more activist-oriented changes, as organizers take advantage of their constituency's greater political leverage or support. On the other hand, just as contractions in the political opportunity structure tend to diminish levels of movement activity and promote the institutionalization of social movements, such downturns are expected to promote conservative changes as organizations attempt to maintain at least a minimal political presence, as illustrated by the example of the National Woman's Party introduced earlier.

Independent of political variations structured by the electoral system, protest cycle dynamics are also expected to influence the rate of organizational change. Cycles of protest are characterized by the rapid diffusion of contentious collective action across multiple constituencies, increased levels of both organized and unorganized participation, and innovations in the collective action repertoire (Tarrow 1994). Tarrow (1994) argues that as protest cycles peak, established organizations become more radical as they compete with newer, more activist-oriented organizations for mass

⁴ Two other dimensions of the environment are also theoretically relevant: competitive and institutional conditions. However, in exploratory analysis, I found that for this organizational population, political and resource conditions were more substantively significant. For example, neither general controls for organizational density or for origin and destination densities (Baum and Singh 1996; Haveman 1994) influenced the rate of change between core strategies. Nor did a measure of changes in the legal-institutional environment, indexed as the period following the passage of the Civil Rights Act of 1964 and the Voting Rights Act of 1965 (Burstein 1985; Edelman 1992). An argument could also be made for examining the influence of the "cultural environment" on organizational change, but given that the analysis is limited to a single political culture over a relatively short time period, it seems reasonable to treat political culture as constant.

support and public attention (see also Koopmans 1993). The rapid proliferation of civil rights organizations during that movement's "heyday," as well as the increased activism by such organizations as the National Urban League, provide some evidence for this view (McAdam 1982).

The potential influence of resource availability is more mixed. On the one hand, increases in funding may provide a less competitive environment within which organizations are willing and able to experiment with core changes in their identity, especially taking advantage of opportunities for increased activism. For example, foundation funding of social change along the lines of the Ford Foundation's involvement in community action programs in the 1960s promoted the development of more activist organizations as groups took advantage of new sources of support (McCarthy and Zald 1973). However, under conditions of resource scarcity, social movement organizations are more inclined to alter confrontational tactics into more mainstream or acceptable approaches to social change (Powell and Friedkin 1987; Zald and Ash 1966). In general, grant makers tend to favor organizations with more conservative, bureaucratic structures, as well as those serving more established constituencies and pursuing more institutionally acceptable goals and activities (Jenkins 1985). Accounts of the Civil Rights movement detail just such a trend at the movement level, with foundation funding favoring comparatively more moderate organizations such as the National Association for the Advancement of Colored People and the Southern Christian Leadership Conference, suggesting a "channeling" effect (Jenkins and Ekert 1986; McAdam 1982).⁵

The premise underlying this discussion is that certain transitions are more activist or conservative than others. In particular, a shift from either advocacy or service provision toward a protest form of organization signals a more direct threat to authorities, with the potential consequence that the group will sacrifice some degree of institutional support and face a greater risk of failure as a result. In contrast, a shift from either advocacy or protest toward a more conventional strategy is expected to confer survival advantages as organizations conform more closely to institutional rules and expectations about appropriate models of organization (DiMaggio and Powell 1983; Meyer and Rowan 1977). Whereas advocacy is an intermediate strategy—one that entails the use of institutional means for

⁵ As one reviewer noted, moderation is "in the eye of the beholder." For example, SCLC was considered to be a radical organization by southern law enforcement officials, and the NAACP was banned in a number of states because of its supposed Communist affiliation. The research cited on foundation funding of social movements demonstrates, however, that more mainstream organizations benefited from a "radical flank" effect (Haines 1984) that positioned them as a less threatening, i.e., more moderate, alternative to groups such as SNCC and the Black Panthers.

social change—a transition from protest to advocacy is likely to decrease the risk of failure for groups making such changes. It is also possible that when service organizations try to adopt an advocacy agenda, they move closer onto the terrain of direct social movement activism and jeopardize their survival chances (Kriesi 1996).

The consequences of change are not only likely to vary as a function of the content of change, but they also depend on the external context within which such transitions are attempted. That is, there is likely to be an interaction between the content of change and the timing of change (Barnett and Carroll 1995). In general, changes attempted when the social movement environment is relatively open are likely to offset expected liabilities of transformation. The availability of slack resources and greater acceptance of the movement by officials and the public would seem to be ideal conditions to experiment with new organizational identities. This implies that activist changes undertaken during peaks of protest cycles or when political and resource conditions are favorable are less likely to increase the failure rate than if such transitions are attempted when political and resource conditions are restrictive. By the same token, conservative changes undertaken in open social movement environments may not enhance organizational survival since supporters may challenge the organization's willingness to become more moderate at a time when greater levels of institutional challenge seem possible.

The following hypotheses summarize the expected relationships between changes in the social movement environment, the rate and content of organizational change, and the consequences for survival:

HYPOTHESIS 1.—*Improvements in the social movement environment will increase the rate of strategic change among social movement organizations, particularly activist changes, whereas contractions in the environment will increase the rate of conservative change.*

HYPOTHESIS 2.—*Changes in the direction of greater activism will increase the risk of organizational failure, whereas transitions that signal greater institutional conformity will decrease the risk of failure.*

HYPOTHESIS 3.—*Activist changes during favorable resource and political conditions may offset the disruptive effects of organizational change, whereas conservative changes in open environments are not likely to provide significant survival advantages.*

Core Profiles and Adaptation

The characteristics of the organizations attempting to alter their core identity are also critical with respect to the probability of change and the relationship between change and survival. In particular, structural-organizational attributes such as age, size, and formalization may im-

prove the ability of social movement organizations to capitalize on environmental change and withstand potential disruptions. Recent work also examines the influence of prior experience with organizational change in enhancing an organization's flexibility (Amburgey et al. 1993; Delacroix and Swaminathan 1991). In the case of social movement organizations and voluntary associations, the organization's goals are especially critical (Zald and Ash 1966).

Structural-organizational attributes.—Older organizations are sometimes conceptualized as unresponsive to external pressures, in part because they must be attentive to the expectations of established members and sponsors. This is referred to as a "rigidity of aging" thesis (Singh and Lumsden 1990). An alternative view, referred to as the "fluidity of aging" hypothesis, is that as organizations age they are in fact more likely to experiment with change, since investment in organizational maintenance calls for an ability to adapt to changing environmental circumstances. Related studies of voluntary social service organizations and day care centers suggest that this is the more common form of age dependence among a range of nonprofit organizations (cited in Kelly and Amburgey 1991). Anecdotal evidence that social movement and affiliated organizations are more flexible as they become older was provided earlier: the NUL had been active for over 50 years when it shifted to a civil rights orientation, whereas the NWP initiated substantial changes after roughly 30 years of activity.

Organizational size and degree of administrative formalization are also expected to situate organizations differently with respect to their ability to adapt to changing circumstances. Haveman (1993), for instance, has documented an association between organizational size, bureaucratic rigidity, and inertia. For social movement organizations, formalization may be antithetical to change in much the same way that formal movement organizations are less capable of innovation in general (Staggenborg 1988). At the same time, larger organizations are also more likely to be differentiated and decentralized, which may enable them to undertake core changes without suffering negative consequences (Starbuck 1965). This may be particularly true of organizations that attempt to shift toward service activities, which are more resource intensive than advocacy or protest forms of organization. The two contrasting hypotheses described here are of a "rigidity of size" and "fluidity of size" (Haveman 1993). To the extent that older, larger, and more formal organizations do experiment with changes in core strategy, it is likely that such transitions will tend to be in more conservative directions, as predicted by earlier theories of goal displacement (Zald and Ash 1966). Younger, smaller, and less formalized groups, in contrast, may be more flexible and able to adopt more activist forms of social change as the opportunity or need arises.

There may also be an interaction between environmental conditions and organizational attributes that influences the timing and content of core change in strategy. For example, established organizations may be more likely to make conservative changes when political conditions or resource availability contract. At the same time, these organizations may be more successful at pursuing activist changes, even during restrictive periods. By extension, less established groups (those that are younger, smaller, or more decentralized) may be more inclined to increase activism if political and resource conditions are favorable.

Internal characteristics are also expected to influence the organization's ability to survive the disruptive effects of core changes. For example, more established organizations—those that have been around longer, adopt more institutionalized or familiar operating structures, or have more substantial membership support—may be better able to withstand the potential disruption associated with organizational change activity. Younger, less formalized, and smaller organizations, on the other hand, may need to be cautious about changing their strategies. By implication, decreased activism by less established groups may offset the disruptive effects of change. In general, changes that signal greater institutional conformity on the part of these less legitimate organizations should improve their survival prospects. To summarize:

HYPOTHESIS 4.—Older, larger, and more formal organizations are more likely to change core strategies, especially in conservative directions, whereas younger, smaller, and less formalized groups may be more flexible in adopting activist forms of social change.

HYPOTHESIS 5.—Established organizations are more likely to shift to conservative models when political and resource conditions contract, whereas less established groups are more likely to pursue increased activism when political and resource conditions are favorable.

HYPOTHESIS 6.—Established organizations are more likely to withstand the disruptive effects of change, particularly activist change.

Organizational goals.—Another central attribute, organizational goals, is likely to have an independent effect on the propensity and ability of social movement organizations to pursue alternative strategies or tactics (Dalton 1994). Organizations with a radical orientation that calls for a fundamental restructuring of society are expected to be more prone to inertial pressures compared with organizations espousing reformist goals, since ideological considerations are less mutable than the pragmatic goals of many contemporary “professional” social movement organizations (McCarthy and Zald 1977; Dalton 1994; see also Kitschelt [1989] on the distinction between ideologues, lobbyists, and pragmatists). In particular, radical organizations are likely to resist pressures to adopt associational

strategies such as service provision. This has been discussed more recently in terms of the relationship between goals and the choice of organizational structure (Breines 1980; Staggenborg 1988), but it is also relevant to the choice of strategies (Dalton 1994) and, by extension, the propensity for change. At a minimum, the very fact of considering such changes is likely to engender conflict in more radical movement organizations than in reform-oriented groups that tend to be more centralized and less conflict prone (Staggenborg 1988; Dalton 1994). Again, context matters: organizations with a commitment to radical social change may be less likely to depart from a protest agenda when financial or political support is available widely or grassroots protest is widespread. Likewise, improvements in political and resource conditions may encourage reform-oriented organizations toward greater activism since public sentiment is in their favor. This suggests that radical organizations are less likely to attempt organizational change, especially when the social movement environment is more open, and are more likely to fail if they do.

HYPOTHESIS 7.—*Organizations committed to radical social change are less likely to change strategies, particularly when the social movement environment is expanding, and are more likely to fail if they do.*

Repertoires of flexibility.—A final factor is the role of temporal dynamics such as prior change history and time dependence in change rates. There appears to be a positive “momentum” associated with an organization’s history of change, suggesting that if organizations are seeking alternative models for action, they are most likely to refer to the most recent type of change activity. And the probability this solution will be chosen is highest after the event (Kelly and Amburgey 1991; Delacroix and Swaminathan 1991). Previously flexible organizations have been able to integrate such behavior into their existing “repertory” and may be more capable of seeking out alternative models of action under variable political and resource conditions. Such change propensities are thought to diminish, or decelerate, as time elapses (Amburgey et al. 1993). Prior flexibility may effectively enhance any propensities toward change associated with such characteristics as formalization, ideological commitments, and organizational age. Organizations with some prior change experience may also be more willing and able to seek out alternative modes when political and resource conditions shift, particularly when the context for organizational activities becomes less open. Organizations with a repertoire of flexibility are expected to have an adaptive advantage (Amburgey et al. 1993).

HYPOTHESIS 8.—*Prior flexibility is expected to increase the rate of change at a declining rate.*

HYPOTHESIS 9.—*Prior flexibility is expected to enhance change tendencies associated with organizational goals and structure.*

HYPOTHESIS 10.—*Prior flexibility is expected to increase the rate of change and survival when conditions in the social movement environment contract.*

DATA AND METHODS

The analyses of organizational change and survival employ data collected from the first 23 editions of the *Encyclopedia of Associations* (Gale Research Company 1955–86). The research design includes voluntary associations in the United States that are open to a national membership base and have women, African-Americans, Asian-Americans, and Hispanic-Americans as their constituencies. Their purposes are national in scope, although activities may be conducted locally, and center on gaining equality or promoting equal participation for their constituencies. The *Encyclopedia* provides descriptive information on national voluntary associations, including such background information as the year the organization was formed, its membership, objectives, and activities. The information is self-reported once organizations are located by Gale's research staff or after they request inclusion themselves; entries are updated or revised by the organization for each subsequent edition. Defunct and inactive groups are listed in the index. These two features provide the opportunity to construct event-history records for each group and to study change over time in organizational attributes.

There are some important limitations on the use of the *Encyclopedia* as a data source. Protest groups tend to be underrepresented, although those that play prominent roles in accounts of the civil rights, feminist, and Chicano movements are listed in the directory. For example, the *Encyclopedia* lists better-known organizations such as the Black Panthers, the Black Liberation Army, the Student Nonviolent Coordinating Committee, and the Chicago Women's Liberation Union, as well as less visible organizations that indicate some protest component to their activities, including black organizations, such as the Afro-American Student Association, Charter Group for a Pledge of Conscience, and the National Black United Front, and feminist organizations, such as the Congressional Union and the Freedom Socialist Party. Organizations may also be reluctant to document involvement in more confrontational activities or to overstate their pursuit of more legitimate ones such as advocacy. A more important limitation on this data source for the purposes of the current analysis is that organizations may not accurately update their entries from year to year or there may be a significant lag between when the organization changed its focus and when they note that in their description, both of which would underestimate the timing of change in relationship to environmental conditions. This combination of limitations implies that tran-

sitions to and from the protest form of organization are the most likely to be underestimated, suggesting particular caution in interpreting relevant results.

A final caveat regarding research design is that this study concentrates on organizational activity at the national level. The analyses presented below do not, therefore, address local variation in organizational forms—and thus the range of available choices or “state space” for organizational change. The decentralization of the U.S. state suggests that such a comparative local-level analysis would be informative, although there is no reason to expect symmetry between national and local processes.

Methods

I use the maximum-likelihood event-history methods described by Allison (1982) to analyze organizational change and disbanding. This is a logistic regression strategy that measures the probability of event occurrence (in this case, either strategic change or disbanding) over time. The data structure for this type of analysis requires that one observation is created for each year of the organization's existence. The first observation, or “spell,” for an organization corresponds to its founding year (or the first year of the study, if the association was formed prior to 1955). The last record for an organization corresponds either to the year in which it becomes defunct or to the end of the study (1985). Each organization can have as few as one or as many as 31 observations (which would be the case for groups founded before 1955 and active after 1985).⁶ The total number of spells corresponds to years of activity; change is modeled as a repeatable event, which means that organizations are not censored until they cease activity. Covariant values are updated yearly for each organization. There are 11,369 “organization years” for the 871 groups included in this analysis. The event-history analysis uses the logistic regression procedure in Limdep 7.0 (Greene 1995).

⁶ Since 1975, the *Encyclopedia* has been updated yearly; prior to that time it was published in somewhat inconsistent intervals. In years when the *Encyclopedia* was not published, a spell was created by filling in the missing years either from the preceding or following coded entry. If there were an even number of missing years in an interval, the empty spells were filled in from each of the earlier and later records. For example, to deal with the gap in publication between 1961 and 1964, data for 1962 were taken from the 1961 entry, and data for 1963 from the 1964 record. If there were an uneven number of years in an interval, a random number was drawn to decide which record would be used to fill in the odd year (if the number was less than .50, the earlier entry was used and vice versa). This method was also used when there was no recorded information on an active organization. As a result of employing this technique, there may be some regularity to the data that might not otherwise be present, in that it assumes constancy in such features over spells.

The discrete-time hazard rate can be formally represented as

$$P_{it} = \Pr (T_i = t | T_i \geq t, X_{it}),$$

where T is the discrete random variable giving the uncensored time at which the i th organization changes strategy (or disbands), t is a specific point in time, and X_{it} is a vector of explanatory variables. This gives the conditional probability that the i th organization changes (or disbands) at time t . Allison (1982) suggests using the logistic regression function to specify how the hazard rate depends on time and the explanatory variables:

$$\log [P_{it}/(1 - P_{it})] = a(t) + B'X_{it}.$$

This model specifies that variations in the risk of change (or disbanding) can occur autonomously with time (parameters $a(t)$), and may also be associated with the explanatory variables X (parameters in coefficient vector B). The method allows information about the organization to be updated yearly and to take this into account when examining the factors associated with the risk of change or disbanding.

Dependent Variables

Core change in strategy.—Core change in organizational strategy is measured as a dummy variable coded “1” if the organization changed its dominant strategy between the prior year and the current year. In addition to a general measure of change in strategy from year to year, I construct measures of whether or not the organization has made a transition between the three dominant strategies that represent the “state space” for organizational transition: protest, advocacy, and either service provision or cultural action.⁷ A change is recorded as having taken place if the strategy variable for the current year is different from that in the prior year. Whereas origin strategy was coded by the author and determined subjectively from the organization’s self-reported description in the *Encyclopedia*, the change variable was computed by comparing the prior and current strategy variable and returning a value of 1 if they were not the same. The reliability of this measure therefore rests on the original coding of organizational strategy and does not involve any evaluation of the extent of change.⁸

⁷ In exploratory analyses, I examined transitions to and from the cultural form. The results were substantially similar to those to and from the service form when analyzed separately and to the results reported here (available from the author).

⁸ To improve the reliability of this measure, I first recoded strategy from a possibility of 12 categories (original codes are available from the author) into mutually exclusive categories of: (1) any protest activity (singly or in combination with advocacy, service, or cultural activities); (2) any advocacy activity, including combined advocacy and service, and (3) any service activities without advocacy and cultural activities. Of

Strategic Change and Adaptation

TABLE 1

FREQUENCY OF CORE STRATEGIC CHANGES BY NATIONAL WOMEN'S
AND RACIAL-ETHNIC ORGANIZATIONS, 1955-85

ORIGINAL STRATEGY	NEW STRATEGY		
	Protest	Advocacy	Service
Protest		7	0
Advocacy	4		45
Service	0	42	

NOTE—No. of transitions = 98, no. of organizations = 69, base population size = 870.

Table 1 depicts the kinds and number of changes. Overall, national women's and racial minority organizations experience 98 transitions in organizational strategy during 1955-85. These changes are carried out by 69 of 870 organizations.⁹ This suggests that such changes are relatively rare among these social movement and movement-affiliated organizations. In terms of the specific kinds of changes, transitions between the advocacy and service forms of organization are the most typical, with 45 changes from advocacy to service or cultural strategies and 42 transitions in the opposite direction. There are also 7 observed changes from protest to advocacy forms of organization and only 4 moves from advocacy to protest. There are no observed transitions between the protest and service forms over this period of analysis, suggesting that these two methods of

particular importance was the decision to constrain combined advocacy and service organizations to the advocacy category rather than treating this hybrid form as a distinct state space for organizational change. There appears to be some inconsistency in the coding of whether the organization primarily does advocacy or service or a combination of both, which generates the appearance of a great deal of change to and from the hybrid form over the organizational life course. It is unclear whether the high number of calculated changes represents actual transitions or is an artifact of coder inconsistency, and it seems more prudent to assume the latter. My logic for treating these organizations as advocacy rather than service is similar to that employed in combining service and cultural groups, that is, based on the level of institutional challenge and vulnerability implied by the dominant activities. In this respect, I would argue that the advocacy component is determinant.

⁹ One additional organization was eliminated from the sample because it represented an outlier with respect to the number of changes undertaken. This organization was coded as having made a total of eight changes between advocacy and service forms of activity over the course of the study period (four in each direction). The atypical nature of the case suggests that the original coding of this organization was not reliable. In addition, excluding this case also strengthens confidence in the results obtained from the analysis of interaction effects, which are particularly sensitive to outliers.

social change represent noncontiguous options for women and racial minorities. The boundaries between what I have referred to as an associational form of organization, comprising service and cultural activities, and the advocacy form, consisting of more institutionally legitimate forms of lobbying and reform, however, seem to be relatively permeable.

Organizational disbanding.—The dependent variable in the failure-rate analysis is whether or not an organization ceased activity in the prior year. This variable is coded “1” once the organization is listed in the *Encyclopedia* as defunct, inactive (and it did not resume activity by the end of the study), “address unknown” (listed in the past, but requests for updated information were unanswered for three editions or returned by the Post Office), or “lost” (coded in one or more editions, but do not appear subsequently). The date of dissolution is either the year reported in the *Encyclopedia* or the last appearance of the organization in the directory. A total of 213 organizations (of 870) ceased activity over the study period.

Independent Variables

Environmental conditions.—The three central concepts that I measure are political opportunities, protest dynamics, and resource availability. Political conditions are indexed by a variable coded “1” if the incumbent presidential administration is Democratic, which Walker (1991) found to be an important factor in shaping the activities of citizens groups (lagged one year). Protest activity is a combined measure of civil rights and feminist protest in the prior year.¹⁰ I construct linear and quadratic terms to specify the cyclical nature of protest (quadratic term divided by 100 to reduce the number of significant digits). Resource availability is measured by the yearly level of combined foundation and corporate giving to non-profit organizations in the United States (lagged one year; 1982 constant dollars, in billions; AAFRC 1988).

Organizational characteristics.—Organizational age is calculated based on the reported year of organizational founding. I use the natural logarithm of age in the transition-rate analysis (Amburgey et al. 1993) and a quadratic specification in the failure-rate analysis (Minkoff 1993). In the failure-rate analysis, I also include a dummy variable indicating if an organization was formed prior to the 1955 start of the study, which was not

¹⁰ Craig Jenkins provided the black-protest-event data and Rachel Rosenfeld and Kathryn Ward provided feminist-protest-event data. Each series was coded using the *New York Times Annual Index* and includes both contentious action (demonstrations, boycotts, sit-ins) and conventional events (petitions, registration campaigns) initiated by each movement. See Jenkins and Ekert (1987) and Rosenfeld and Ward (1996) for fuller descriptions of each series.

significant in the transition-rate analysis.¹¹ I also control for number of members (logged value); when this information is missing, I assign a value of 1 to the organization, which may overestimate the liability of smallness. Number of paid staff represents a measure of organizational formalization and professionalization (McCarthy and Zald 1973, 1977). Goals are indexed by a dummy variable coded "1" if the organization espouses radical social-structural change and "0" if it is oriented to reform or integration.¹² I also include a dummy variable coded "1" if the organization represents women's interests in contrast to African-American, Asian-American, or Hispanic-American interests.

Following other studies (esp. Amburgey et al. 1993; Delacroix and Swaminathan 1991), I have constructed two variables to capture profiles of flexibility. The first is a "duration clock" variable that measures the time elapsed since the last change. This variable is initially set to zero until an event occurs, at which point it records the elapsed time since the change (in years). If another change occurs, the clock is reset to zero and then again records the elapsed time. The second variable measures the cumulative number of changes, which has a value of zero until the first change, the value of one until the second change, and so on. In contrast to other research using such measures, these variables index general change histories rather than cumulative number of specific changes of the type being modeled or time elapsed since that specific type of change. Given that changes in strategy are relatively infrequent within this population, and given the high costs associated with making such core changes in dominant social change strategies, such type-specific change variables are less informative than general indicators of an organization's adaptability. Ex-

¹¹ The data used in this study are left censored in 1955. Of the 870 organizations considered in this analysis, 95 were active in 1955 (ranging in age from 33 to 97). This leads to two potential problems: The first is sample selection bias, which may occur when the sample is chosen on the basis of an endogenous variable such as survival, and there are systematic differences between the disbanding rates of groups formed before and after the start of the study that cannot be measured. This is likely if the survival of the early set of organizations is due to attributes that distinguishes them in unknown ways from those formed later. The second issue is that information on organizational changes is not available before 1955, and the previous histories of left-censored groups must be ignored in estimating the correlates of organizational change. I follow Baum and Singh (1996) in controlling for whether the organization has left-censored spells. In the transition-rate analyses, this variable was not significant (nor were interaction effects between left-censored spells and any other independent variables) and is omitted.

¹² A handful of organizations that have conservative goals, in the sense that they perceive maintenance of the status quo or a return to previous forms of social relations to be in the constituency's interest, are excluded from the current analysis in order to keep questions of movement-countermove dynamics separate (see Meyer and Staggborg 1996).

ploratory analysis confirmed that type-specific measures of this sort were not significant predictors of specific changes. In the failure-rate analysis, I tested the effect of two measures of change that were not relevant in the analysis of organizational change. The first is a dummy variable indicating whether or not a change occurred, taking a value of zero until the occurrence of a change and the value of one afterward, regardless of the number of changes. The second variable measures the disruptive effect of the first change, coded "1" in the year when a change was indicated and "0" for all other years of activity. This measure tests whether changes in an organization's founding rationale are more difficult or disruptive. Table A1 in the appendix provides descriptive information on the dependent and independent variables employed in the analyses presented below.

THE CORRELATES OF ORGANIZATIONAL CHANGE

Table 2 provides a summary of the central hypotheses and results that guide the analyses presented in this and the following two sections. The first set of issues that I address relates to the correlates and content of organizational change. Table 3 presents logistic regression estimates of the likelihood of organizational change. The first column presents a model of general change in strategy, and subsequent columns specify the correlates of specific types of organizational change (i.e., from protest to advocacy and vice versa or from advocacy to service or vice versa). The independent variables include measures of the political and resource environment, organization-level attributes, and indicators of the organization's prior change behavior. I also report significant interaction effects between organizational and environmental variables. Since my primary interest revolves around how effects vary for distinct types of transitions, the discussion of the results centers on comparing the influence of independent variables across each column or model.

The first questions are how do changes in the political and resource environment influence the rate of organizational change among social movement and affiliated organizations, and do external pressures promote conformity or activism in social change strategies? Hypothesis 1 posits a direct relationship between open political opportunities, developing protest cycles, and increased resource availability and the rate of change, especially more activist-oriented shifts. There is mixed support for this hypothesis. Incumbency of a Democratic presidential administration and increases in foundation funding have positive influences on the rate of change in certain instances. The results reported in model 3 significantly indicate a somewhat higher likelihood of organizational change from advocacy to protest strategies during years of Democratic presidency, but this variable is not significant in the other models. Improvements in foun-

TABLE 2

SUMMARY OF HYPOTHESES AND RESULTS

Summary of Hypotheses	Summary of Results
1. Open SM environments increase rate of activist change; closed SM environments increase rate of conservative change.	Mixed support: Democratic administration increases advocacy \rightarrow protest; funding or increases change service \rightarrow advocacy; protest cycles diminish rate of change. Not supported.
2. Activist change increases risk of failure; conformist change decreases risk of failure.	Partial support: funding \times flexibility increases failure rate.
3. Activist change in open SM environment decreases risk of failure; conservative change in open SM environment not associated with failure rate.	Partial support: fluidity of aging and formalization, regardless of direction.
4. Established organizations more likely to change, especially in conservative directions, less established organizations more likely to make activist changes.	Not supported.
5. Established organizations more likely to make conservative changes when SM environment contracts; less established organizations more likely to make activist changes when SM environment improves.	Not supported.
6. Established organizations more likely to survive disruptive effects of change, particularly activist change.	Not supported. radical goals \times flexibility increases change; no significant interaction between goals, flexibility, and failure. Supported.
7. Radical organizations less likely to change strategies, especially when SM environment expands, and more likely to fail if they do.	Not supported: age \times flexibility decreases change; age \times goals increases change.
8. Prior flexibility increases rate of change at a declining rate.	Mixed support: funding \times flexibility decreases change; protest \times flexibility increases change.
9. Prior flexibility enhances change tendencies associated with goals and structure.	
10. Prior flexibility increases rate of change and survival when conditions in SM environment contract.	

NOTE —SM = social movement

TABLE 3
DETERMINANTS OF CHANGES IN CORE STRATEGY

	TYPE OF CHANGE				
	Any change (1)	Protest → Advocacy (2)	Advocacy → Protest (3)	Advocacy → Service (4)	Service → Advocacy (5)
Democratic president ($t - 1$)	-.011 (.036)	1.063 (.908)	2.260* (1.281)	.065 ^{s-2} (.187 ^{s-1})	.004 (.019)
Foundation funding ($t - 1$)497** (.207)	-.194 (.388)	.119 (.800)	.227 (.186)	.371* (.217)
Protest activity ($t - 1$)679 ^{s-2} (.639 ^{s-2})	- 320 ^{s-2} (.514 ^{s-2})	- 333 ^{s-2} (.611 ^{s-2})	- .767 ^{s-2} ** (.375 ^{s-2})	- .608 ^{s-2} ** (.329 ^{s-2})
Protest × protest	-.484 ^{s-2} ** (.213 ^{s-2})
Age (log)615*** (.164)	.407 (.536)	-.101 (.664)	.428*** (.193)	.479** (.201)
Membership size (log)038 (.040)	.140 (.119)	.134 (.147)	.069 (.048)	.067 (.051)
No. of paid staff167 ^{s-2} *** (.062 ^{s-2})	.094 ^{s-2} (.540 ^{s-2})	.070 ^{s-2} (.540 ^{s-2})	.177 ^{s-2} *** (.062 ^{s-2})	.167 ^{s-2} *** (.064 ^{s-2})
Radical change goals	-.573 (.994)	1.590 (1.172)	2.059 (1.329)	.624 (.623)	.294 (.752)

Feminist organization084 (.294)	-.084 (.947)	-1.004 (1.326)	.051 (.360)	.514 (.373)
No. of prior changes	20.362*** (2.244)	6.552*** (2.001)	2.105*** (.477)	5.445*** (.721)	5.645*** (.739)
Years since last change	-2.483*** (.462)	-.169 (.407)	-532 (.531)	-344* (.191)	-.589** (.234)
Age × prior changes	-2.533*** (.275)	-1.728** (.768)	-1.649** (.749)	-1.379*** (.259)	-1.177*** (.244)
Radical goals × prior changes	1.807** (.995)
Prior changes × funding	-1.250*** (.252)
Prior changes × protest760*** (.342*)617*** (.224*)	...
Constant	-10.690*** (1.554)	-8.886*** (3.048)	-10.725* (5.692)	-8.316*** (1.380)	-9.814*** (1.581)
Log likelihood	-295.427	-39.530	-23.323	-198.269	-178.394
N events	98	7	4	45	42
N spells	11,345	11,345	11,345	11,345	11,345

Note.—Data represent logistic regression results, SEs are in parentheses

* $P < .10$
 ** $P < .05$
 *** $P < .01$

dation sponsorship for nonprofit activity promote the rate of change, both with respect to general changes in core strategy and transitions from service to advocacy activities. These results provide some confirmation of the view that improvements in the political opportunity structure and the funding environment provide a context within which movement-affiliated organizations are able to adopt more direct forms of institutional action. Although the evidence is indirect, these findings question the established wisdom that foundation patronage necessarily channels social movement activity and voluntary organizations into more moderate directions—at least in terms of the process of organizational change.

The relationship between increases in protest activity and organizational change contradicts theoretical expectations. Tarrow (1994) has argued that at the peak of protest cycles, more moderate groups tend to adopt more radical forms of action as they compete for attention and support. By extension, I have suggested that upswings in protest cycles present a more generalized opportunity for experimenting with more activist organizational identities, an effect which is likely to decline as the cycle diminishes in intensity. With respect to the general process of change, it appears that protest activity may marginally promote organizational change early in the cycle but that this positive effect is offset by later increases. In model 1, the linear term is positive but not statistically significant and the quadratic effect is negative and significant; I report estimates for both coefficients because the inclusion of the squared term significantly improves the fit of the model even though the main effect becomes insignificant. Calculating the point of inflection ($bL/-2bQ$), this decline in the facilitative influence of protest takes place when the number of protests reaches 136, which occurs within the first years of the study period. However, there is no significant association between protest events and changes between advocacy and protest organizational forms. And, in the case of transitions between the advocacy and service forms of organization (models 4 and 5), increases in protest activity appear to depress the rate of organizational change (quadratic terms omitted because they did not improve the fit). While a decrease in the rate of change from advocacy to service was expected, it is also the case that organizations committed to service or cultural action do not take advantage of the opportunity provided by widespread protest to move into the advocacy model. One interpretation of these results is that short-term political turbulence makes organizers less inclined to risk the disruptive effects of organizational change, even if the political context seems conducive.

The second set of questions revolves around what organizational characteristics promote or inhibit flexibility in the face of changing circumstances and what specific types of change they influence. Hypothesis 4 predicts that more established organizations—those that are older, larger,

and more formalized—are more likely to change in conservative directions, whereas younger, smaller, and less formal groups are expected to seek out more activist-oriented alternatives. Implicit in this hypothesis is the view that there is a “fluidity” associated with such features as age, size, and formalization, but that flexibility also depends on the type of change being undertaken. There is strong support for a view of movement organizations becoming more flexible as they become more established but no indication that change is necessarily conservative. In line with a fluidity of aging hypothesis, older organizations are better able to respond to environmental shifts and pressures for change from within the organization (Kelly and Amburgey 1991). This effect is most pronounced with respect to transitions between the advocacy and service organizational forms and as a predictor of general strategic change. There is a similar pattern of results regarding organizational formalization, indexed by the number of paid staff. Staff-centered organizations are more likely to experiment with core changes, particularly to and from the advocacy and service forms. Organizational age, size, and formalization are not, however, significant factors predicting transitions between protest and advocacy. There is also no indication that organizations with larger memberships have significantly higher rates of organizational change. Overall, flexibility is associated with older, more professionalized organizations. This contrasts with the view that growth and bureaucratization promotes organizational inertia or that change is necessarily conservative.

Over and above structural features, I have argued that organizational goals are especially relevant for understanding the change propensities of social movement organizations. The first part of hypothesis 7 posits that organizations with radical change goals are less likely to engage in core strategic changes—especially more conservative ones—because of their stronger ideological commitment. There is no evidence to support this argument. In fact, under certain circumstances the opposite may be the case, as I discuss below.

Hypotheses 8–10 emphasize the relationship between “repertoires of flexibility” and the propensity for organizational change. In each model presented in table 3, increases in the number of prior changes in strategy significantly increase the subsequent rate of change, which suggests that there is a positive momentum associated with organizational adaptation (Kelly and Amburgey 1991). This effect, however, is time dependent in the sense that the probability of change decreases over time, a result that is significant in the model of general change and in the models of shifts between service and advocacy forms. In general terms, these results confirm hypothesis 8 and suggest that social movement organizations are similar to the other for-profit and nonprofit organizations studied extensively (Barnett and Carroll 1995).

Hypothesis 9 predicts that prior flexibility should enhance the change tendencies associated with organizational goals and structure. This hypothesis receives only partial support. Regardless of the content of change, there is a significant interaction between age and prior change history measured by number of prior transitions. Although the direct effects of each of these variables are positive, the interaction effect is negative. This suggests that while aging and greater experience with change increase flexibility, core transitions that take place early in an organization's history "decelerate" the momentum for future change behavior (Amburgey et al. 1993). Timing, then, is critical: if younger organizations experiment with new strategies, they lose the advantages associated with aging. It also appears that, at least with respect to the model of general change, the probability of core change is higher for radical organizations that already have some prior history of change. This finding challenges the presumption that radical orientations necessarily diminish flexibility; rather it appears that organizations with such goals may be more intent on taking advantage of opportunities for innovation in order to maximize their objectives, assuming that they have a repertoire of change to draw upon.

To examine how prior change experience situates organizations to take advantage of shifts in the external environment, I also tested interaction effects between the number of prior transitions and environmental conditions. The first part of hypothesis 10 predicts that prior flexibility will increase the rate of change when conditions in the social movement environment contract. Support for this hypothesis is mixed. The positive momentum associated with prior flexibility diminishes when funding levels increase, offsetting the direct and positive influence of resource availability on general change, and shifts from service to advocacy. Having already positioned themselves differently vis-à-vis the environment in the past, these organizations are less likely to make subsequent alterations in their dominant activities, even when funding opportunities improve. In contrast, prior flexibility proves to be an asset with respect to periods of intensified protest: there is a positive interaction effect between the number of prior changes and the number of protest events. This effect is significant in the model of general change as well as the model of the rate of change from advocacy to service activities. It appears that only those social movement organizations that can draw on a repertoire of flexibility are able to capitalize on periods of intensified protest, offsetting the constraining effect of protest on the rate of change described earlier. It is unclear, however, why this effect is particularly significant for the process of changing from advocacy to service, which represents shifting toward a more conventional strategy when extrainstitutional protest intensifies.

THE CONSEQUENCES OF ORGANIZATIONAL CHANGE

The final set of questions examines the relationship between organizational change and survival: (1) Are core changes so disruptive that they increase the risk of failure, and do conservative changes offset any liabilities associated with organizational transformation? (2) Does the timing of these changes or the kinds of organizations attempting them make a difference as well? Table 4 provides logistic regression estimates of the failure rate of women's and racial minority organizations, controlling for similar variables as presented in the analysis of organizational change (with the addition of organizational strategy and whether or not the organization has left-censored spells). The dependent variable—whether the organization disbanded between the prior and current years of observation—is the same in each model, but I vary the measures indexing the effect of organizational change. Models 1, 4, and 5 include a measure of any experience with change, and models 2 and 3 include a measure indexing the impact of first change. My focus here is on comparing the effects of the first three variables reported in table 4 for each model.

The results presented in table 4 demonstrate that core change in strategy is critically disruptive for social movement organizations. Model 1 shows that organizations that have had any experience with core change in strategy are significantly more likely to disband than those that maintain a consistent organizational identity. As predicted by a structural inertia perspective, core changes effectively “reset the liability of newness clock,” although in time the failure rate diminishes. Hypothesis 2 predicted that the content of change would situate organizations differently in this respect, namely that activist changes would increase, and conservative changes decrease, the risk of failure. The data do not support this view. Models 4 and 5 indicate that organizations with any experience of change from advocacy to service or vice versa have higher failure rates, and although the duration-clock variable indicates a declining effect, this measure is not statistically significant.

The relationship between change and survival is somewhat distinct when considering the impact of shifts between the advocacy and protest forms of organization. Models 2 and 3 control for the impact of first change, which exploratory analysis indicated was a better predictor of the failure rate in these cases compared with the measure of any change experience. The first time an organization makes a change from either protest to advocacy or advocacy to protest significantly increases the risk of failure. Time elapsed since first change was not significant in either of these models, which suggests that the impact of change stays with these organizations as they make an effort to continue their activities with a

TABLE 4
CHANGES IN CORE STRATEGY AND ORGANIZATIONAL DISBANDING

	TYPE OF CHANGE				
	Any change (1)	Protest → Advocacy (2)	Advocacy → Protest (3)	Advocacy → Service (4)	Service → Advocacy (5)
Any experience with change	5.429** (2.309)	1.284** (.588)	7.021** (3.105)
Years since last change	-.348* (.185)	-.152 (.155)	-.894 (.581)
Impact of first change	2.411** (1.111)	3.133** (1.296)
Funding × change experience	-.608* (.345)	-.756* (.450)
Foundation funding ($t - 1$)172** (.087)	.155* (.085)	.152* (.085)	.150* (.085)	.167** (.086)
Democratic president ($t - 1$)076 (.164)	.069 (.164)	.063 (.164)	.080 (.164)	.081 (.164)
Protest cycle ($t - 1$)	-.130* ^{2*} (.082* ²)	-.133* ^{2*} (.081* ²)	-.130* ^{2*} (.081* ²)	-.130* ^{2*} (.081* ²)	-.136* ^{2*} (.081* ²)
Age067*** (.018)	.068*** (.018)	.068*** (.018)	.066*** (.018)	.069*** (.018)

Age × age	-.105 ^{e-2} *** (.032 ^{e-2})	-.107 ^{e-2} *** (.032 ^{e-2})	-.105 ^{e-2} *** (.032 ^{e-2})	-.107 ^{e-2} *** (.032 ^{e-2})
Founded pre-1955 ..	-.652* (.378)	-.656* (.376)	-.623* (.376)	-.666* (.376)
Membership size (log)	-.122*** (.022)	-.121*** (.022)	-.122*** (.022)	-.121*** (.022)
No. of paid staff082 ^{e-2} (.097 ^{e-2})	.097 ^{e-2} (.100 ^{e-2})	.083 ^{e-2} (.098 ^{e-2})	.092 ^{e-2} (.099 ^{e-2})
Radical change goals751*** (.220)	.741*** (.221)	.736*** (.221)	.742*** (.221)
Feminist organization	-.024 (.148)	-.022 (.148)	-.019 (.148)	-.032 (.148)
Protest (vs. service) ..	.395 (.338)	.291 (.351)	.423 (.340)	.405 (.339)
Advocacy (vs. service) ..	-.060 (.161)	-.076 (.160)	-.055 (.161)	-.069 (.161)
Constant	-4.975*** (.593)	-4.844*** (.577)	-4.828*** (.578)	-4.931*** (.584)
Log likelihood	-1,006.175	-1,008.379	-1,007.882	-1,006.081
N events	213	213	213	213
N spells	11,345	11,345	11,345	11,345

NOTE.—Data represent logistic regression results; SEs are in parentheses.

* $P < .10$

** $P < .05$

*** $P < .01$

new core identity. The impact of initial change between the advocacy and service forms of organization was also not significant (results available from author). These results suggest an important qualification to hypothesis 2, which predicted that changes in the direction of greater activism increase the risk of organizational failure and that transitions that signal greater institutional conformity decrease the risk of disbanding. There is no indication that what I have referred to as conservative changes, for example, from advocacy to service or protest to advocacy, diminish the risk of failure. All organizations, regardless of whether the changes they make signal greater institutional conformity or challenge, appear to undermine their survival prospects as a result. Changes within a narrower range of political action, from protest to advocacy or advocacy to protest, however, do seem to be much more critical.

Hypotheses 3, 6, and 7 contained predictions regarding interaction effects between organizational attributes, external conditions, and the consequences of different types of organizational change. There was only one significant interaction effect between change experience and environmental conditions. Models 1 and 5 demonstrate that those organizations that have a history of any kind of strategic change, in particular changes from service to advocacy, are significantly less likely to fail when resources become more widely available. One plausible interpretation of this result is that funders are more inclined to sponsor what they perceive to be more flexible organizations, especially given the high level of competition for such funds. Overall, the expected relationships between the institutional standing of different organizations, the nature and timing of the kinds of changes they were attempting, and organizational survival were not borne out by the data. Nor did organizational goals situate organizations differently with respect to weathering core changes in strategy.

INTEGRATING THE ANALYSIS OF SOCIAL MOVEMENT ORGANIZATION CHANGE AND SURVIVAL

The results presented in tables 3 and 4 provide an opportunity to compare how organization-level and environment-level factors influence the processes of organizational change and survival. Such a comparison provides important insight into organizational dynamics and the growth of SMO populations more generally, providing the basis for an integrated model of social movement organizational change and development (Zald and Ash 1966). Table 5 summarizes the central results that form the basis of this comparison.

The dilemmas faced by activist groups as they attempt to negotiate variable political and social conditions become clear when comparing the correlates of organizational change and failure. Although institutionally

Strategic Change and Adaptation

TABLE 5

INTEGRATED COMPARISON OF THE PROCESSES OF ORGANIZATIONAL CHANGE AND FAILURE: SUMMARY RESULTS

	Change	Failure
Social movement environment:		
Democratic administration	Positive (advocacy → protest)	Not significant
Funding	Not significant	Positive
Protest cycle	Negative	Negative
Internal features:		
Age	Positive	Positive linear; negative quadratic
Membership size	Not significant	Negative
Staff size	Positive	Not significant
Radical change orientation	Not significant	Positive
Prior flexibility	Positive	Positive

structured political opportunities have been found to be a critical feature of social movement emergence and development, shifts in established political support have a minimal effect on the rate of organizational change or the survival chances of women's and racial minority organizations. Periods of Democratic control of the presidency do increase the likelihood of moving from advocacy to protest but have no apparent influence on the organizational survival rate. Heightened protest activity, in contrast, tends to diminish the rate of change, while at the same time providing a more general buffer against failure independent of an organization's level of flexibility. Protest cycles are, in this sense, a source of positive political opportunity for movement organizations, but grassroots political turbulence is not conducive to experimenting with strategic change—unless an organization is already equipped with a “repertoire of flexibility.”

The influence of funding trends is more complicated. Although increases in resource availability promote the process of organizational change, organizations with greater flexibility in the past are significantly less likely to pursue subsequent alterations in their core identity, even when funds become more widely available. Paradoxically, improvements in the funding environment also promote the rate of organizational failure. I have suggested elsewhere that this counterintuitive relationship between funding levels and the failure rate might indicate that when foundation funding is more available to address constituency concerns, it becomes harder to sustain more formally organized independent efforts at social change (Minkoff 1995). In the context of the current analysis, organizations are in a no-win situation: on the one hand, improvements in resource

availability are clearly essential for altering core strategies, but similar conditions make it more difficult for organizations to survive, exacerbating the disruptive effects of organizational change. The finding that organizations with prior change experience are significantly less likely to fail when funding conditions improve takes on added relevance: the implication is that it is worth investing organizational resources in flexibility to buffer against resource uncertainty—even though change increases the risks of failure.

With respect to internal characteristics, organizational age is particularly critical for processes of organizational change and maintenance. Table 4 demonstrates that women's and racial minority organizations experience a significant age-dependence in survival. In an earlier work (Minkoff 1993), I found that these organizations were exposed to higher risks of failure through their eighteenth year of activity, at which point the liability of "adolescence" diminished. Given that organizations are more likely to explore changes in strategy as they age, this might exacerbate the risk of failure for many younger groups. Although there was no significant interaction effect between age and change experience with respect to the failure rate, this comparative interpretation is plausible and merits further examination.

Characteristics associated with more established or legitimate organizations—particularly age and formalization—more generally are correlated with flexibility and survival. In fact, investing in the development of a professional staff might provide one important resource that enhances flexibility without exposing the organization to significant risk of failure. At the same time, although larger membership does not seem to influence organizational flexibility, this feature does improve the survival prospects of national movement organizations. And, although I expected goal orientation to be correlated with rigidity, organizations with radical social change objectives and a history of change appear to have greater flexibility with respect to the general propensity to change core strategies. These organizations are, however, significantly more likely to fail than those that express a reform orientation.

CONCLUSION

The process of organizational change is determined by a complicated interaction of organizational and environmental factors, which situates organizations differently with respect to their ability to bend with the wind. The dilemma facing social movement organizations as they operate in rapidly changing social and political environments cannot be overstated. Not only do they face contradictory pressures from the outside, but the decision to alter their core identity places them at greater risk of failure.

To some extent, organizers may be able to institute organizational structures that enable them to weather external discontinuities, but such investments in organizational maintenance are themselves likely to be contentious to the extent that they conflict with the political goals that motivate these organizations to begin with.

What must be stressed is that the payoffs of flexibility are extremely limited. Women's and racial minority organizations that have a history of core strategic change are more likely to fail than those that remain committed to their original identity. The disruptive effects of core change diminish somewhat over time but not before reexposing organizations to the kinds of liabilities that confront newly established groups as they seek out resources and legitimacy (Stinchcombe 1965). There is no evidence of a straightforward connection between tenure or formalization and conservative forms of organizational change. Nor is there any evidence that external funding channels already established organizations into more conventional activities, although such resources tend to be targeted at more mainstream social and political organizations to begin with. These findings provide empirical support for Zald and Ash's (1966) conclusion that institutionalization and bureaucratization are not necessarily correlated with increased conservatism.

It is also fairly clear that core changes that signal greater conformity to more established—and less institutionally confrontational—models of organizational action do not provide significant survival advantages to movement organizations. For example, advocacy organizations that move onto the terrain of service provision are as likely to fail as those that change from a primary commitment to service provision toward advocacy. Nonetheless, there is some indication that changes in the original charter of groups moving between protest and advocacy or vice versa are distinctively disruptive. As the boundaries between protest and advocacy have become increasingly blurred since the 1960s cycle of protest, and both forms of action have become more institutionalized (McCarthy, McPhail, and Smith 1996; Minkoff 1994), organizations that express more direct confrontation appear to be subject to shared dynamics that differentiate them from other kinds of movement-affiliated groups.

The consequences of organizational change go beyond the continued viability of individual organizations. The nature of these processes also have implications for the development of organizational populations and, in this case, the national social movement sector (Garner and Zald 1987). Although this research documents that the dynamics of organizational change are not necessarily conservative, it is also the case that organizations with more legitimate, institutionally acceptable profiles are advantaged in terms of their ability to adapt to changing environments. Through both processes of adaptation and selection (Hannan and Freeman 1984),

these older, more professionalized movement organizations begin to dominate, thereby altering the distribution of organizational forms at the national level and building the legitimacy of the organizational system. It therefore becomes more difficult for younger, smaller, and more decentralized organizations—qualities often associated with more challenging protest groups—to establish a viable national presence.

Here, though, is the paradox: the most promising circumstances for promoting the continued development of the national social movement sector come at the peak of protest cycles, but not only are such cyclical surges limited in duration, they also place limits on the prospects for organizational change except for the minority of organizations that have a repertoire of prior flexibility. Protest cycles require an organizational infrastructure in order to diffuse activism across diverse social groups (Minkoff 1997). In the context of ongoing debates about the efficacy of the professionalization and institutionalization of social movements for more spontaneous grassroots mobilization (Piven and Cloward 1977), the question remains open whether the organizational sector that is being shaped by the processes of organizational adaptation and survival documented here is capable of capitalizing on such transitory moments of political opportunity, thereby remaining a viable avenue for continued social and political participation. Future research needs to look more closely at the strategic composition of social movement organizational populations with an eye toward the readiness of movements to support grassroots mobilization. It is also necessary to document similar organizational ecologies for a broader array of constituencies and concerns: Women's and racial minority organizations may be distinctive in their traditions of maintaining a diversity of organized options for social change, providing avenues for transformation among already established models as the need arises.

The approach taken in this article presents some basic challenges to thinking about social movement organizing and institutionalizing. As a first point, it is necessary to underscore the continued relevance of organizational analysis for the study of social movements—an insight that guided Zald and Ash (1966) over three decades ago and informs the resource-mobilization approach more generally (McCarthy and Zald 1973, 1977). As Perrow (1992) has argued forcefully, the formal organization is the fundamental form of social organization in contemporary society. In a variety of guises, formal organizations have become increasingly necessary vehicles for any kind of sustained social behavior, and their instantiation in the legal order sustains that fact. One result is that national social movement organizations are central actors in the political process, particularly in societies such as the United States that have an associational or pluralist culture (Minkoff 1997). Because some degree of organizing is necessary, constituencies that cannot maintain formal orga-

nizations have fewer options for collective dissent, and the failure of the type of organizations being studied here means that a portion of the public is essentially "devoiced," if not technically disenfranchised. Although the turn away from organizational analysis in the past decade—with greater attention to collective identity, cultural framing, and political opportunities—has deepened our understanding of movement processes, the fact remains that understanding how political and social contexts shape the availability of organizational models and their development over time requires a more sophisticated approach to movement organizations and their relationships to the environment.

A further implication is that it is specifically a macro-organizational perspective that represents the most promising approach for studying the development of social movements and movement organizations. A long tradition of case-study research on organizational change has clearly informed the current analysis, but without the analytic leverage provided by ecological and institutional approaches to organizational populations, it would not be possible to draw conclusions regarding the more general environmental and organizational dynamics that set the context within which individual organizations operate. Returning to the two illustrations with which I began this article, it is clear that although specific organizations may be able to successfully "bend with the wind" and alter their dominant identities, the rise and fall of movement organizations and movements themselves hinge on external contingencies. And, it is the aggregate processes of change and survival that shape the organizational sector that is so essential for the sustained presence of national social movements in the United States.

These issues are of more than passing theoretical interest. Given the central role played by social movement and voluntary organizations in the U.S. public and political arenas (what some would refer to as civil society and the state), their ability to successfully negotiate environmental transformations is also of critical substantive importance. This point is made most forcefully by revisiting the history of African-Americans in recent decades. Despite the formal gains of the Civil Rights movement, and the somewhat improved socioeconomic status of the black middle class, on most indicators, the quality of life for African-Americans in the United States worsened in the 1970s and 1980s. As well, political conditions have mitigated against the mobilization of a unified voice for racial equality (see Jaynes and Williams 1989). In this context, the ability to maintain established organizations is essential since they represent an important infrastructure for civil rights activism (McAdam 1982; Morris 1984) and the diffusion of protest potential to a broad array of challenging groups (McAdam 1995; Minkoff 1997). However, it may also be necessary to adapt organizational objectives in order to maintain a national presence

over time. The ability to do so is clearly circumscribed in ways that highlight the organizational dynamics and dilemmas that require attention by social movement activists, over and above the political ones that compel them to mobilize to begin with.

APPENDIX

TABLE A1
DESCRIPTIVE STATISTICS

	Minimum	Maximum	Mean	SD
Event-history data (spells):				
Age	1	128	15.10	19.47
Age (log)	-.69	4.85	2.35	.843
No. members (in thousands)	1	4,000	20.55	185.16
Members (log)	0	15.20	4.00	3.55
No. paid staff	0	2,000	10.24	76.34
Feminist organization (yes = 1)	0	1	.42	.49
Radical organization (yes = 1)	0	1	.063	.24
No. of prior changes	0	4	.044	.27
"Clock"	0	26	.15	1.11
Any change	0	1	.03	.18
Left censored	0	1	.24	.43
Democratic administration ($t - 1$)	0	1	.34	.47
No. protests ($t - 1$)	35	664	126.94	129.04
Funding ($t - 1$)	2.64	7.78	6.27	1.12
Organization-level data:*				
Age	1	128	14.99	16.16
Age (log)	-.69	4.85	2.35	.843
Members (in thousands)	1	4,000	14.45	159.05
Members (log)	0	15.20	4.00	3.55
Staff	0	2,000	8.90	80.96
Feminist organization	0	1	.45	.50
Radical organization	0	1	.07	.26
No. of prior changes	0	4	.11	.42
"Clock"	0	26	.32	1.64
Any change	0	1	.08	.27
Left censored	0	1	.11	.31
Social movement environment:				
Democratic administration ($t - 1$)	0	1	.40	.50
No. protests ($t - 1$)	35	664	188.67	171.91
Funding ($t - 1$)	2.64	7.78	5.60	1.62

NOTE.— N for event-history data is based on total number of organizations multiplied by total number of years in operation (11,345), N for organization-level data is based on number of organizations (870); and N for social movement environment is based on number of years (31)

* Based on last recorded entry

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Sociobiology, Status, and Parental Investment in Sons and Daughters: Testing the Trivers-Willard Hypothesis¹

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While some dismiss sociobiological theories as untestable, post hoc explanations, this article argues that sociologists should instead increase their efforts to identify and engage those theories that have novel empirical implications. Regarding parental investment, Trivers and Willard use Darwinian reasoning to hypothesize that high-status parents favor sons over daughters and that low-status parents favor daughters over sons. The application of this hypothesis to contemporary societies has been widely accepted by sociobiologists, although it has received little actual empirical scrutiny. The Trivers-Willard hypothesis is tested in this study using two nationally representative surveys of American adolescents and their parents. Across several different measures of investment, little evidence of the predicted parental investment behaviors is found. This article seeks not only to contribute to settling the empirical point at issue but also to encourage a renewed and empirically focused dialogue between sociologists and sociobiologists.

INTRODUCTION

Sociological research has long suggested that parental investment strongly influences educational and life outcomes and that investment is a function of both available parental resources and parental choices (Coleman 1966; Blau and Duncan 1967). To understand why some parents choose to invest more than others in the future of their offspring, sociologists typically have emphasized the interplay of a variety of *proximate* factors, including

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parents' incomes and education, family configuration and racial/ethnic background, and children's sex and age (e.g., Steelman and Powell 1991; Schneider and Coleman 1993; Downey 1995). Meanwhile, others contend that parental investment in general, and how it is influenced by proximate variables, can be adequately understood only by attending to *ultimate* causes. In this regard, many have urged sociologists to look to some of the evolutionary theories that have been proposed by sociobiologists (Rossi 1984; Nielsen 1994). These theories posit that parents' behavior toward their children is structured by innate cognitive mechanisms, which developed originally as evolutionary adaptations, and that contemporary patterns of parental investment reflect those behaviors that led to the greatest reproductive success in our evolutionary past. The idea that parental behavior is strongly shaped by past Darwinian pressures has been used to provide evolutionary explanations of phenomena such as why mothers tend to invest more in their children than do fathers (Blum 1997) and why parents tend to invest more in biological children than in step-children (Daly and Wilson 1988).

The real promise of sociobiological theories is not that they can help us make sense of known patterns but that they may enable us to deduce new facts about the social world that have not yet been found. Yet, a recurrent criticism of current sociobiological theories is that they too often provide nothing more than a post hoc explanation of how existing phenomena could have arisen as a direct result of Darwinian selection. Because the theories have no empirical implications beyond what they were originally devised to explain, they cannot be put to any genuine test. While it is true that much of sociobiology's work on parental investment is not deductively based and cannot be readily tested, one prominent and long-standing exception is the Trivers-Willard hypothesis. Using elegant evolutionary reasoning, Trivers and Willard (1973) conjecture that parents should exhibit a sensitivity to their position in the social hierarchy when deciding to invest in sons versus daughters. For reasons described below, Trivers and Willard argue that throughout most of evolutionary history, low-ranking parents have produced the greatest number of grandchildren and great-grandchildren by investing more in their daughters than their sons, while high-ranking parents achieved the most progeny by investing in sons over daughters. For nonhuman animals, Trivers and Willard argue that rank is primarily a matter of "physiological condition," while for humans, rank is to be conceived in terms of position "on a socioeconomic scale." Trivers and Willard predict that, even in the most developed societies, less advantaged parents will favor their daughters and more advantaged parents will favor their sons.

Sociobiologists have widely accepted the applicability of the Trivers-

Willard hypothesis to humans, so much so that Hrdy (1987, p. 101) states that many consider it a "proved theory." Some have argued that the Darwinian impetus for the rich to favor their sons is so strong that it explains why the wealthy of some societies practice female infanticide (Dickemann 1979). Others claim that the impetus for the poor to favor their daughters explains why, in some impoverished societies, infant mortality rates are much higher for boys than girls (Cronk 1989). It also has been suggested that the biases in parental investment predicted by Trivers and Willard exert lifelong effects on personality: Sulloway (1996, p. 431), for example, speculates that upper-class women may tend toward radicalism as the result of being systematically "discriminated against" by their parents.

Evidence for the Trivers-Willard hypothesis among humans is mixed, despite its confident use by many sociobiologists. Some seem to take for granted its applicability to parental behavior toward older children and adolescents in advanced Western societies, but to our knowledge the hypothesis has never formally been tested within this population. This article seeks to determine whether such confidence in the Trivers-Willard hypothesis is warranted by testing whether its predictions hold true among parents of adolescents in the United States. We use primarily a large, nationally representative sample of American eighth graders and their parents, but we also supplement the analysis with comparable data on high school sophomores. By testing the Trivers-Willard hypothesis, we seek both to contribute toward settling the empirical point in question and to encourage a renewed dialogue between those who embrace sociobiology and those who remain skeptical. At the same time, however, we hope to underscore the importance of using *empirical criteria* to evaluate the role that Darwinian theories should play in sociologists' thinking about social issues.

BACKGROUND

Sociology, Sociobiology, and Science

Both within and outside sociology, the reputation of sociobiology has suffered from its association with a troubled history of efforts to reduce social phenomena to alleged Darwinian roots (see Gould 1981; Degler 1991). From Herbert Spencer (1891) to *The Bell Curve* (Herrnstein and Murray 1994), contested biological reasoning has been used to support political agendas and to justify imperialism, stratification, racial and sexual discrimination, and even genocide. Yet the line of research that emerged from Edward O. Wilson's (1975) *Sociobiology* has been careful, especially in recent years, to distance itself from those who emphasize supposedly innate differences among races and classes (see Nielsen [1994] for a

thoughtful review).² Contemporary sociobiology emphasizes putatively universal aspects of human behavior and argues that these universalities derive from a shared and highly specialized set of cognitive mechanisms ("modules") that developed over millions of years of Darwinian selection. Examples of such modular explanations that have been applied to contemporary developed societies include those offered for language (Jackendoff 1993; Pinker 1994), sexuality (Buss 1994), social contracts (Cosmides and Tooby 1992), and, as is important here, parental investment.

Sociobiology has enjoyed growing visibility in a variety of fields, including psychology (Simpson and Kenrick 1997), anthropology (Barkow, Cosmides, and Tooby 1992), and economics (Rothschild 1990; Krugman 1996). Among sociologists, Cohen and Machalek (1988; see also Vila and Cohen 1993) have offered a theory of criminal behavior very much in the spirit of recent sociobiology: they conjecture that expropriative crime is rooted in an evolved strategic sensitivity to specific conditions and opportunities, but they deny that there are any essential genetic differences between criminals and noncriminals. Concerning parental investment, Biblarz, Raftery, and Bucur (1997) find that men raised in stepfamilies or by single fathers have lower socioeconomic attainment than men raised by both biological parents or by single mothers—a pattern, they argue, that is consistent with Hamilton's (1964) evolutionary model of kin selection.

At present, many sociologists are still skeptical of sociobiology, and few have tried to incorporate sociobiological propositions into their work. Some sociobiologists have taken the resistance of sociologists as evidence of biological ignorance, ideological bias, or intellectual irrelevance. In this vein, van den Berghe (1990, p. 173) expresses sentiments not uncommon in the sociobiological literature: "The general failure of sociologists to understand, much less accept, an evolutionary perspective on human behavior transcends mere ignorance and ideological bias, although it incorporates a good deal of both. It also includes a general anthropocentric discomfort with evolutionary thinking, a self-interested resistance to self-understanding, and a trained sociological incapacity to accept the fundamental canons of scientific theory construction." Two decades ago, Ellis (1977) compared the relationship between sociology and sociobiology to that of astrology and astronomy, predicting that the decline of sociology was imminent if the discipline did not incorporate more sociobiological

² Of particular interest to this article, Nielsen (1994) provides a nice overview and discussion of efforts to apply sociobiological theories to modern societies. In addition, it is following his usage that we apply "sociobiology" to work now known by many other names, most prominently "evolutionary psychology" but also "Darwinian anthropology" and "biosociology."

thinking. More recently, Ellis (1996) has claimed that the shrinking numbers of sociology majors bear out his prediction, maintaining that sociology is doomed if it cannot cure its irrational, unscientific "biophobia."

Criticisms of sociology's resistance to evolutionary explanation typically assume that this resistance has little to do with the merits of the explanations themselves. Yet there are several reasons why a cautious stance toward sociobiological theory may be well justified. As noted, sociobiology has been dismissed by some for offering little more than post hoc explanations (Gould 1997). Many of the theories that do appear to have testable implications are indistinguishable in their predictions from a more parsimonious rational actor (or other) model (Kitcher 1985; Cronk 1991). In addition, sociobiological theories are often so complicated and intertwined that one proposition can be invoked to rescue the empirical failure of another. For example, Simon (1990) has proposed that apparent Darwinian imperatives may sometimes be subverted by strong adaptive pressures toward docility and conformity; the consequence of this for empirical testing, as Horgan (1995, p. 179) points out, is that "if a given behavior accords with Darwinian tenets, fine; if it does not, it merely demonstrates our docility."³ For these reasons, one may doubt whether the bulk of sociobiological claims can be empirically falsified, and, consequently, one may question whether the enterprise deserves the scientific status it claims. If ultimate explanations do not imply new and testable knowledge about the social world, one might ask, then how do they move beyond being just speculations? Because so little is known about our evolutionary past, and because the possibilities of adaptive explanation are so little constrained, the acceptance or rejection of particular sociobiological propositions too often seems to depend less on scientific criteria than on how much one accepts the untestable assumptions underlying the claims.⁴

Nonetheless, it should be remembered that the testability of a claim stands in no necessary relationship to its truth. Many sociologists of scientific knowledge maintain that successful scientific programs are *invariably* built upon at least some assumptions that cannot be empirically falsified (Barnes, Bloor, and Henry 1996). In sociobiology, the ratio of untestable to testable assertions may seem high, but an alternative to dismissing the entire program on these grounds is to make a greater effort to find and

³ Certainly, sociology has not been immune to theoretical frameworks capable of accommodating contradictory empirical possibilities, as pointed out in some of the more prominent criticisms of Parsonian structural-functionalism (e.g., Wrong 1976).

⁴ In this regard, Gould (1997) points out that sociobiological claims that a particular disposition or behavior had adaptive value in our evolutionary past are rarely accompanied by paleontological or other corroborating evidence.

engage those hypotheses that have novel empirical implications. New sociobiological theories are often explicitly presented as challenges to prevailing sociological ideas, and, in recent years, some of these theories have received a popular attention that other social scientists can only envy. By pursuing those claims that can be clearly and straightforwardly tested, such as the Trivers-Willard hypothesis, sociologists not only contribute to resolving the empirical point at issue but also may contribute to the dialogue over where the "burden of proof" should reside for other sociobiological theories that turn on similar assumptions but are not readily testable.

The Trivers-Willard Hypothesis

Trivers and Willard (1973) start with the premise that the reproductive success of males tends to be closely and positively related to their social rank, in part because high-ranking males are more likely to procreate with more than one female. In polygynous human societies, for example, high-status males are more likely than low-status males to have more than one wife (and many offspring), while low-status males are more likely to have no wives (and no offspring). The reproductive success of females is less variable than that of males, for their reproductive potential is less strongly affected by the possibility of multiple mates. As a result, in the vast majority of evolutionary environments, high-status males may be expected to have a higher average number of offspring than their sisters, while low-status females have more offspring on average than their brothers.

According to Trivers and Willard, if we assume that the rank of parents is correlated with that of their children, then it follows that high-status parents who have sons will have more grandchildren than high-status parents who have daughters. On the other hand, low-status parents with daughters will have more grandchildren than low-status parents with sons. This is illustrated in figure 1. Because differences in rates of reproduction are what drive natural selection, Trivers and Willard argue that species should have developed a mechanism by which members vary the sex ratio of their offspring in response to their rank, with low-ranking parents producing more daughters and high-ranking parents producing more sons. Evidence for Trivers and Willard's conjecture about varying sex ratios has been supported in studies of several nonhuman species (e.g., Rivers and Crawford [1974] for mice; McFarland Symington [1987] for spider monkeys; and Clutton-Brock, Albon, and Guinness [1986] for red deer), while the evidence for humans has been more mixed (see Hrdy 1987).

Of more interest to sociologists, however, Trivers and Willard claim

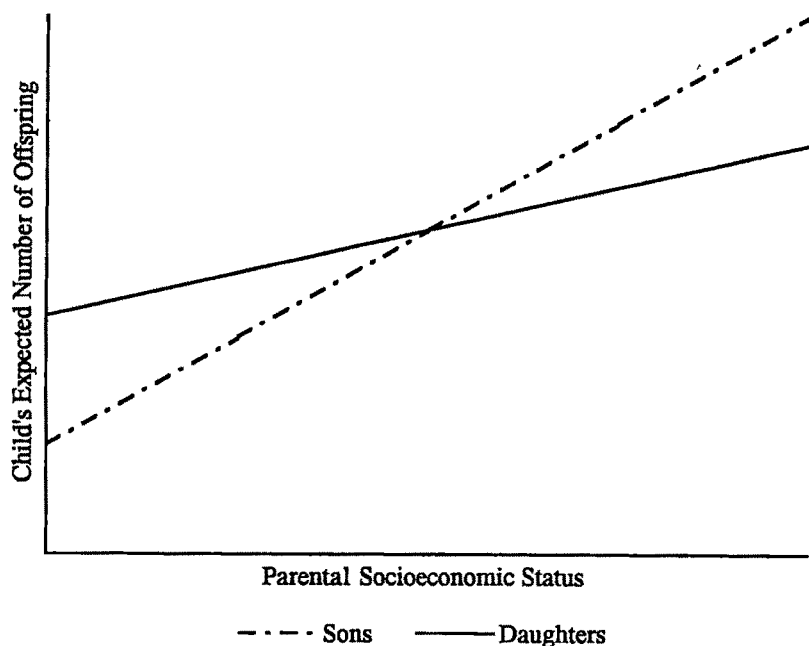


FIG. 1.—The relationship among sex, parents' status, and expected number of offspring (theoretical).

that their hypothesis applies to *parents' behavior toward children after birth just as it applies to the sex ratio*. They write: "If the model is correct, natural selection favors deviations away from 50/50 investment in the sexes, rather than deviations in sex ratios per se. In species with a long period of [parental investment] after birth of young [such as humans], one might expect biases in parental behavior toward offspring of different sex, according to parental condition; parents in better condition would be expected to show a bias toward male offspring" (Trivers and Willard 1973, p. 91).

We are thus to expect low-status parents to invest more in female children than male children, while high-status parents should invest more in males than females. Moreover, as we discuss shortly, once such a tendency has evolved, its influence on parental investment should persist even in evolutionary environments in which a Trivers-Willard effect does not contribute to greater fertility (e.g., in contemporary American society and others in which social status and number of offspring are not positively related). The effect is also expected when it runs counter to apparent cultural

prescriptions (Cronk 1991); along these lines, Wright (1994, p. 173) contends that the Trivers-Willard hypothesis works "by shaping human feelings, not by making humans conscious of its logic."⁵

For children with opposite-sex siblings, the Trivers-Willard hypothesis presents a scenario of direct competition: we would expect sons in high-status families to receive greater investment on average than daughters, while daughters in low-status families should receive more investment on average than sons. At the same time, however, we would also expect to observe concurrent sex differences in the investment received by children with no siblings and children with only same-sex siblings. This is because sociobiological theories of kin selection—the logical base of the Trivers-Willard hypothesis—do not concern only the allocation of resources from parents to offspring (Buss 1995; Pinker 1997; Crawford 1998). Instead, the question of how much to invest in each child is considered part of a much broader problem in which parents must also consider how much to invest in their other blood relatives (e.g., siblings, nieces, nephews, and cousins) and how much to invest in other activities that may help maximize their own fitness (e.g., saving for future offspring, pursuing additional mating opportunities, or attempting to advance one's own position in the status hierarchy). Against these different sources of competition, the greater evolutionary value of sons in advantaged families and daughters in disadvantaged families should influence parental decisions. As a result, the evolutionary logic outlined above leads us to expect similar sex differences in parental investment for children in all types of sibling relationships: comparing across families, boys with high-status parents should receive more parental investment on average than girls, while girls with low-status parents should receive more investment than boys. (At the same time, because one could argue that Trivers-Willard effects should reveal themselves most plainly in families with both sons and daughters, our study examines *both* a general sample of child respondents and a sample restricted to only those children with opposite-sex siblings).

⁵ This said, the prevailing wisdom among those theorists of the "coevolution" of biology and culture would seem to expect cultural prescriptions about child rearing to reflect biological imperatives, rather than be opposed to them (see, e.g., Lumsden and Wilson 1981; Cavelli-Sforza and Feldman 1981; Boyd and Richerson 1985; Barkow 1989). No theory of the actual physiological or cognitive mechanism affecting parental investment in the predicted manner is offered by Trivers and Willard. Our study examines only the question of whether the predicted patterns obtain within a sample of contemporary adolescents. Should these patterns exist and exist for the reasons outlined by Trivers and Willard, we would still not know what are the proximate mechanisms influencing parental behavior.

Research on the Trivers-Willard Hypothesis

Previous studies examining the Trivers-Willard hypothesis have used widely divergent methods and conceptualizations of investment. We noted above that some scholars have claimed to find support for the Trivers-Willard hypothesis in the observation that some less developed societies have higher rates of infant mortality for boys than girls. This work assumes, without corroborating evidence, that sex differences in infant mortality are determined largely by parents' investing more in children of the healthier sex, rather than by other factors, such as sex differences in an infants' vulnerability to a region's diseases. Parental infanticide has also been used as an indicator of an unwillingness to invest in children of the murdered sex; as mentioned above, Dickemann (1979) invokes the Trivers-Willard hypothesis to explain why female infanticide is practiced among the wealthy of some societies. Kitcher (1985), however, argues cogently that female infanticide is unlikely to serve the Darwinian ends that Dickemann suggests. Other studies that claim support for the Trivers-Willard hypothesis measure parental investment in terms of frequency of parent-child interaction (Betzig and Turke's [1986] study of the Ifaluk) and bridewealth payments (Borgerhoff Mulder's [1987] study of the Kipsigis); meanwhile, patterns of parental investment contradicting the Trivers-Willard hypothesis have been observed in cross-cultural comparisons by Hartung (1982; see Betzig 1990) and in ethnographic accounts of the Mundugumor of New Guinea (McDowell 1991).

Testing the Trivers-Willard hypothesis in contemporary Western societies may appear to be complicated by the diminishing relationship between status and fertility: indeed, at present the two may be inversely related (U.S. Bureau of the Census 1991). As a result, although the Trivers-Willard strategy likely would maximize the number of grandchildren in our evolutionary past, it would not necessarily do so today. At the same time, however, a core principle of the leading program in sociobiology (evolutionary psychology) is that modern, developed societies have not existed long enough to reverse or substantively alter the cognitive mechanisms that have evolved over the last thousands or millions of years (Nielsen 1994; Crawford 1998). For sociobiologists, the tendency for humans to seek and value status is rooted in its connection to reproductive success; the idea that humans in contemporary societies value status *as if* it were still connected to fertility provides the linchpin of contemporary applications of many sociobiological theories (e.g., Buss [1994] on sexual attraction; Wright [1994, pp. 242–50] on gender stratification; Thornhill [1998] on aesthetics; and Fisher [1992] on marriage and divorce). As a consequence, to expect the Trivers-Willard hypoth-

esis to hold under contemporary conditions is consistent with the prevailing theoretical logic of sociobiology. Moreover, Gaulin and Robbins (1991) provide evidence that the assumptions necessary for the Trivers-Willard mechanism to evolve still hold in present-day North America.

Indeed, Trivers and Willard *specifically* cite the contemporary United States as an example of the applicability of their hypothesis to human societies. For the contemporary United States and Canada, Gaulin and Robbins (1991) also report a series of findings that they claim are consistent with the hypothesis, but their measures of parental investment (primarily nursing behavior and interbirth interval) are questionable.⁶ A study of contemporary inheritance practices in the Vancouver area found that parents with large estates tended to favor sons in their bequests, while parents with small estates favored their daughters (Smith, Kish, and Crawford 1987). While this would seem to support the Trivers-Willard hypothesis, a similar study of inheritance in Sacramento County failed to replicate these results (Judge and Hrdy 1992). At the same time, we know of no study of a developed society that tests whether the Trivers-Willard hypothesis holds for parents of pre- or early adolescents. This is somewhat surprising, for some sociobiologists have argued that parents should be most strongly attached to children of this age (Crawford, Salter, and Jang 1989).

Sociological Research on Parental Investment

Although sociologists have heretofore not addressed the Trivers-Willard hypothesis, sociological work in the status attainment, rational choice, and human capital traditions has afforded a number of important insights into the proximate variables that affect parental investment. Muller and Kerbow (1993) report a strong positive correlation between parents' education and parents' involvement with their children; Muller (1993) and Muller and Kerbow (1993) find a similarly high correlation between involvement and parents' income. Family structure also influences investment: children in step- and single-parent households (Downey 1994), children in large families (Blake 1989; Steelman and Powell 1991; Downey 1995), and children spaced closely together (Powell and Steelman 1993, 1995) all

⁶ Sieff (1990) notes problems with using nursing behavior as a measure of parental investment for humans. In using birth spacing as a measure of investment, Gaulin and Robbins (1991) use not only the interval following a child's birth but also the interval *prior*, although this would imply the unlikely scenario of parents systematically waiting longer to conceive because they know what the sex of their next child will be.

tend to receive fewer resources from their parents than other children. None of these main effects on investment presage or preclude the interaction between child's sex and parents' status predicted by Trivers and Willard.

Concerning the effect of child's sex on investment, recent research has challenged the conventional wisdom (e.g., Rosen and Aneshensel 1978) that parents in the United States are generally biased in favor of boys. Carter and Wojokietwicz (1997), for example, find that parents are more closely involved in the education of their daughters than their sons. In a study more tightly focused on pecuniary investments in children, economists Behrman, Pollak, and Taubman ([1986] 1995, p. 85) suggest that "parental preferences either exhibit equal concern or slightly favor girls." Parents, however, do report preferring that their next child be male; contradicting the Trivers-Willard hypothesis, Coombs (1978) reports the preference for sons is strongest among those of low status. Parents with male children also are more likely to remain married than parents with no children or only female children (Morgan, Lye, and Condran 1988). As divorce has been suggested to have a number of negative effects on children, including reduced investment (Seltzer 1994), boys may be said to profit relative to girls from a lower likelihood of their parents divorcing.

These last differences aside, at present, the United States is unusual among world societies for its relatively high levels of investment in daughters. Research on other developed and (especially) developing societies has documented a clear bias pervading all social classes in favor of boys (Fawcett 1983; King and Hill 1993; see also, e.g., Parish and Whyte [1978] on China; Brinton [1988] on Japan; Rosenzweig and Schultz [1982] on India; Tallman, Marotz-Baden, and Pindas [1983] on Mexico; Kagitcibasi and Sunar [1992] on Turkey; and Buchmann [1996] on Kenya). It may seem inconsistent with the Trivers-Willard hypothesis that the United States, as one of the world's wealthiest societies, would feature relatively high levels of average investment in daughters versus sons when compared to other, less developed societies. At the same time, this observation is counterbalanced by Greenhalgh's (1985) finding that as East Asian societies have become more wealthy, the bias in parental investment toward sons has become stronger (as Trivers and Willard might predict). However, the Trivers-Willard hypothesis ultimately concerns differences in parental investment that occur *within* societies, not between them, and testing the hypothesis requires data comparing children in the same society.⁷ Our test using such data is described in the next section.

⁷ Intrafamilial comparisons are not possible using NELS or other data of similar size, scope, and quality concerning adolescents. We have no reason to expect our use of interfamilial instead of intrafamilial data to bias systematically the relevant interaction

DATA AND METHODS

Data

To test the Trivers-Willard hypothesis, we rely primarily on the National Educational Longitudinal Study of 1988 (NELS), a general-purpose survey of 24,599 eighth graders sponsored by the National Center for Educational Statistics. NELS was designed to provide information on a wide variety of factors thought to affect children's development, educational performance, and life outcomes. Most of our measures are constructed from the 1988 survey, but two are taken from a 1990 follow-up of the original respondents. NELS participants were chosen using a stratified, two-stage probability sample in which first schools were randomly selected and then students were randomly selected within these schools.⁸

The NELS data are particularly appropriate to testing sociobiological hypotheses of parental investment for several reasons. First, the data set includes many different measures of investment, drawing upon information gathered from the students and their parents.⁹ Using different measures drawn from different sources increases the assurance that our findings are less vulnerable to biases in student or parent responses or problems with any single measure of investment. Second, as a study of eighth graders, the NELS data capture students at a relatively advanced stage of the period of intensive parental investment and just at the onset of their own potential reproductive careers. As noted above, this is also the age in which sociobiologists expect the highest level of attachment of parents to children. Third, the large sample size not only permits the use of multivariate analyses and the testing of interaction effects but also increases the likelihood that even very modest influences on parental investment will be statistically significant; the data consequently allow a relatively liberal test of the hypothesis.

effects in one direction or the other. Moreover, comparisons of results of interfamilial and intrafamilial studies indicate that they largely tend to reveal the same patterns but that results from intrafamilial studies tend to be less significant than those from interfamilial studies. As a result, using interfamilial data here may be seen as providing a *generous* test of the hypothesis.

⁸ Winship and Radbill (1994) discourage using sampling weights in certain circumstances, but weights are necessary here because the weights are a function of at least one of the dependent variables (enrollment in private school). Analyses using unweighted data yield similar results.

⁹ Most research indicates that, under conditions similar to those here, adolescents' and parents' reports of behavior are reasonably consistent with behavioral data gathered through other means (see, e.g., Davies and Kandel 1981; Swearingen and Cohen 1985). Some work has suggested that parents overestimate their investment in their children relative to children's own reports; in this circumstance, we would expect the intercepts of our models to be affected but not the interaction effects at issue here.

Measures of Parental Investment

The Trivers-Willard hypothesis predicts that parents of low socioeconomic status will invest more heavily in daughters than sons, while high-status parents will invest more heavily in sons than daughters. Sociobiology tends to treat parental investment as a unidimensional concept, encompassing everything from the production of children per se to all behaviors toward children throughout their development. Sociologists, on the other hand, have considered there to be several, qualitatively different, means of investment through which parents may expend resources to positively affect the futures of their children. Parents may be willing to expend enormous resources for their children's benefit in some ways while being much less generous in others. In a similar way, some forms of investment may be marked by a systematic favoring of one gender over another, while others are not. Focusing on one type of resource may obscure evidence supporting the Trivers-Willard hypothesis. As a consequence, to provide as broad a test as possible, we analyzed how child's sex and parents' status influence five general categories of investment: *economic*, *interactional*, *supervisory*, *social*, and *cultural*.¹⁰ These categories should not be taken as mutually exclusive, for there undoubtedly is some overlap among them. Table 1 provides summary statistics and descriptions of all the measures of parental investment used in our study, as well as of the measures of socioeconomic status discussed in the next section.

Economic investments.—Economic investments may be the most salient way in which parents differ in their level of investment: parents differ in the amount of money they have to invest, but they also choose to spend more or less of this money on their children. We concentrate here on measures of parents' spending on their children's education.¹¹ In the NELS questionnaire, parents were asked whether they had saved any money for their children's future education; those who reported saving money were asked how much they had saved. We use both as measures of economic investment. We also use whether or not parents send their children to a private school. While parents may send their children to a private school for a variety of reasons, the utilization of private schools very often implies

¹⁰ Indeed, a recurrent criticism of sociological, and economic, scholarship on parental investments is that it has not looked at a wide enough range of resources (Coleman 1988). At the same time, as the results will show, our overall conclusions about the Trivers-Willard hypothesis do not stand or fall on the inclusion or exclusion of any one measure of parental investment or even of any one *type* of investment.

¹¹ Investments in education benefit children not only in terms of their occupational future. Access to more privileged educational institutions (e.g., private schools, elite colleges) also increases the likelihood that the child's peers will be from high-status families and, more important from a sociobiological perspective, increases the likelihood that the child will marry and reproduce with a high-status mate

salient financial considerations. Another measure of economic investment we use is the number of educational items families have in their homes, such as reference books and periodicals.¹² The presence of these objects in the home is associated with both superior achievement within school and higher incomes as adults, even for children of similar socioeconomic backgrounds (Teachman 1987; Downey 1995; Fejgin 1995). As with private schooling, the acquisition of educational objects implicates considerations beyond the economic, yet the objects also entail expenses that parents could have foregone.

Interactional investments.—Financial expenditures may greatly enhance children's futures, but several studies also show that children benefit from greater personal involvement by parents in their education (Muller 1993; Sui-Chu and Willms 1996; Carter and Wojtkiewicz 1997). Parents may provide instruction and guidance to their children by regularly talking with them about their educational experiences. We measure the amount of parent-child interaction about education by how often children talk to their parents about course selection, school activities, and class material. Parents may be more or less actively involved in their child's school, which we measure in terms of whether parents have gone to a school event, attended a school meeting, or visited the child's classes in the current school year. Parents may also participate more or less actively in the school's parent-teacher organization, which allows them to receive information about curricula and perhaps also to influence school policies or teacher expectations in their child's favor. We measure affiliation with a parent-teacher organization in terms of membership, meeting attendance, and involvement in the organization's activities.

Supervisory investments.—Just as parents are more or less closely involved in their child's schooling, they also more or less closely supervise their child's activities outside of school. In rational choice theory, effort spent monitoring and supervising the activities of other persons is a cost no different than spending money (Becker 1981; Hechter 1987). Parental supervision is associated positively with improvements in children's self-reliance, happiness, sense of social responsibility, and educational success (Baumrind 1989). Here, we use a scale of parental supervision that measures how much effort parents spend trying to find out what their child does outside of school, who the child's friends are, and how the child spends her or his money.

Social investments.—In developing the concept of "social capital," Coleman (1988, 1990, pp. 300–21) has argued that overlap between the interactional networks of parents and children may provide important

¹² When a personal computer is included among educational objects in the home, the relevant results do not change.

TABLE 1

DESCRIPTIVE STATISTICS FOR PARENTAL INVESTMENT AND SOCIOECONOMIC STATUS, NATIONAL EDUCATIONAL LONGITUDINAL SURVEY, 1988

Variable Name	Description	Metric	Mean	SD	N	Source
Measures of Parental Investment:						
Economic investment in child's future.						
Started saving for child's education	Whether parent(s) have begun saving for their child's education after high school	0 = no, 1 = yes	.50	.50	18,417	Parent
Money saved for college ..	Amount of money parents have saved for child's future education	Thousands of dollars	5.52	5.26	8,542 ^a	Parent
Private school	Child attending a private school in eighth grade	0 = child in public school 1 = child in private school	.20	.40	21,188	School
Educational objects in home ^b	Presence in home of (1) place to study, (2) daily newspaper, (3) regular magazine, (4) encyclopedia, (5) atlas, (6) dictionary, (7) more than 50 books, (8) pocket calculator, (9) typewriter	Number of items	6.95	1.67	19,874	Student

Parental involvement in child's education:

Talk with child about school ^b	Frequency child talks to parents about (1) school activities, (2) course selection, (3) things studied in class, and talks to (4) mother or (5) father about planning high school program	0 = has not talked to parents about any of these to 10 = has talked more than three times this year to parents about each of these	6.67	2.38	20,188	Student
Involvement with child's school	In the current school year, parents have (1) attended a school event, (2) attended a school meeting, (3) spoken with child's teacher or counselor, (4) visited child's classes	0 = parent has not done any of these to 4 = parent has done each of these	2.15	1.21	15,417	Student
Parent-teacher organization ^b	Parent (1) belongs to PTO, (2) attends PTO meetings, (3) takes part in PTO activities	0 = parent does none of these to 3 = parent does each of these	98	1.13	20,280	Parent
Supervision of child's activities. Monitoring of child's behavior ^b ...	Parents try to find out (1) who child's friends are, (2) how child spends free time, (3) where child goes after school, (4) where child goes at night, (5) how child spends her/his money	0 = parents try "not at all" to find out any of these to 15 = parents try "a lot" to find out each of these	9.78	3.74	12,555	Student ^c
Investment in social capital. Knows child's friends	No. of eighth grader's friends parent know by first name or nickname	No. of friends known (maximum 5)	3.56	1.69	20,305	Parent
Knows child's friends' parents	No. of friends' parents child's parent knows	No. of friends' parents known (maximum 5)	2.67	1.69	20,305	Parent

TABLE 1 (Continued)

Variable Name	Description	Metric	Mean	SD	N	Source
Investment in cultural capital.						
Cultural classes ^b	Student has attended classes outside school in (1) art, (2) music, (3) dance, (4) language, (5) computers	0 = no classes to 5 = classes in all of these areas	.49	.76	19,245	Parent
Cultural activities ^b	Student has gone to (1) art, (2) science, (3) history museums	0 = has not visited any museums to 3 = has visited each type of museum	1.51	1.26	19,723	Parent
Key independent variables:						
Family income	Family income from all sources, 1987	Dollars \times 10,000	4.16	3.79	21,188	Parent
Parents' education	Educational level of most highly educated parent	0 = did not finish high school to 4 = at least an M.A. or equivalent	2.07	1.15	21,188	Parent
Male	Child's sex	0 = female, 1 = male	.50	.50	21,188	Student

^a Item asked only of those parents who indicated that they had saved some money for their child's future education

^b Items were factor analyzed. Alphas (standardized) talk about school = .72, parent-teacher organization = .73, monitoring behavior = .84, parental control = .82, educational objects = .62, cultural classes = .53, cultural activities = .80, evaluation of parents = .82

^c Items from NELS follow-up questionnaire, 1990 (sophomore year)

educational benefits in its own right (see also Lee and Brinton 1996; Teachman, Paasch, and Carver 1996, 1997). When parents know their children's friends, they are better able to judge the merits of the child's peer group and may also have an additional source of information about the child's feelings and possible problems. When the parents of adolescent friends know one another, they are able to exchange information about their children's activities, help one another enforce family rules, and share responsibility for monitoring and supervision. We measure social capital both as the number of the child's friends that the parent can identify by first name or nickname and the number of these friends' parents that the child's parent knows.

Cultural investments.—Finally, parents who regularly provide their children with the opportunity to participate in elite culture activities (e.g., theatre, museums) are considered to be investing in the "cultural capital" of their children (Bourdieu 1977). As elite tastes and cultural knowledge are socially valued, children with high cultural capital are theorized to be more likely to enjoy educational and occupational success and are more likely to marry a high-status partner. Studies have largely supported this thesis (DiMaggio 1982; DiMaggio and Mohr 1985; Kalmijn and Kraaykamp 1996). We operationalize Bourdieu's investment in cultural capital in terms of the child's enrolling in art, music, dance, language, or computer classes outside of school and in terms of the child's visiting art, science, or history museums.

Measures of Socioeconomic Status and Additional Controls

Although Trivers and Willard (1973) contend that their results may be applied to humans in terms of position on a "socioeconomic scale," they do not specify how to measure socioeconomic position within a society. Because socioeconomic status has long been considered a function of a family's income and parents' education, we use both as measures of status. Family income is measured as the total income (in dollars) earned by the family from all sources in 1987, the year before the survey was given. Education is measured on a scale of progressive achievement indicating whether the most highly educated parent graduated from high school, attended college, graduated from college, or received an advanced professional degree.

We included four additional controls in our model so that possible Trivers-Willard effects are not obscured by potentially confounding variables.¹³ First, we control for the number of siblings the child has (including

¹³ As it turns out, the substantive conclusions of our paper are unchanged by the inclusion or exclusion of these control variables

all full-, half-, and stepsiblings), as it is conceivable that favoritism toward males or females may be affected by family size. Second, we control for the age of the child's mother. Steelman, Powell, and Carini (1999) show that children tend to benefit from having older parents; including mother's age in the model controls for the possibility that these benefits are unequally distributed between sons and daughters. Third, to take into account the family structure effects on investment described above, we include a control for whether the child resides with both of her or his original parents. Fourth, because both parental investment and gender preferences may vary by race/ethnicity, we include dummy variables indicating the racial/ethnic background of the child (white, black, Latino, Asian, or Native American).

Because our measures of socioeconomic status are taken from the parents' questionnaire, we could use only those cases in the data set for which both student and parent questionnaires are available, which excluded 1,948 of the original 24,599 cases (7.9%). Another 1,463 cases (5.9%) were dropped because parents either did not complete the income or education questions or their questionnaires were missing information on one of the control variables.¹⁴ This left 21,188 cases in our sample. In the models estimated below, cases were also deleted if respondents did not complete any of the questions used to construct the measure of parental investment used as the dependent variable; consequently, sample sizes for the regressions are usually less than 21,188.

Supplemental Analyses

To ensure that our findings are not idiosyncratic to one data set, we supplement our analysis of NELS data with an examination of the 1980 High School and Beyond (HSB) study. Also conducted by the National Center of Educational Statistics, HSB is a large, nationally representative survey of adolescents that uses a format and sampling procedure similar to NELS, with three important differences. First, while the base-year respondents in NELS are eighth graders, HSB interviews high school sophomores and seniors. Our analysis of HSB here uses the tenth-grade respondents only.¹⁵ Second, whereas NELS attempted to interview parents of all student respondents, in HSB only a randomly selected subsample of

¹⁴ Because there is a comparatively large number of missing cases in NELS for mother's age, missing values on this variable were imputed from other independent variables.

¹⁵ Analyses using the twelfth-grade sample yield no additional support for the hypothesis.

parents was interviewed. Third, because HSB does not contain information on the sex of the child respondents' siblings, we were unable to conduct all of the supplementary analyses for HSB that we present for NELS.

All of the measures of economic investment that we tested from NELS have clear counterparts in HSB. Three measures—whether parents had started saving for their child's future education, how much parents had saved, and whether parents have enrolled their child in a private school—are based on virtually identical phrasing in HSB and NELS; the remaining measure, educational objects in the home, differs only in that HSB asks about fewer objects. To examine interactional and supervisory investments, we include measures of how frequently parents talk with their child and how closely they monitor their child's activities. Although substantively similar to measures we use from NELS, these are based on different questionnaire items. HSB, unfortunately, does not contain any measures of social or cultural capital comparable to those in NELS. Means, standard deviations, and descriptions of the HSB measures of parental investment are provided in table 2. Sample sizes of these measures vary widely because we rely on the much smaller sample of parent questionnaires for our two measures of savings for college.

In our analysis of HSB, measures of parents' income and education are drawn from the student surveys rather than parents' reports. This was done so that the full sample could be retained when analyzing the measures of investment drawn from student or school information. In auxiliary analyses in which the sample was restricted to only those cases in which parent interviews were available, substantively identical results were obtained regardless of whether student or parent reports were used to measure education and income.

RESULTS

Main Effects of Sex, Education, and Income

Table 3 presents OLS and logistic regression estimates for two models of parental investment. The main-effects model estimates the effects of child's sex and parents' status on parental investment, while the interaction-effects model tests whether the relationship between child's sex and investment varies with increased status. The Trivers-Willard hypothesis is tested by the second model, but a brief consideration of the main-effects model is worthwhile, as the results indicate that our measures of parental investment behave similarly to those used in other studies. Like Carter and Wojtkiewicz (1997), we find that girls receive a higher expected level of investment than boys for several of our dependent variables. Girls have more interactions with their parents about school, are more heavily super-

TABLE 2
DESCRIPTIVE STATISTICS FOR PARENTAL INVESTMENT AND SOCIOECONOMIC STATUS, HIGH SCHOOL AND BEYOND, 1980

Variable Name	Description	Metric	Mean	SD	N	Source
Measures of parental investment:						
Started saving for child's education	Whether parent(s) have begun saving for their child's education after high school	0 = no, 1 = yes	.48	.50	2,390	Parent
Money saved for college	How much money parents have saved for their child's future education	Thousands of dollars	3.04	3.28	1,016 ^a	Parent
Private school	Child attending a private school in tenth grade	0 = child in public school 1 = child in private school	.13	.36	20,667	School
Educational objects in home ^b	Presence in home of (1) place to study, (2) daily newspaper, (3) encyclopedia, (4) more than 50 books, (5) pocket calculator, (6) typewriter	Number of items	4.60	1.29	19,387	Student

Talk with child ^b	Frequency child talks to parents about personal experiences and planning high school program	0 = does not talk with parents about either of these to 4 = talks with parents "every day" about personal experience and "a great deal" about planning high school program	3.45	1.44	20,152	Student
Monitoring of child's behavior ^b	Parents (1) keep close track of how child is doing in school and (2) almost always know where child is and what child is doing	0 = neither parent does either of these to 3 = parents do both of these	2.51	.79	19,954	Student
Key independent variables:						
Family income	Estimated family income from all sources, 1980	Dollars × 10,000	2.03	.99	20,667	Student
Parents' education	Educational level of most highly educated parent	0 = did not finish high school to 4 = at least an M.A. or equivalent	1.76	1.22	20,667	Student
Male ..	Child's sex	0 = female, 1 = male	.49	.50	20,667	Student

^a Item asked only of those parents who indicated that they had saved some money for their child's future education

^b Items were factor analyzed. Alphas (standardized): monitoring of activities = .54, talk with parents = .63, educational objects = .54

TABLE 3
UNSTANDARDIZED COEFFICIENTS FROM REGRESSIONS OF PARENTAL INVESTMENT MEASURES USING THE NATIONAL EDUCATIONAL
LONGITUDINAL STUDY, 1988

MEASURE OF INVESTMENT	MAIN-EFFECTS MODEL			INTERACTION-EFFECTS MODEL			
	Male	Education	Income	Male	Education	Income	Male × Education Income
Economic investment in child's education:							
Started saving for child's education ^a071* (.031)	.282*** (.017)	.096*** (.006)	-.040 (.101)	.201*** (.026)	.138*** (.014)	.011 (.035)
Total money saved for college266** (.097)	.456*** (.054)	.521*** (.001)	.017 (.337)	.366*** (.095)	.524*** (.028)	.082 (.124)
Private school ^a	-.016 (.038)	.364*** (.019)	.131*** (.005)	-.196 (.123)	.268*** (.043)	.073*** (.012)	.018 (.037)
Educational objects in home ..	.032 (.021)	.371*** (.011)	.057*** (.003)	.066 (.069)	.368*** (.017)	.064*** (.006)	-.015 (.023)
Involvement in child's school:							
Talk with child about school	-.460*** (.032)	.394*** (.017)	.043*** (.005)	-.640*** (.109)	.375*** (.026)	.044*** (.009)	.022 (.035)

Involvement with child's school054** (.019)	.216*** (.010)	.028*** (.003)	.271*** (.059)	.253*** (.014)	.026*** (.005)	-.072*** (.020)	.001 (.007)
Parent-teacher organization013 (.015)	.183*** (.008)	.045*** (.003)	.019 (.040)	.184*** (.013)	.042*** (.005)	-.014 (.015)	.008 (.006)
Supervision of child's activities:								
Monitoring of child's behavior	-.949*** (.066)	.226*** (.035)	.005 (.009)	-.834*** (.242)	.299*** (.055)	-.012 (.018)	-.126 (.081)	.006* (.003)
Investment in social capital:								
Know child's friends	-.313*** (.022)	.248*** (.012)	.022*** (.003)	-.379*** (.068)	.235*** (.018)	.023*** (.006)	.010 (.022)	.010 (.008)
Know child's friends' parents	-.166*** (.022)	.219*** (.012)	.036*** (.003)	-.209** (.064)	.210*** (.020)	.035*** (.006)	.010 (.022)	.007 (.008)
Investment in cultural capital:								
Cultural classes	-.381*** (.098)	.152*** (.005)	.028*** (.002)	.025 (.027)	.201*** (.010)	.043*** (.004)	-.097*** (.011)	-.028*** (.004)
Cultural activities029 (.017)	.277*** (.009)	.034*** (.003)	.078 (.048)	.296*** (.013)	.038*** (.005)	-.025 (.018)	.003 (.007)

NOTE.—Robust SEs are in parentheses. *N*s range from 8,542 to 21,188 (see table 1). Models include additional controls for respondent's race/ethnicity, marital status of parents, mother's age, and number of siblings in family.

* Logistic regression of dichotomous dependent variable, all others use OLS regression.

* $P < .05$ (two-tailed test)

** $P < .01$.

*** $P < .001$.

vised by their parents, have a greater investment in social capital, and are more likely to have taken cultural classes outside of school than boys.¹⁶ These apparent advantages do not carry over to economic investment, however: parents of boys are more likely than parents of girls to have begun saving for their child's future education, and on average they have also saved more money.

Consistent with Muller and Kerbow (1993) and Muller (1993), the results for the main-effects model also indicate that education and income strongly and positively affect the provision of many different forms of parental investment, including those not directly related to available material resources.¹⁷ All of our measures of parental investment are positively affected by increases in parents' education. Meanwhile, the only measure of investment not positively affected by increases in family income is parents' monitoring of their children. That the effects of income on this dependent variable differ so markedly from the other measures of investment raises the possibility that monitoring (at least as operationalized here) may not be an appropriate measure of parental investment; instead, it may reflect other qualities of parents, such as a tendency toward authoritarian behavior or a more general propensity toward regulation.

Testing the Trivers-Willard Hypothesis

The Trivers-Willard hypothesis predicts that increases in education and income should yield greater returns in parental investment for sons than for daughters. The interaction effects model in table 3 tests the Trivers-Willard hypothesis by adding interaction terms that test whether the effects of increases in parents' status differ for boys and girls. The measures of investment used here are constructed so that the coefficients of the interaction terms will be positive when effects are consistent with the hypothesis and negative when inconsistent.

As may be seen in table 3, however, very few of the interaction effects

¹⁶ While we focus on the Trivers-Willard hypothesis, others might offer evolutionary explanations for some of the *main* effects of gender on investment that we observe. For example, parents may invest more in the cultural capital of daughters than sons because this is perceived as more important for daughters in luring a mate. Parents may supervise or invest more in the social capital of daughters to better ward off unapproved matings (which, in an evolutionary sense, have a greater cost for parents of daughters than parents of sons). None of these change the *interaction* effects between sex and status that Trivers and Willard predict.

¹⁷ Comparing standardized coefficients (not shown), the effect of education on all the interactional, social, and cultural measures is significantly stronger than the effect of income. Education and income more equally affect economic investment, not surprising, financial investments in children are strongly impacted by parents' earnings.

Testing the Trivers-Willard Hypothesis

are significant. Contrary to the expectations of the Trivers-Willard hypothesis, we observed few differences between boys and girls in the effect of either parents' education or income on the amount of investment received. In the interaction of sex with education, significant interaction effects were observed for only two (of 12) measures of investment, and both of these were in the direction *opposite* of that predicted by Trivers and Willard.¹⁸ As parents' education increases, the expected increase in parental involvement in schooling is larger for daughters than sons. Likewise, increases in education also yield a larger increase for daughters than sons in the number of cultural classes taken.

The hypothesis fares only slightly better when we consider the interaction between child's sex and family income. Again only two of the 12 interactions we tested are significant; only one is consistent with the hypothesis. As Trivers and Willard would predict, income increases affect parents' monitoring of sons more positively than it affects their monitoring of daughters.¹⁹ This is the only significant effect supporting the hypothesis we observed for our 12 measures of parental investment and two measures of status.²⁰ Given the number of tests we conducted, we cannot rule out the possibility that this single effect supporting the hypothesis is simply the result of chance.²¹ In addition, we suggested earlier that the monitoring variable may be a poor measure of investment because, unlike the other measures, monitoring was not positively affected by income.

Additional Tests Using NELS

Up to this point, our analyses provide strong reason to question the applicability of the Trivers-Willard hypothesis to these data. Few significant effects were observed, and those that were found were more often than

¹⁸ A TOBIT regression of total dollars saved for college on the independent variables that treated those cases in which parents had saved no money as left-censored ($\tau = 0$) did not yield a significant Trivers-Willard effect ($P_{\text{male} \times \text{education}} = .83$, $P_{\text{male} \times \text{income}} = .53$).

¹⁹ When income is measured in logged dollars, the interaction between child's sex and family income significantly affects the number of child's friends known by the parent in the direction predicted by the Trivers-Willard hypothesis ($\beta = .668$, $P = .048$). However, the effect of the interaction of sex and income on the monitoring of child's activities is no longer significant ($P = .214$).

²⁰ Analyses measuring status in terms of parents' occupation (0 = unemployed to 4 = upper professional [e.g., doctor, lawyer]; see Downey 1995, p. 753) also failed to reveal any significant interaction effects supporting the hypothesis.

²¹ Moreover, it is unclear whether the observed interaction is actually consistent with what Trivers and Willard would predict. Instead, for all levels of income, parents spend more effort monitoring their daughters than their sons. The gap between daughters and sons narrows as income increases, but the difference is reduced by less than 40% from the lowest to highest income quintiles.

not opposite the direction that the model predicts. To check if these findings were robust to alternative specifications, we conducted a variety of additional tests. First, because (as discussed above) the children who face the most direct competition for parental resources are those with opposite-sex siblings, we tested the Trivers-Willard hypothesis on a NELS subsample that excluded all child respondents with either no siblings or with only same-sex siblings.²² The results of these analyses are presented in table 4. Looking at the interaction-effects model, we find significant interactions between child's sex and parent's education for four variables: educational objects in the home, involvement in child's schooling, monitoring of the child's activities, and cultural classes taken. None of these significant interactions are in the direction predicted by the hypothesis. The only interaction between child's sex and family income that is significant is for the number of cultural classes taken, and this too is opposite the predicted direction.

Second, we considered the possibility that the Trivers-Willard hypothesis may only differentiate those at income extremes (perhaps because such a large majority of Americans perceive themselves as "middle class"). Comparing those families with the lowest annual incomes (less than \$10,000) to those with the highest (greater than \$75,000), we find no significant differences in investment that support the hypothesis. Along similar lines, we examined whether parental behavior is sensitive to local-level differences in status rather than national-level differences. When we compare families whose incomes are below the average of the other NELS respondents from their school to those whose incomes are above average, we again find no significant effects that support the hypothesis.²³

Supplemental analyses also indicate that our results are not substantively affected by the inclusion or exclusion of any of the added control

²² Information on the sex composition of sibling relationships was included only on the 1990 NELS follow-up survey. Accordingly, when restricting the sample to children with opposite-sex siblings, we could use only those cases for which 1988 and 1990 data were available.

²³ We also looked at several other measures of economic investment: the amount of money parents expected to pay for their child's education, parents' willingness to go into debt to finance their child's education, how early parents' had started saving for their child's future education, and whether parents had enrolled the child in a pre-school or Head Start program prior to kindergarten. None revealed any significant interactions between parents' status and child's sex. In addition to looking at measures of parental investment, we looked at other items, which, while not measures of investment themselves, could be seen as proxies. Of these, the child's positive regard for her or his parent yields significant results in the direction opposite the hypothesis, while parents' educational expectations for the child yields significant results in the predicted direction.

variables.²⁴ The results do not substantively change when the sample is restricted to those child respondents who live with both of their original parents (eliminating all families with single parents or stepparents). Separate analyses of white, black, Hispanic, and Asian respondents indicate that the patterns observed in tables 3 and 4 are reasonably consistent across all of the groups.

Supplemental Analyses Using HSB

As mentioned earlier, we investigated the possibility that our findings were idiosyncratic to the NELS data set by conducting a similar analysis of HSB. Table 5 presents the estimated effects of a main-effects and interaction-effects model on measures of parental investment from HSB. Comparing the main-effects model in table 5 with that of table 3 (in which the same regressors were examined using NELS) shows that results are generally consistent across data sets. Both data sets certainly evince strong main effects of income and education across various measures of investment. More important, however, when we look to the interaction-effects model in table 5, we see little support for the Trivers-Willard hypothesis. No significant interactions between child's sex and family income are observed. The interaction of sex and education is significant for only one variable, frequency of talk between parent and child; however, this effect is in the direction opposite of that predicted: increases in education yield a larger expected increase in frequency of talk for daughters than sons.

As with NELS, we performed a number of auxiliary analyses to check the robustness of our findings (available from the authors). Dividing the data into subsamples based on race and parents' marital status yields no additional support for the hypothesis. In a similar way, we find no support for the hypothesis when only those with the highest and lowest incomes are compared or when parents' education and income are measured relative to the mean of the other HSB respondents from the same school. Meanwhile, when parents' education is used as the only measure of status in the model (as was done with NELS in table 4, model 1), the interaction of child's sex and education is significant in the expected direction for

²⁴ Significant effects supporting the Trivers-Willard hypothesis may be obscured by our using the model to estimate the interaction of child's sex with two different measures of socioeconomic status. We tested this possibility by performing analyses in which the two interaction terms were estimated in separate models. For the full sample, we observed two substantively consequential divergences from results presented in table 3: the interaction between income and child's sex is significant for frequency of talk about school, but the sex-by-income interaction is no longer significant for monitoring of the child's activities. For the sample restricted to children with opposite sex siblings, the results were substantively identical to those presented in table 4.

TABLE 4

UNSTANDARDIZED COEFFICIENTS FROM REGRESSIONS OF PARENTAL INVESTMENT MEASURES FOR CHILDREN WITH OPPOSITE-SEX SIBLINGS
ONLY USING THE NATIONAL EDUCATIONAL LONGITUDINAL STUDY, 1988

MEASURE OF INVESTMENT	MAIN-EFFECTS MODEL			INTERACTION-EFFECTS MODEL		
	Male	Education	Income	Male	Education	Income
Economic investment in child's education:						
Started saving for child's education ^a084 (.047)	.302*** (.025)	.102*** (.010)	-.055 (.155)	.304*** (.037)	.145*** (.019)
Total money saved for college	.062 (.148)	.477*** (.082)	.498*** (.022)	-.078 (.516)	.363*** (.128)	.508*** (.038)
Private school ^a	-.006 (.064)	.393*** (.032)	.146*** (.008)	-.304 (.204)	.274** (.059)	.088** (.015)
Educational objects in home	.076* (.031)	.357*** (.016)	.058*** (.004)	.326* (.102)	.379*** (.024)	.066*** (.007)
Involvement in child's school:						
Talk with child about school	-.464*** (.048)	.393*** (.026)	.035*** (.008)	-.685*** (.152)	.353*** (.038)	.040** (.013)
						.014 (.050)

Involvement with child's school054 (.028)	.204*** (.014)	.029*** (.004)	.231* (.090)	.234*** (.021)	.026*** (.007)	-.068* (.031)	.008 (.010)
Parent-teacher organization052* (.023)	.180*** (.012)	.046*** (.004)	.050 (.065)	.187*** (.017)	.039*** (.007)	-.021 (.025)	.017 (.009)
Supervision of child's activities:								
Monitoring of child's behavior	-.927*** (.083)	.256*** (.043)	-.001 (.012)	-.547* (.258)	.355*** (.061)	-.010 (.022)	-.212* (.089)	.059 (.031)
Investment in social capital:								
Know child's friends	-.322*** (.033)	.243*** (.018)	.028*** (.005)	-.295* (.114)	-.243*** (.026)	.032*** (.008)	-.009 (.036)	-.001 (.012)
Know child's friends' parents	-.192*** (.033)	-.209*** (.018)	.043*** (.005)	-.153 (.107)	.215*** (.027)	.046*** (.009)	-.001 (.035)	-.004 (.001)
Investment in cultural capital:								
Cultural classes	-.391*** (.015)	.165*** (.081)	.026*** (.003)	.032 (.041)	.210*** (.013)	.041*** (.005)	-.105*** (.016)	-.023*** (.006)
Cultural activities	-.039 (.026)	.276*** (.013)	.041*** (.004)	.109 (.074)	.292*** (.019)	.049*** (.007)	-.021 (.028)	-.002 (.010)

NOTE.—Robust SEs are in parentheses. Models include additional controls for respondent's race/ethnicity, marital status of parents, mother's age, and number of siblings in family. Sample sizes range from 3,671 to 9,014.

* Logistic regression of dichotomous dependent variable, all others use OLS regression.

* $P < .05$ (two-tailed test).

** $P < .01$.

*** $P < .001$.

TABLE 5
UNSTANDARDIZED COEFFICIENTS FROM REGRESSIONS OF PARENTAL INVESTMENT MEASURES USING HIGH SCHOOL AND BEYOND, 1980

MEASURE OF INVESTMENT	MAIN-EFFECTS MODEL			INTERACTION-EFFECTS MODEL			
	Male	Education	Income	Male	Education	Income	Male × Education × Income
Started saving for child's education ^a	-.215* (.097)	.432*** (.047)	.245*** (.056)	-.649** (.247)	.371*** (.065)	.185* (.081)	.123 (.092)
Total money saved for college249 (.232)	.349** (.110)	.531*** (.128)	-.104 (.626)	.283 (.168)	.511** (.180)	.124 (.218)
Private school ^a	-.215* (.097)	.432*** (.047)	.245*** (.056)	-.474** (.156)	.293*** (.034)	.229*** (.045)	.090 (.047)
Educational objects in home ..	-.014 (.019)	.247*** (.008)	.258*** (.011)	-.032 (.051)	.246*** (.011)	.255*** (.015)	.003 (.016)
Talk with child	-.456*** (.022)	.178*** (.010)	.099*** (.013)	-.313*** (.055)	.199*** (.014)	.116*** (.019)	-.044* (.020)
Monitoring of child's activities	-.083*** (.013)	.047*** (.006)	.027*** (.007)	-.101** (.032)	.050*** (.008)	.020* (.010)	-.006 (.012)
							.014 (.014)

NOTE.—Robust SEs are in parentheses. Models include additional controls for respondent's race/ethnicity, marital status of parents, and number of siblings in family. Ns range from 1,016 to 20,667 (see table 2).

^a Logistic regression of dichotomous dependent variable, all others use OLS regression.

* $P < .05$ (two-tailed test).

** $P < .01$.

*** $P < .001$.

whether the child attends a private school. This is the only significant result we observed in our analyses of HSB data that supported the Trivers-Willard hypothesis. Given that we tested several models with six dependent variables and two measures of status, this lone result could be due to chance.

DISCUSSION

The study of parental investment is regularly cited as one of the areas of sociology that would profit most from an integration of sociobiological ideas (Rossi 1984; Nielsen 1994). This integration has been slowed by the frequent criticism of sociobiology as "untestable, and therefore unscientific" (Gould 1997, p. 51). Instead of dismissing the whole program on these grounds, however, we have taken the relative lack of testable theories in sociobiology as underscoring the importance of engaging those claims that can be empirically evaluated. Because sociologists have been accused of being too ideologically biased to evaluate sociobiological claims fairly, we have taken a number of precautions to make our test both as fair and as replicable as possible. We selected two publicly available and widely used data sets. We employed a broad variety of measures of parental investment that were all consistent with what has been used in previous studies (Schneider and Coleman 1993; Downey 1995; Teachman, Paasch, and Carver 1996, 1997; Carter and Wojtkiewicz 1997). We tested the hypothesis using many different model specifications and controls, and we tested for effects within different subsamples of NELS respondents. Nevertheless, our results almost uniformly fail to provide any support for the Trivers-Willard hypothesis in contemporary American society. We find little evidence that low-status parents favor daughters and high-status parents favor sons.

In the absence of the predicted interaction effects, we are left with a fairly strong set of main effects. While sociobiological explanations of these effects undoubtedly could be devised, the effects already comport well with more conventional sociological theories that focus on proximate causes. The significant effects of education and income on almost all of the variables we examined—including those not directly related to finances—are consistent with the longstanding arguments of status-attainment researchers, who maintain that the higher aspirations that middle- and upper-class parents have for their children lead them to evince greater interest than do working-class parents in the educational futures of their children (Blau and Duncan 1967; Kerchoff 1995). In recent years, status-attainment research increasingly has become interested in how the correlation between parents' and children's status is mediated by the provision of parental resources in childhood and adolescence (Teachman 1987;

Blake 1989; Downey 1995). The possible tendency for parents across all social strata to save more for the college education of their sons than daughters is consistent with the human capital argument that since the expected returns to education are higher for males than females, parents should be more willing to finance the education of their sons than that of their daughters (see Steelman and Powell 1991). At the same time, this argument is not without flaws of its own, not just because these results were observed in NELS but not HSB but also because we find that girls receive more investment than boys for our measures of parental involvement, monitoring, and social and cultural capital. Again, however, our results here were anticipated by Carter and Wojtkiewicz (1997), who speculate that parents' involvement in the education of daughters may be increasing because parents are less confident that their daughters will be able to rely on a husband for support (due to increases in the divorce rate and the number of women delaying marriage). As a consequence, although we do uncover significant effects in our data, none are new findings and all can be accommodated within existing sociological explanations; meanwhile, the novel effects predicted by the Trivers-Willard hypothesis are not found.

The failure of the Trivers-Willard hypothesis in our data may betray fundamental flaws in the application of the hypothesis to parental investment in adolescents, or it may simply indicate that the contemporary United States should be placed outside the scope conditions of the theory. The contemporary United States certainly differs in many ways from both the societies of our evolutionary past and most societies today. An important difference, discussed earlier, is the attenuation of the link between status and reproductive success.²⁵ Although critics of sociobiology have long argued that the severed link between status and fertility (*inter alia*) makes most sociobiological theories irrelevant to developed societies (e.g., Kitcher 1985), sociobiologists have countered by arguing that the advent of industrialized societies has been too novel and too recent to alter substantially the cognitive mechanisms that have evolved over thousands or millions of years. This position has allowed sociobiologists mostly to ignore the question of scope conditions for their theory, and instead sociobiologists have largely assumed that the theoretical mechanisms they postulate operate similarly across even those most diverse societies. Our results could be taken as suggesting that this assumption of cross-cultural applicability is ill founded, at least with regard to the Trivers-Willard

²⁵ Other environmental circumstances may also be cited; e.g., the relatively high degree of social mobility in contemporary American society may confuse or complicate a status-contingent mechanism like that posited by Trivers and Willard.

hypothesis, and that further consideration of the appropriate scope of the theory is warranted.

At the same time, we urge caution in interpreting our study as a test of scope conditions. We worry about the possibility that tests of sociobiological theories can be treated as "confirmations" when results support the theory and as "defining scope conditions" when the theory is contradicted. If the scope conditions of the Trivers-Willard hypothesis can only be discovered empirically, then it is unclear why the hypothesis should be considered more persuasive than whatever sociocultural explanations one could also devise post hoc. The whole reason why so many critics have considered sociobiology to be untestable is that its reasoning can be so flexible and unconstrained that few of its hypotheses seem genuinely falsifiable.²⁶ Without the ability to predict social patterns, sociobiological explanations have no decisive advantage over more traditional (and more proximately oriented) sociological explanations. What sociobiology needs is a more thorough framework for conceiving the scope of its ideas so that the question of whether a mechanism will affect behavior in a developed society can be predicted *in advance* of empirical study.

Barring this, we can note only that Trivers and Willard make clear that (a) the hypothesis is intended to apply to humans in the contemporary United States along a socioeconomic scale and (b) the hypothesis implies bias in long-term parental investment just as it implies bias in sex ratio. Subsequent research published in sociobiological journals has followed these assumptions about the hypothesis and has gone unchallenged; studies have applied the hypothesis to both the least and the most developed societies and to offspring ranging in age from infants to adult heirs. As a consequence—and given the lack of evidence supporting the hypothesis for investment in adolescents in other cultures—the evidentiary burden would now seem to fall upon those who might attribute our findings to the exceptional character of American society rather than to the more fundamental limitations of the theory.

Indeed, if our findings are correct, and socioeconomic status does not affect the treatment of sons and daughters in the manner that Trivers and Willard predict, then it questions the degree to which our understanding of why parents invest more in some children than others is augmented by reflections upon our evolutionary past. Instead, perhaps investment is best understood in terms of the proximate causes on which sociologists have concentrated: the resources possessed by parents, their education,

²⁶ With regard to the Trivers-Willard hypothesis, Sieff (1990) and Anderson and Crawford (1993) graft a number of complexities and contingencies onto the hypothesis that would seem to allow it to be reconciled with any pattern of parental investment that one might observe.

the number of other dependents extracting parental resources, and cultural norms on child rearing. Of course, our findings should not be taken as a general indictment of the sociobiological perspective, as the results here cannot speak to the many other theories that sociobiologists have offered. Nevertheless, because sociobiology has few theories with such clear and unanticipated empirical implications, the failure of the Trivers-Willard hypothesis may occasion a rethinking of where the burden of proof should reside for sociobiological ideas that either are not readily testable or have not yet received similar empirical attention. If sociobiology is to become part of mainstream social science, then it must provide theories that are empirically testable and that lead to the discovery of what Lakatos (1970) calls "new facts"—verifiable facts about the social world that would be unknown without the theory. When sociobiology provides testable hypotheses about society, it is the professional obligation of sociologists to test these hypotheses fairly and rigorously. Where our evolutionary past can be demonstrated to influence present social behaviors in a predictable fashion, the sociobiological perspective may provide sociology with an extremely powerful tool of explanation and discovery. On the other hand, if empirical tests consistently fail to support sociobiological hypotheses, then sociologists' rejection of the perspective is simply good science.

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Modeling the Relationship between the Criminal Justice and Mental Health Systems¹

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The last decade has witnessed a plethora of social control studies, ranging from imprisonment to psychiatric hospitalization. Unfortunately, research on each of these two forms tends to be isolated from the other, and research on the relationships between them is limited. In this article, the relationship between the mental health and criminal justice systems is examined. The relationship is modeled in terms of the casual processes that underlie it: processes that are common to both systems, and processes that underlie the effect of one system on another. Using a panel of cities, the article reveals strong cross-system effects and that racial composition strongly influences jail capacity. Through this effect, both jail and hospital admissions are influenced.

INTRODUCTION

Although always an integral part of sociology, the study of social control has waxed and waned. Originally, the concept was defined broadly as any structure, process, relationship, or act that contributes to the social order;

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indeed, the concepts of social order and social control were indistinguishable (Meier 1982; Gibbs 1989). A consensus is now emerging that distinguishes social control from the social order it is meant to explain and that distinguishes among social control processes. One basic distinction is among processes of internal and external control. The former refers to a process whereby people adhere to social norms because they believe in them, feeling good, self-righteous, and proud when they adhere to them and feeling bad, self-critical, and guilty when they do not. This process is sometimes termed "socialization." External control refers to a social process whereby people conform to norms or rules because they are rewarded with status, prestige, money, and freedom when they adhere to them and are punished with the loss of them when they do not. This process is sometimes termed coercive, external, or just "social control" (Meier 1982; Gibbs 1989; and Liska 1997).

Only recently have researchers studied coercive social control as a dependent variable. These studies can be categorized as either micro or macro. Using individuals as the units of analysis, microstudies examine how various control activities, such as arresting, prosecuting, and sentencing, are affected by the legal, psychological, and social characteristics of people. Using collectives as the unit of analysis, macrostudies examine how social control activities are distributed across neighborhoods, cities, states, and countries, and over time. Numerous studies examine the criminal justice system, including prison capacity, admission, and police arrest rates; the mental health system, including hospital capacity and admission rates; and the welfare system, including eligibility restrictions and payments. These systems differ in their balance of physical constraints, psychological therapy, and economic sanctions, but they all are at least partially organized to control behavior. The criminal justice system clearly emphasizes physical constraints; the mental health system emphasizes both physical constraints and psychological therapy (including medication management); and the welfare system emphasizes economic rewards/sanctions—the carrot over the stick. Each system's relative balance of control and help has long been the subject of considerable debate and empirical study (see Pivan and Cloward 1971; Scull 1977a; Foucault 1978; Garland 1990; and Liska 1997).

Most of these macrostudies focus on only one organization, policy, or program of either the criminal justice, mental health, or welfare systems. They are organized or categorized by substantive forms of control (e.g., arrests, imprisonment, hospitalization, and welfare eligibility restrictions) rather than by theoretical hypotheses. For the most part, these topical literatures are isolated from each other. Although they sometimes address similar issues, they do not build on each other or even cite each other. Researchers studying imprisonment are criminologists interested in pris-

ons, researchers studying mental hospital admissions are interested in mental health, and researchers studying welfare are interested in social services. Because macrosocial control research is balkanized into isolated pockets around diverse forms of control, theoretical issues that cut across these forms are blurred, and the boundaries that define the study of macrosocial control are diffuse and vague.

In this paper, we take a small step toward developing theoretical and empirical bridges between the two major control organizations in contemporary society: the criminal justice and mental health systems. We first review three small and somewhat isolated literatures that do examine the empirical relationships between these social control bureaucracies. Drawing on the commonalities of these literatures, we specify models of the social processes that underlie these empirical relationships. Finally, using a panel of cities over 10 years, we estimate these models.

CRIMINAL JUSTICE AND MENTAL HEALTH SYSTEMS

The relationship between the criminal justice and mental health systems is not just a theoretical issue, but it has been a "hot" policy issue for some time. There has been considerable public debate on this relationship, specifically the transferring of people from the criminal justice to the mental health system and thereby substituting treatment for punishment (medicalization of deviance) and the inappropriate incarcerating of the mentally ill in the criminal justice system (the criminalization of mental illness).

This relationship between the criminal justice and mental health systems has been addressed through three research strategies: historical studies of the emergence of the two systems, social indicator studies of the covariation between the populations and admission rates of the two systems, and survey studies of the prevalence of mental illness and criminality among the prison and mental hospital populations.

Historical Research

Social historians (Rothman 1971; Foucault 1965, 1978; Scull 1977*a*, 1977*b*; Ignatieff 1978; and Garland 1990) have examined the emergence of social control institutions in Europe and the United States during the 17th, 18th, and 19th centuries—a period that saw the rise of contemporary social control institutions, such as asylums, prisons, reformatories, orphanages, and almshouses. While agreeing that these institutions were part of a general social reform movement, social historians disagree on why they emerged. On one side of the debate, Rothman (1971) argues that as America shifted from an agricultural and rural society to an industrial and urban one, traditional forms of social control, such as the family and

religion, ceased to be effective. Social reformers sought to build institutions that would instill the discipline, obedience, and hard work of a by-gone era. While these institutions may have achieved more control than reform, that was not the intention of the reformers. On the other side of the debate, Ignatieff (1978, 1981) and Scull (1977*a*) embed the social reformers in the class structure of capitalism. Ignoring the reformer's proclaimed intentions, they explain the emergence of these control institutions as a concerted effort by elites to discipline the masses to the rhythms of capitalism.

Whatever their disagreements, social historians tend to agree that contemporary criminal justice and mental health systems emerged together as complementary responses by authorities and elites to the threat of urban disorder associated with urbanization, industrialization, and capitalism. Yet, other than hypothesizing that mental hospitals and prisons emerged during similar periods in response to similar social conditions, these studies do not explore how these control bureaucracies functioned together to control threatening populations and actions. (For an exception, see the recent work of Sutton [1990, 1991].)

Social Indicator Research

During the first half of the 20th century, psychiatrists medicalized social problems, successfully arguing that the cause of many social problems, like crime, lies in the psychological malfunctioning of people and that the solution lies in their treatment by medical specialists in treatment centers. Consequently, by the mid-1950s, the U.S. mental hospital population significantly increased to over 500,000 (Morrissey 1982), compared to the U.S. prison population of 185,000 (BJS 1986). However, since the mid-1960s, these trends have reversed. The mental hospital population decreased significantly, and the prison population increased significantly, so that the hospital population was down to 337,000 by 1970, 132,000 by 1980, and 90,000 by 1990² (NIMH 1990) and the prison population was up to 196,000 by 1970, 304,000 by 1980, and 773,000 by 1990 (Gilliard 1998; BJS 1986). These trend reversals in both populations have stimu-

² We realize that a reduction in the hospitalization of the mentally ill does not entirely reflect a contraction of the mental health system. It has, however, decreased control of this population substantially. The efficacy of drug treatments, expansion of medicaid benefits, and a shift toward community-based treatment of the mentally ill has allowed the majority of persons who otherwise would have been institutionalized to reside in the community in boarding and care homes, halfway houses, and supervised apartments. Although the net of services provided to the mentally ill has grown, the goal of providing the least restrictive setting has led to considerably less direct control of the mentally ill.

lated questions of the extent to which prisons and mental hospitals are functional social-control alternatives.

Using archival records, some researchers address this question through an examination of the temporal and cross-sectional covariation between these two forms of control. Drawing on cross-national data, Penrose (1939) originally suggested that social control systems evolve from physically controlling people (prisons) to medically treating them (asylums), thereby suggesting an inverse relationship between prison and mental hospital populations. In what is perhaps the first rigorous study of this question, Biles and Mulligan (1973) report a negative cross-sectional correlation ($-.78$) between the 1967–68 state prison population and the number of hospital beds; but, in a time-series follow-up, Grabosky (1980) reports a positive correlation ($.42$) between the size of the U.S. prison and mental hospital populations from 1930 to 1970. These bivariate studies have continued to yield inconsistent results, although they have become quite methodologically sophisticated. (See Inverarity and Grattet [1989] for a review.) While they are important in documenting the strength of the covariation between dimensions of the two systems, they do not partition that covariation into multiple underlying social processes, such as the effect of processes common to both systems and the effect of one system on another.

Survey Research

Since the mid-1970s a third strategy has emerged for examining the relationship between the criminal justice and mental health systems. This strategy examines the mechanisms through which problematic populations move from one system to another and the extent to which that movement has been stimulated by the deinstitutionalization of the mental health system (Steadman 1979; Arvanites 1988). Specifically, it is hypothesized that tightening hospital admission criteria has rerouted entry into the mental health system. Many people who do not meet the tightened criteria for involuntary and even voluntary admission enter the mental health systems via the criminal justice system. They are frequently first arrested for violating any number of laws governing public behavior. Rather than maintaining them within an increasingly overcrowded criminal justice system, they are transferred to other control systems, including the mental health system.

One set of studies examines this process by examining the criminal justice experience (such as being arrested, imprisoned, and jailed) of people admitted into mental hospitals before and after deinstitutionalization. Studies of arrest seem to support the above conclusion. For example, in an early study of New York, Melick, Steadman, and Coccozza (1979) report

that the percentage of patients with prior arrests increased from 15% in 1946–48 to 32% in 1969 and to 40% in 1975. In a study of five states, Arvanites (1988) reports that the percentage of patients with prior arrests increased from 30% to 47% between 1968 and 1978. Studies of imprisonment, however, are more inconclusive. For example, Steadman et al. (1984) report that for six states between 1968 and 1978 the percentage of prison inmates with prior mental hospitalization increased from 8% to 11%; however, reflecting the inconsistency in this literature, they also report that the increase occurred for only three states but that it was large enough to outweigh the decrease for the other three states, yielding a net increase.

If the mentally ill are overarrested but not overimprisoned, where are they going? Some researchers suggest that they are being warehoused in city and county jails. Surveys of jail populations (e.g., Swank and Winer 1976; Lamb and Grant 1982; Steadman and Dunn 1982; Pogrebin and Regoli 1985; Adler 1986; Teplin 1990; Palermo, Gumz, and Liska 1992) report that the mentally ill constitute 5%–20% of jail inmates. And many studies strongly suggest that this movement of the mentally disordered through the city-county jail system, either to be transferred to the mental health system or to remain in the justice system, was initiated by the deinstitutionalization of the mental health system (Mechanic 1989; Freeman and Roesch 1989; Warner 1989). While the evidence is far from conclusive, Arvanites (1988) concludes that when the criteria for admission to mental hospitals were strengthened, practically closing their front doors, authorities managed the less seriously mentally ill within local jails and transferred the more serious into mental hospitals through the side door via legal mechanisms, such as “incompetent to stand trial” and “not guilty by reason of insanity.”

In sum, over the years three bodies of research have emerged that address the relationship between the criminal justice system and the mental health system. Organized around disciplinary and research methods, these bodies of research coexist in splendid isolation from each other, although they address similar theoretical issues. Historical studies suggest that the criminal justice and mental health systems emerged together as complementary forms of control in response to social conditions that threatened the 17th-, 18th-, and 19th-century social order, particularly the ruling elites. Social-indicator studies examine the simple cross-sectional and temporal covariation between the structure and functioning of these two control systems. And survey studies examine the processes by which problematic populations move between these two systems.

These three bodies of research can be conceptualized as different strategies that address different aspects of the same theoretical issue. All three strategies implicitly, if not explicitly, assume a causal model where social

threat, as perceived by elites, organizational authorities, or social majorities, affects both the criminal justice and mental health systems and where these two systems affect each other. Various specific causal processes are studied. First, an increase in social threat leads to an increase in both the criminal justice and mental health systems. This is the causal process examined by the social historians in their studies of the emergence of social-control institutions during the 17th, 18th, and 19th centuries. Second, expansion in either the criminal justice or mental health system leads to a decrease in social threat and financial resources, which in turn leads to a decrease in the other system. This is the process implicitly studied by the social indicator researchers in their studies of functional alternatives. Third, contraction of one control system increases the burden on the other system and changes the balance of transfers between them, increasing the transfer flow from the contracted system to the other system. This is the causal process implied by the survey researchers in their studies of transfers between the criminal justice and mental health systems before and after deinstitutionalization of the latter.

MODELING THE RELATIONSHIP BETWEEN THE CRIMINAL JUSTICE AND MENTAL HEALTH SYSTEMS

We draw on all three literatures in modeling the causal processes that underlie the covariation between these control systems. While these literatures suggest that the covariation between the structure and functioning of the mental health and criminal justice systems reflects multiple causal processes, most contemporary research within each literature employs a methodology that simply does not allow this covariation to be partitioned among these processes. Indeed, much research examines only bivariate relationships between the two systems, such as the relationship between their capacity or admission rates. For example, historical, cross-sectional, and time-series studies of the admission rates of both systems frequently observe just the covariation between them, sometimes detrending it and only occasionally explicitly controlling for any other variables. This bivariate strategy generally interprets negative covariation as support for the functional alternatives or trade-off hypothesis and positive covariation as support for the threat hypothesis. Yet, neither hypothesis can be assessed empirically by simply observing the bivariate relationship between these forms of social control. Such an assessment requires a strategy whereby the effects of one system on another are isolated from the effects of the social processes common to both systems.

We know of only two such studies, and they report inconsistent results. Using a time series from 1948 to 1985, Inverarity and Grattet (1989) report that the unemployment rate (as an indicator of social threat) affects both

the prison and asylum population rates (as well as the welfare recipient and military personnel rates); but they report no evidence that these control organizations affect each other. On the other hand, using a pooled cross section of states from 1880 to 1923, Sutton (1991) reports that because of scarce resources, increases in some forms of control lead to decreases in others. Specifically, increases in private institutions (more welfare oriented) for controlling youth led to decreases in public institutions (more coercive) for controlling them (Sutton 1990), and decreases in almshouses led to increases in asylums (Sutton 1991). In this article, we attempt to further expand this strategy by examining the reciprocal or cross-system effects of the criminal justice and mental health systems on each other, while controlling for exogenous social processes common to both systems.

Dimensions of Control

In estimating the reciprocal effects of these control systems on each other, it is useful to distinguish two dimensions of social control organizations: the capacity for social control—such as the number of beds, cells, and personnel per capita—and the actual level of control activity—such as the number of inmates/patients and admissions per capita. Exogenous processes that affect the capacity rates of both systems may not affect their admission rates and vice versa. For example, revenue rates may affect capacity rates but not admission rates. And the impact of one system on another is more likely to be expressed through the impact of capacity rates on admission rates than the reverse. While the admission rate may influence the future capacity rate of its own system, it is unlikely to influence the future capacity rate of other systems. On the other hand, the capacity rate of either system may influence the admission rates of both systems.

Drawing on the literature, we propose two competing hypotheses on cross-system effects: the functional-alternative and the conduit hypotheses. The former argues that a substantial proportion of problematic behavior could be processed by both systems. To the extent that the capacity of one system is large, it not only increases the admission rate of its own system, but by processing a large proportion of the behavior that could be processed by both systems, it also decreases the admission rate of the other system. To the extent to which the capacity of one system is small, the marginal deviants who could be processed by either system are processed (admitted) by the other system. Hence, the functional-alternative hypothesis suggests a negative relationship between the capacity and admission rates of one system and the admission rate of the other. The conduit hypothesis suggests that if the processing capacity of one system is very large, it may serve as an intake net for both systems, keeping some

people within its system and channeling others to the other system. Hence, the conduit hypothesis suggests a positive relationship between the capacity and admission rates of one system and the admission rate of the other.

While these two hypotheses appear to be competing, they may be complementary. That is, some research suggests that the functional-alternative hypothesis may describe the effects of the mental health system on the criminal justice system and that the conduit hypothesis may describe the effects of the criminal justice system on the mental health system. Numerous researchers (Rothman 1971; Arvanites 1988) argue that the mental health system traditionally controlled (institutionalized) a wide range of nonserious deviants who might otherwise have ended up in the criminal justice system—the medicalization of deviance. Starting in the 1960s, the deinstitutionalization of the mental health system has reversed this trend, freeing a substantial number of patients, many of whom eventually have ended up in local jails (Teplin 1990; Adler 1986; Palermo et al. 1992)—the criminalization of mental illness. Hence, the functional-alternative hypothesis suggests that as the capacity of the mental health system contracts, the inmate population and admission rates of the criminal justice system (especially jails) expand, and, as the capacity of the mental health system expands, the inmate population and admission rates of the criminal justice system (especially jails) contract.

On the other hand, the criminal justice system may function as a conduit or gateway to the mental health system. People who behave inappropriately, especially on the street, are frequently contacted by police and temporarily constrained in local jails; then they are either released, further processed in the criminal justice system, or transferred informally or formally (“incompetent to stand trial” or “not guilty by reason of insanity”) to the mental health system. Studies of police decision making (Teplin 1984, 1990) show that police frequently divert problematic people to mental hospitals via arrest and pretrial jail admission. Teplin (1984) reports that the mentally ill are twice as likely as ordinary people to be arrested for the same incidents. It thus seems reasonable to argue that the extent of such cross-system processing depends on the capacity of the city/county criminal justice system (jail size) and thus that the capacity of that system positively affects the admission rates of the mental health system (see fig. 1).

The covariance between the criminal justice and mental health systems may also reflect common or correlated processes, which should be controlled in estimating the effects of the two systems on each other. We examine two categories of variables: financial resources and social threat. The criminal justice and mental health systems share common financial resources at the federal, state, county, and city levels, which could lead

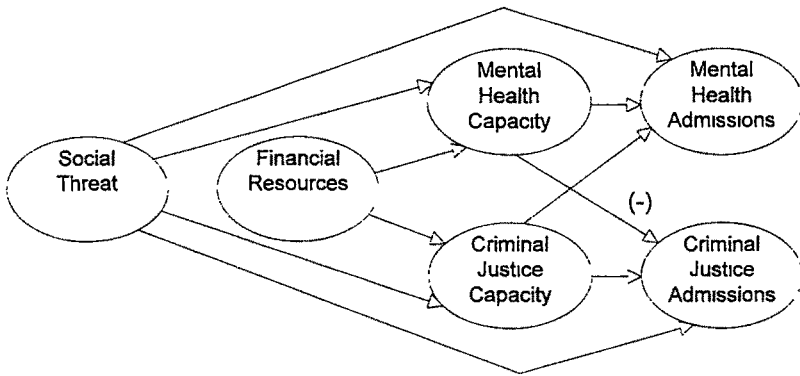


FIG. 1.—Common and cross-system causal processes. Predicted signs of all coefficients are positive, except where noted.

to a positive covariance between the capacity of both systems. Hence, financial resources should be in the analysis.

Briefly, the social threat model implies that the level of organizational control, including criminal justice and mental health systems, reflects perceived social threat to authorities (Liska 1992, 1997). Research has focused on identifying structural conditions that are likely to be perceived as threatening by both political and economic authorities. Drawing on this research tradition, we focus on two such conditions. One, economic conditions, such as the unemployment rate, have long been hypothesized to be perceived as threatening to political and economic authorities and thus to affect the level of social control expressed by both the criminal justice and mental health systems (Inverarity and Grattet 1989; Rothman 1971; Scull 1977a, 1977b; Edwards 1988; and Liska 1992). Thus we include three dimensions of economic structure: percentage unemployed, percentage below poverty, and median income. Two, racial composition, such as the percentage nonwhite, has also been hypothesized to be perceived as threatening to political and economic authorities, most of whom are white (Blalock 1967; Liska 1992), and thus to influence the level of social control expressed by both the criminal justice and mental health systems. Moreover, this relationship between the percentage of nonwhite and perceived social threat has been hypothesized to depend on the level of racial segregation (Jackson 1989), being weakest when nonwhites are geographically segregated—itsself a form of control. Thus we include three dimensions of racial composition: percentage nonwhite, percentage black, and residential segregation.

One fundamental problem with the social threat model is that it does not clearly specify how the perceived social threat of authorities influences the day-to-day decision making of criminal justice and mental health personnel that culminate in rates of social control activity. One process, however, is already implied in much macroresearch: structural factors (as mentioned above) influence the perceived threat of urban authorities, who directly influence the allocation of funds to expand or contract the capacity of control systems, and capacity in turn directly influences the level of control activity. As this process is a long-term one, we test various causal lag structures. Other than through their effects on capacity, these structural factors may influence the day-to-day admission decisions of criminal justice and mental health personnel through other social processes. We will discuss them later.

Finally, we include some structural factors (population size and density, and divorce rates) that are not necessarily structural sources of social control but that are used in macrosocial control studies and may be common structural sources of control in both systems. Population size and family disruption, for example, are frequently hypothesized to decrease informal social control and to increase the reliance on formal mechanisms of control, including the criminal justice and mental health systems.

In sum, we are attempting to estimate cross-system effects net of common sources of social control. In addition to common financial sources, we have used the social threat model to identify common structural sources of social control. We have tried to err on the side of being overly inclusive and thus perhaps have stretched the conceptual integrity of the social threat model.

Units of Analysis

One basic theoretical and methodological issue is the level at which the analysis should occur: city, county, state, federal. Both control bureaucracies are funded and administered at all four levels and are studied at all four levels. To some extent, the level of analysis should be sensitive to the levels at which the causal processes operate. Consider the common sources of covariance between the two systems: financial revenues and social threat. While sources of revenue exist at all levels, sources of threat tend to be located in urban areas, especially in central cities. Therefore, to critically test the threat hypothesis, research designs should focus on political units that most approximate urban areas and thus are most sensitive to forms of urban threat. Moreover, while bureaucracies of control may influence each other at all levels of analysis, there is considerable evidence that much, if not most, of that influence is at the city level. Stud-

ies (Arvanites 1988; Teplin 1990; Palermo et al. 1992) show that the deinstitutionalization of the mental health system influences the population of city jails, not state prisons, and that the transfer of populations from the criminal justice to the mental health system occurs from city/county jails, not state prisons, to mental hospitals. Hence, the city/county level would seem to be a meaningful level for examining the impact of the mental health and criminal justice systems on each other.

While emphasizing urban areas as the primary unit of analysis, we realize that many control agencies that respond to urban threats are funded and administered at higher levels. For example, while city police do most of the arresting, those arrested are incarcerated in city and county jails; and mental hospitals located in urban areas are funded and administered at the city, county, and state levels. The fact that different control agencies are funded and administered at different levels of analysis has seriously hampered studying the relationships between them. While focusing on cities as the primary units of analysis, we address this issue by also attaching information on county jails, county mental hospitals, and state mental hospitals to the city-level datafile.

In sum, our goal is to examine the cross-system or reciprocal effects between two critical control dimensions (capacity and admission rates) of the criminal justice system (urban jails) and the mental health system (urban mental hospitals). Drawing on the literature, we posit two general hypotheses: the functional-alternative hypothesis asserts that the capacity of mental hospitals positively affects hospital admission rates but negatively affects jail admission rates, and the conduit hypothesis asserts that the capacity of jails positively affects the admission rates of both control systems. We also examine common sources of revenue and perceived social threat that may affect the capacity and admission rates of both systems. Unless these processes are examined and controlled, estimates of reciprocal cross-system effects will be biased. The model is summarized in figure 1.

PROCEDURES

Sample

The sample is composed of a panel of approximately 100 cities with populations over 50,000. These cities, originally selected because their segregation levels had been calculated since 1940, are a good representation of large U.S. cities. For the study period, the sample contains 60% of U.S. cities with populations over 100,000 and 82% of the largest 50 cities. A few cities fall slightly under the 50,000 level for some years, and because of some missing data and a few outliers the sample varies slightly over

the period.³ It is also well distributed geographically: 23% are located in the East, 25% are located in the Midwest, 21% are located in the West, and 31% are located in the South (including the mid-Atlantic region). The study focuses on a 10-year period from 1978 to 1988 because the national jail survey was first available in 1978,⁴ and recent data from the 1990s were not yet available at the time of the analysis. This 10-year period is particularly suitable and interesting for our purposes. Both control systems were changing, one increasing and the other decreasing.

Measures

Dimensions of control.—Control activity for both the criminal justice and mental health systems is measured as admission rates, and control capacity for both systems is measured by bed, employee, and expenditure rates. We originally planned to build a three-index measurement model for both jail and mental health capacity, but the relationship between these capacity dimensions for both systems is so high (all correlations above .87) that little additional information is gained by using all three dimensions. We focus on beds per capita because it most clearly measures the aspect of capacity that constrains control activity. While we include both public and private mental hospitals, we focus on public hospitals as the primary control bureaucracy. Most admissions to public mental hospitals are involuntary, whereas most admissions to private mental hospitals are voluntary. For the mid-1980s (the middle of our study decade) about 70% of admissions to public (state, county, and city) mental hospitals were involuntary, whereas only 16% of admissions to private mental hospitals were involuntary (Center for Mental Health Services 1986; NIMH 1990). The jail data are available from a national census of jails repeated every five years (National Institute of Justice 1978, 1983, 1988), and the mental hospital data are available from the American Hospital Association (1978, 1983, 1988) guidebook.

We include both city- and county-level jails, and city-, county-, and state-level hospitals. All of the city-level capacity and control activity are

³ For 1983 and 1988, the sample varies from 96 to 102 cities, but for 1978 the sample drops to 82 cities, mainly because of missing data for %black. To estimate whether this smaller sample might have any effect on our estimates of the equations, we substituted %nonwhite (for which there is much less missing data) for %black and reestimated the equations. The estimates are very similar to those presented in the tables

⁴ The jail survey was also conducted in 1973, but the codes linking a jail to a specific city have not been well preserved so that the vast majority of jails cannot be linked to a city.

simply allocated to the respective cities. However, county- and state-level capacity and control activity are more difficult to allocate. While considering various allocation principles for the county-level jails and hospitals, we allocated to the city the portion of the county-level capacity and control equal to the proportion of city- to county-level population. For example, if the city population constitutes 75% of the county population, then 75% of the county jail capacity and control activity are assigned to the city.⁵

Many of our cities are served by state mental hospitals. Some are located within our cities/counties but serve areas beyond the city or county in which they are located. We contacted each of these hospitals (a total of 91) to determine their catchment area (counties served).⁶ Some cities are served by state hospitals that fall outside of the city/county, but the city is within their catchment area. To identify the hospitals that serve these cities and their catchment areas, we contacted the state mental health departments. We attribute to the city a portion of the capacity and control activities of these hospitals equal to the proportion of the city to catchment-area population. The control capacity and activities of these hospitals may be considerably less sensitive to urban threats and to the structure and functioning of the city/county criminal justice system. Nonetheless, we also include them, but we maintain two indexes of hospital control capacity and activity, one including these hospitals and one not.

Our cities are also served by general hospitals with and without psychiatric units. Kiesler and Sibulkin (1987) were able to construct national-level estimates of "episodes" within general hospitals for 1965–80, using the NCHS Discharge Survey. Unfortunately, comparable data on psychiatric capacity, crucial to our theoretical hypotheses, are unavailable, and the National Center for Health Statistics data cannot be disaggregated to the city level. While the general hospital data may be useful to developing counts of total psychiatric episodes and admissions, they are less relevant to our hypotheses of social control. General hospitals tend to be private, which means that their capacity and admittance rates are probably not very sensitive to perceived social threat; and they tend to admit the less serious cases for short-term care, which are least likely to be perceived as threatening. It is the public mental hospitals, the focus of our study, that

⁵ This procedure is somewhat complicated by the fact that eight of the 100 cities are located in more than one county. Four of these are primarily located in one county and pose no problem. For each of the remaining four, we use all the counties and assign to each city a portion of each county's jail capacity and control activity equal to the proportion of the city to county population.

⁶ A few hospitals that operated during our period of analysis are no longer in service and thus could not be contacted.

are likely to be sensitive to perceived social threat and to admit (control) the most serious (threatening) cases.⁷

Common sources of covariance.—We examine three general common sources of covariance: shared revenue, structural variables that influence perceived social threat, and structural variables that influence the level of informal social control.⁸ We include three sources of revenue: city, county, and state. We attribute to the city all of its own revenue, a portion of the county revenue equal to the proportion of the city to county populations, and a portion of the state revenue equal to the proportion of the city to state population. This procedure provides a measure of all sources of revenue that can support the control facilities used by the city. We compose a number of indexes: city and county revenue per capita; county and state revenue per capita; and city, county, and state revenue per capita.

We include various variables thought to be structural sources of either perceived social threat or informal social control and thus to influence both the mental health and criminal justice systems. We include multiple dimensions of arrest and crime rates: specific arrest and crime rates, indexes of property and violent crime and arrest rates, and indexes of total crime and arrest rates. While official crime and arrest counts remain flawed, they are the only available indexes of crime and arrests by city. We thus use them cautiously. We examine three dimensions of economic structure (percentage unemployed, percentage below poverty, and median income), individually and as an index with each variable weighted by its factor score. We examine two dimensions of racial structure: racial composition (percentage nonwhite/percentage of black) and segregation (index of dissimilarity). We examine two dimensions of population structure: size and density (population per square mile). And we examine one dimension of family structure (percentage of divorced). These variables are collected or constructed from information available in the Uniform Crime Reports, FBI tapes, and U.S. Census (U.S. Bureau of the Census 1990a, 1990b).

We expect the effect of the structural variables (e.g., unemployment rates) on the capacity and activity rates of these control bureaucracies to take some time. This is especially true for capacity because it takes time to build and close jails and hospitals. We thus experimented with various causal lags (1, 3, 8, and 13) in the capacity equation. In the tables, we present the estimates using three-year lags, which generally yield the strongest effects.

⁷ Some cities are also served by community care facilities. As they provide minimal social control, frequently providing no more than sleeping facilities and off-site day programs, they are not the focus of this project.

RESULTS

The results are presented in three sections. First, we compare the distributional characteristics and comparability of our urban sample to national trends. Second, in a series of models, we estimate the effects of the above structural variables on the capacity and admission rates of both systems and the effects of the capacity of one system on the admission rates of both systems. We do this for the three time periods (1978, 1983, and 1988).⁸ Third, using LISREL, we examine the extent to which the model parameters are invariant over time.

National and Urban Trends

First, we consider the comparability of our urban sample over the 10-year period to long-term national trends. This is important for various reasons. One, we wish to know how closely our urban sample reflects national trends, and, two, we are concerned that our procedures for imputing a proportion of county and state control capacity and admissions to cities in no way biases our data and model estimates.

Table 1 shows the jail capacity and admission rates (per 100,000 population) for both the complete national jail survey (National Institute of Justice 1978, 1983, 1988) and our urban sample drawn from that survey. As might be expected, our urban sample shows a capacity rate about 25% higher than the national estimate and an admission rate about 48% higher than the national estimate. As a point of comparison, the arrest rates of cities with populations larger than 50,000 is 34% higher than the nation as a whole for these same years.

Table 1 also reports the national figures for capacity and admission rates of public mental hospitals over the last 40 years. Note that capacity rates began a long-term reduction beginning about 1955, followed by a reduction of admission rates in the 1970s. Our sample of urban capacity and admission rates closely tracks these figures but, like the jail data, show higher rates. From 1978 to 1988, national capacity drops from 93 to 44 beds per 100,000 population, and admission rates drop from 153 to 106

⁸ The hospital data are collected for 1987, 1982, and 1977, and the jail data are collected for 1988, 1983, and 1978. Thus, when we include hospital capacity in a jail admission equation, we are in effect lagging hospital capacity by one year. To use comparable lags in the hospital admission equation, we interpolate the jail capacity for a one-year lag. Hence, hospital admissions for 1987 are regressed on jail capacity for 1986; hospital admissions for 1982 are regressed on jail capacity for 1981; but hospital admissions for 1977 are regressed on jail capacity for 1978 (data are not available for a linear interpolation). Since jail capacity does not vary much from one year to the next, the findings are very similar whether we use a contemporaneous or lagged measure of it.

TABLE 1

PUBLIC MENTAL HOSPITAL AND JAIL CAPACITY AND ADMISSION RATES

YEAR	PUBLIC HOSPITALS				PUBLIC JAILS			
	National Estimates		Urban Sample		National Estimates		Urban Sample	
	Capacity	Admissions	Capacity	Admissions	Capacity	Admissions	Capacity	Admissions
1950	356	100
1960	314	130
1970	213	190
1978	93*	153*	91	213	110	.	141	4,112
1980	65	144
1983	57*	130*	64	171	112	3,469	148	5,226
1988	44*	106*	53	154	139	3,962	165	5,802
1990	40	96
1993	188	5,133	.	.

NOTE—Rates given are per 100,000 population

* Estimate by linear interpolation

per 100,000 population; for our urban sample, capacity drops from 91 to 53 beds per 100,000 population, and admission rates drop from 213 to 154 per 100,000 population. In sum, for both national data and our urban sample, the criminal justice and mental health systems show inverse trends (the subject of much speculation), with the capacity and admission rates of jails increasing and the capacity and admission rates of mental hospitals decreasing.⁹

Jail Models

We discuss the model estimates by control system. First, we estimate the jail models (admission and capacity rates), starting with the capacity equations. The findings are very significant (see the top half of table 2). The percentage of blacks in the population shows strong effects in all three periods, and the percentage of divorce and segregation shows strong effects in two of the three periods (model 1). While we might expect a

⁹ The hospital and jail capacity and inmate/patient admission rates are normally distributed with a slight positive skew (a few cities with very high values). For hospital capacity, population, and admission rates, Macon, Georgia, is an outlier in every year; for hospital capacity rates, Pittsburgh, Pennsylvania, is an outlier for 1978 and 1983, and Shreveport, Louisiana, is an outlier for 1988. For jail admission rates, Little Rock, Arkansas, is an outlier for 1978 and 1983, and Tulsa, Oklahoma, is an outlier for 1988. We only drop these cities from the analysis if including them significantly alters the equation estimates.

TABLE 2

JAIL CAPACITY AND JAIL ADMISSIONS EQUATIONS

	1978				1983				1988			
	Model 1		Model 2		Model 1		Model 2		Model 1		Model 2	
	b	beta	b	beta	b	beta	b	beta	b	beta	b	beta
Jail capacity.												
Population	-1.17	-.12	-1.07	-.11	-.64	-.06	-.29	-.02	.24	.02	.58	.04
%divorce ..	15.99*	.27	16.30*	.27	10.07**	.17	12.21*	.21	6.43	.09	7.81	.12
Economic factor	-.88	-.01	2.10	.03	-7.51	-.08	-7.29	-.08	-16.43	-.16	-16.24	-.15
Revenue ..	.17	.13	.15	.11	.13	.13	.07	.07	.05	.07	.02	.02
Crime00	.01	.00	.01	.00	.08	.00	.05	-.00	-.03	-.00	-.04
Segregation ..	1.67	.18	1.75	.18	2.63*	.25	1.22	.11	3.38*	.27	2.13	.17
%black ..	2.04*	.39	2.07*	.39	1.75*	.35	2.48*	.50	2.13*	.38	2.73*	.48
%black X segregation15*	.2312*	.3110**	.23
R ² (N)309(82)		.358(82)		.236(102)		.294(102)		.189(98)		.222(98)	
Jail admissions.												
Population	-54.44	-.12	-39.41	-.08	-64.52	-.12	-61.61	-.12	-33.61	-.07	-45.08	-.09
%divorce ..	947.02*	.33	241.28	.08	499.74**	.19	172.57	.06	876.82*	.34	771.49*	.30
Economic factor ..	-285.22	-.07	-88.40	-.02	-161.22	-.04	190.17	.04	-830.82**	-.21	-410.39	-.10
Crime15	.12	.10	.09	.11	.08	.00	.00	-.03	-.03	-.01	-.01
Segregation ..	59.64	.13	23.40	.05	53.24	.11	-34.87	-.07	75.64	.16	-.62	-.00
%black ..	17.57	.07	-70.40*	-.27	23.93	.10	-44.53**	-.19	28.28	.13	-24.80	-.11
Jail capacity	36.14*	.75	35.37*	.76	23.19*	.61
Hospital capacity	-5.40	-.14	1.30	.02	4.06	.05
R ² (N)174(82)		.557(82)		.085(102)		.534(102)		.164(98)		.463(98)	

NOTE.—Population per 100,000; economic factor = sum of weighted factor scores for % unemployment, % below poverty, and median family income; jail capacity and hospital capacity per 100,000 population; crime = property and violent crime rates per 100,000 population; revenue = city and county revenue rate per 100 population.

* $P < .05$ (two-tailed test).

** $P < .05$ (one-tailed test).

TABLE 3

ESTIMATES OF %BLACK EFFECT AT THE SEGREGATION MEAN AND AT ONE SD BELOW AND ABOVE IT

	1978			1983			1988		
	SD-	Mean	SD+	SD-	Mean	SD+	SD-	Mean	SD+
b75	2.07*	3.39*	1.47*	2.49*	3.50*	1.90*	2.78*	3.66*
SE97	.83	.98	.60	.65	.86	.69	.75	1.02
Beta14	.39	.64	.29	.50	.71	.34	.49	.65

* $P < .05$ (two-tailed test)

%black effect (social threat hypothesis), its strength and consistency are socially alarming. In each period, it is very strong relative to other variables, as evidenced in the standardized coefficients, and absolutely, as evidenced by the unstandardized coefficients.¹⁰

If both %black and segregation show strong effects, the effects could be additive or interactive. A relatively large black population (20%–30%) may be more visible and thus may be perceived as even more threatening when blacks are concentrated within specific areas than when they spread out throughout the city. We thus enter a %black multiplied by segregation term in the jail capacity equation. To minimize collinearity among the multiplicative term and its components, both variables are centered at their means (Jacarrrd, Turrissi, and Wan 1990). The interactive term shows a strong effect in all three periods, increasing the R^2 from .309 to .358 in 1978, from .236 to .294 in 1983, and from .189 to .222 in 1988. To illustrate its direction and strength, we also estimate the %black effect at three levels of segregation: the mean and one standard deviation above and below it. The %black effect dramatically increases from one standard deviation below to one standard deviation above the mean (see table 3). For 1978, an increase of 1% black yields an increase of only 0.75 admissions per 100,000 population at one standard deviation below the mean (NS) but an increase of 3.39 admissions at one standard deviation above the mean. For 1983, the comparable betas are 1.47 and 3.50, and for 1988

¹⁰ In additional analyses (not in tables), we also control for region (series of dummies) and substitute arrest rates for crime rates. The results are unchanged. The effect of segregation is also moderately strong and increases (doubles) over the 10-year period. It seems that jail capacity is highest in cities where blacks compose a high percentage of the population and where they are segregated. The segregation of urban minorities into ghettos may be perceived as highly threatening to urban authorities.

they are 1.90 and 3.66. The strength of the interaction effect is also reflected in the standardized coefficients (see Jaccard et al. [1990] for calculating procedures). For 1978, the %black beta is .14 at one standard deviation below the segregation mean but .64 at one standard deviation above it, for 1983, the comparable betas are .29 and .71, and, for 1988, they are .34 and .65.¹¹

Turn now to the jail admission rate models. The analysis proceeds as follows: we first estimate a baseline model that includes all the structural variables (model 1); we then add jail capacity rates and mental hospital capacity rates (model 2). The models are estimated for 1978, 1983, and 1988 (see the bottom half of table 2). Two findings are particularly interesting.

One, a comparison of the estimates for models 1 and 2 clearly shows within- but no between-system effects. Jail capacity strongly affects jail admissions in each of the three years. Indeed, jail capacity is the dominant variable in the equation. It seems that "if we build them, we fill them." On the other hand, hospital capacity does not affect jail admissions in any year, providing no support for the functional alternative hypothesis. In this analysis, we measure capacity absolutely, that is, as the number of beds per capita (city population). We might also measure it relatively, that is, as the ratio of the institutional population to the number of beds—sometimes referred to as institutional crowding. It seems reasonable to hypothesize that as crowding increases in one system, admission rates of that system should decrease and admission rates of the other system should increase. We test this hypothesis in two ways: one, we substitute relative for absolute jail and hospital capacity and reestimate the equations in table 2, and, two, we estimate simultaneously the effects of both absolute and relative jail and hospital capacity on jail admissions. The results are clear (not presented in the table): relative capacity shows no within- or between-system effects, whereas absolute capacity continues to show a within-system but no between-system effect (as shown in the bottom half of table 2).

Two, the total effect of %black on jail admissions appears to reflect two processes: a direct effect and an indirect one. The direct effect is negative but modest. That is, while it is negative in each period, it is statisti-

¹¹ Some researchers feel that the racial composition of authorities may be just as important as the racial composition of the social unit. We collected this information for our cities and anticipated two possible effects: one, black officials may be less likely than white officials to allocate scarce local resources to social control, and, two, black officials may be less likely than white officials to view a large black population as socially threatening.

cally significant in only the first two (bottom of table 2) and can only be observed when jail capacity is controlled. In the discussion, below, we will comment on the social process that may underlie it.¹²

Although the direct effect of %black may be negative, its indirect effect is strongly positive, via the strong effect of %black on jail capacity and the strong effect of jail capacity on jail admissions. Given that the %black effect on capacity depends on segregation, we estimate these indirect effects at three levels of segregation (at the mean, one standard deviation below it, and one standard deviation above it) for each year. At the mean level of segregation, the indirect effect of %black on jail admissions, via jail capacity, is .29 in 1978, .37 in 1983, and .30 in 1988. These effects are even stronger when segregation is one standard deviation above its mean (.48, .53, and .40, respectively). There appear to be two casual processes at work: %black may directly decrease admission rates, and %black may indirectly increase admission rates, possibly via the effect of the perceived threat to authorities on capacity and its effect on admission rates. The net effect depends on the relative strengths of these two countervailing effects.

We also wish to note two other findings regarding the effects of %divorce and crime/arrest rates. The %divorcé effect is statistically significant in 1988 and in some of the models for earlier periods. Various processes might account for the effect. At the individual level, married people may have a much lower rate of pretrial admission (about one-half the total admissions). Because they may be perceived by authorities as more needed by others and as more responsible to the community, they may also be perceived as more likely to show up for their trial and thus as more suitable candidates for pretrial release. Crime and arrest rates show no effect on admissions. We estimate the models including the total crime rate index (included in table 2), violent and property crime rate indexes, and specific crime rates. We also reestimate the model substituting arrest rates for crime rates (total crime index, violent and property crime indexes, and each specific crime). We might expect that crime/arrest rates, as a source of demand, might increase jail admission rates. They simply do not.¹³

¹² We also examine the extent to which this effect is dependent on the racial composition of political authorities (white or nonwhite mayor). It is not. The racial composition of political authorities has no independent effect on the jail admission rate and does not moderate the direct impact of %black on the jail admission rate.

¹³ We also examine the effect of admission rates on future capacity. There is no effect; thus, the effect of present capacity on present admission rates is not a spurious effect of past admission rates on present capacity.

Mental Hospital Models

For mental hospitals, we only estimate admission-rate models. While we can also estimate hospital capacity models, they would be misspecified. For hospital capacity, most of the important funding decisions are made at the state level. Thus, to properly study hospital capacity, the analysis should be at the state level. This decision is confirmed by our analysis of revenue, which is collected at the city, county, and state levels. It shows that the capacity of mental hospitals is more affected by state than local revenue rates. Yet, while local conditions may have little impact on public mental hospital capacity, they may substantially impact public hospital admissions (whether they are state or locally funded).

The analysis of hospital admission rates proceeds as follows: We first estimate the full model, including just the structural variables (table 4, model 1). We next add hospital capacity and jail capacity (model 2). We finally estimate the model for just those cities that are served by public mental hospitals located within the city/county (model 3); that is, we exclude cities that are served by public mental hospitals located outside of the city/county. People are more likely to be transferred from jails to mental hospitals if the hospitals are located within the city/county. The two control systems are more likely to fall within the jurisdiction of the same courts, to be administered at the same government level (city/county), and to establish long-term working relationships. Hence, we expect the cross-system effect of jail capacity to be stronger for these cities.¹⁴

From the estimates of models 1 and 2, it is clear that only hospital capacity and jail capacity show effects on hospital admissions that are substantial and reasonably stable (see unstandardized coefficients) over time, supporting the conduit hypothesis. We next estimate the model excluding those cities where public mental hospitals are located outside of the city/county (model 3). The findings are dramatic. When limited to cities with hospitals located in the city/county (about one-half of the sample), the absolute effect (unstandardized coefficients) of jail capacity increases from .75 (model 2) to 1.25 (model 3) in 1978, from .33 to .62 in 1983, and from .18 to .55 in 1988. Equally important, the jail capacity effect (beta) is either stronger or equal to the hospital capacity effect for

¹⁴ We also exclude cities (nine) with capacity and admission rates of zero. These cities have no public city, county, or state mental hospitals listed in the dataset; they rely on general hospitals, community clinics, and private hospitals. Including them yields overestimates of the effect of hospital capacity on admissions (for these cases both variables are zero) and limits the effect of jail capacity on hospital admissions. People cannot be transferred from jails to mental hospitals if there are no mental hospitals serving the city.

TABLE 4

PUBLIC MENTAL HOSPITAL ADMISSIONS EQUATIONS

	1978						1983						1988					
	Model 1		Model 2		Model 3		Model 1		Model 2		Model 3		Model 1		Model 2		Model 3	
	b	beta	b	beta	b	beta	b	beta	b	beta	b	beta	b	beta	b	beta	b	beta
Population	-.76	-.03	.79	.03	-1.79	-.10	2.09	.11	2.82	.15	.65	.04	.32	.01	1.56	.08	-1.17	-.08
Density	-.003	-.07	-.00	-.06	.00	.16	-.009**	-.29	-.01**	-.27	-.01	-.12	-.002	-.06	-.01	-.08	.00	.11
Divorce	-25.80	-.19	-12.62	-.09	-5.75	-.03	-14.08	-.15	-3.00	-.03	-18.34	-.15	-1.15	-.01	14.59	.17	10.76	.09
Economic factor	20.63	.11	16.63	.09	-18.90	-.09	4.98	.03	-1.56	-.01	-45.52	-.28	4.46	.03	-.53	-.00	-23.31	-.13
Crime	.006	.10	.01	.12	.01	.15	-.001	-.02	.00	.01	-.00	-.04	-.04	-.13	-.00	-.05	-.01	-.01
Segregation	4.55	.21	2.75	.13	.18	.00	.18	.01	-.92	-.05	-3.14	-.16	1.64	.09	.56	.03	.86	.04
%black	.04	.00	-1.37	-.11	-.25	-.02	1.35	.17	.71	.07	3.03	.31	1.07	.14	.53	.07	1.20	.12
Hospital capacity			1.07*	.43	.73*	.29			1.11**	.42	.72*	.27			2.03**	.55	1.57**	.39
Jail capacity			.75**	.34	1.25**	.61			.33*	.20	.62**	.41			.18	.12	.55**	.39
R ² (N)	.10(85)		.358(81)		.355(43)		.08(108)		.249(101)		.314(44)		.05(102)		.298(96)		.321(44)	

NOTE.—Model 1 = estimates of the model excluding capacity variables; Model 2 = estimates of the model including all variables; Model 3 = estimates of the model for the subsample of cities with hospitals located within the city/county; population per 100,000, economic factor = sum of weighted factor scores for % unemployment, % below poverty, and median family income, jail capacity and hospital capacity per 100,000 population; crime = property and violent crime rates per 100,000 population; revenue = city and county revenue rate per 100 population.

* $P < .05$ (one-tailed test)

** $P < .05$ (two-tailed test)

all years. For 1977, the jail capacity effect (beta) is .61, while the hospital capacity effect is only .29; for 1982, the jail capacity effect is .41, while the hospital capacity effect is only .27; and for 1987 the jail and hospital capacity effects are equal (.39). It seems that when public mental hospitals are readily available (located within the city/county) jail capacity, not hospital capacity, is the best predictor of hospital admissions.

To examine the extent to which these differences between cities with and without mental hospitals located in them are statistically significant, we compare model fits (using LISREL) where the structural parameters are free to vary across these subsamples and where they are constrained to be equal across them. We estimate a series of models in which each parameter—one at a time—is constrained. These constraints yield statistically significant decreases in model fits (as indicated, e.g., by increases in χ^2/df) only when the hospital capacity parameter is constrained to be equal between the two subsamples in 1977 and when the jail capacity parameter is constrained to be equal between them for any year, thereby showing that the observed differences between the two subsamples in the cross-system effects of jail capacity on hospital admission are statistically significant.

Given the strong effect of %black on jail capacity and the strong effect of jail capacity on hospital admissions, we should expect a strong indirect effect of %black on hospital admissions via jail capacity. We should further expect the effect to be the strongest for cities with hospitals located within them (accentuating the jail capacity effect on hospital admissions) and when segregation is high (accentuating the %black effect on jail capacity). We thus estimate these indirect effects for cities with and without hospitals located in them and at three levels of segregation. As hypothesized, the indirect %black effect on hospital admissions is strongest for cities with hospitals located in them, particularly when segregation is high. For cities with hospitals located in them (one-half of the sample), and when segregation is at its mean, the indirect effects of %black on hospital admissions are .24 in 1978, .21 in 1983, and .19 in 1988, but at one standard deviation above its mean the indirect effects are .39, .29, and .25, respectively. Hence, it seems that %black via its effect on jail capacity strongly affects both jail and hospital admissions.

We also estimate these models for private hospitals. The logic of the conduit hypothesis suggests a transfer of people from public jails to public, not private, mental hospitals. If public jail capacity also affects private hospital admissions, this would raise questions about our interpretation of its effect on public hospital admissions. Supporting our theoretical hypothesis, jail capacity shows absolutely no effect on private mental hospital admission rates in any year.

Time Invariance Models

In all of the models (jail admissions, jail capacity, and hospital admissions), there is some evidence that the effects of specific variables are not invariant over time. Note that the effect of jail capacity on hospital admissions decreases over time. To establish the statistical significance of these differential effects over the three time periods, we use LISREL to estimate the equations across the years simultaneously as one multiyear model. We then compare the fits (χ^2/df) for a nested set of models: where all structural parameters are free to vary over the years, where the parameters of one variable are constrained to be equal in 1978 and 1983, where these parameters are constrained to be equal in 1983 and 1988, and where they are constrained to be equal for all three periods. We repeat this sequence for each equation and each variable. These constraints yield statistically significant increases in the χ^2/df for two variables, jail capacity and hospital capacity, in the hospital admission equation; for one variable, jail capacity, in the jail admission equation; and for no variables in the jail capacity equation. (As the jail capacity effect on hospital admissions is strongest in cities with public hospitals located within the city/county, we also reestimate the equations with the above pattern of constraints for this subsample of cities.) The results, again, show that constraining the jail capacity effect on hospital and jail admissions to be invariant over this time period yields a statistically significant increment in the χ^2/df ratio.¹⁵ This finding suggests that the conduit effect of jail capacity on both jail and hospital admission rates decreases from 1978 to 1988, with the largest decrease occurring in the last five-year period, and that these decreases are statistically significant.¹⁶

DISCUSSION

Studies of macroforms of social control are theoretically narrow. Rather than being organized around theoretical hypotheses, they are organized

¹⁵ To confirm our results, we also reverse our strategy and estimate the equations over the three time periods in a second set of nested models: where parameters of each variable are constrained to be equal over time (a baseline model) and where parameters of one variable are freed over time (repeated for each variable). We compare model fits. Does allowing parameters of any variables to be free over time yield statistically significant better fits, expressed as statistically significant decreases in the χ^2/df ? This strategy confirms the above conclusion. Only freeing the jail capacity effect over this 10-year period yields statistically significant better fits.

¹⁶ While the effects of some other variables (e.g., segregation in the jail capacity equation and %black in the jail admission equation) show interesting patterns of changes over time, these changes are not substantial enough (given the within- and between-sample variation) to be statistically significant.

more and more around specific forms of control, such as prison admission rates, police force size, mental hospital capacity and admissions rates, lynching rates, and Aid to Families with Dependent Children rates. One strategy to bridge these isolated literatures is to study the empirical relationships between different macroforms of control. We focus on the relationship between the two major contemporary control systems: criminal justice and mental health. While extant studies document the strength of this covariation, they do not examine the causal processes that underlie it, such as the effects of common or correlated social conditions on both systems and the effects of one system on another.

We further develop this research strategy in the following ways: One, the few studies that examine this covariation do so at a level where the data are readily available (state prisons and hospitals) either in a cross-sectional analysis at the state level or in a time-series analysis at the national level. However, survey research of jail, prison, and mental hospital populations suggests that most cross-system influence occurs at the local level; that is, people move from jails to hospitals and from hospitals to jails, not from prisons to hospitals and hospitals to prisons. Moreover, the literature suggests that common sources of criminal justice and mental health admissions are also likely to be found at the local level. Unfortunately, the lack of readily available criminal justice and mental health data for a sample at this level has retarded research on the interrelationship between mental health and criminal justice systems of control. We make a concerted effort to address this issue by attaching county-, catchment-, and state-level data to a national sample of cities. Two, we distinguish capacity rates from admissions rates, arguing that the causal processes that affect capacity may not affect admission rates and vice versa and that cross-system effects may best be understood and modeled as the effect of one system's capacity rate on the other's admission rate. We hypothesize two cross-system processes: a functional-alternative process—hypothesized to underlie a negative effect of hospital capacity on jail admissions—and a conduit process—hypothesized to underlie a positive effect of jail capacity on hospital admissions. Three, with a few notable exceptions, extant studies assess the functional and conduit hypotheses by examining the bivariate relationship between these control systems. A negative relationship is thought to support the functional-alternative hypothesis and a positive relationship is thought to support the conduit or some other hypothesis. We make a concerted effort to control for the effects of variables that may affect both systems and thus bias estimates of cross-system effects.

We now highlight the two most important findings. The first concerns the cross-system effects. The hypothesized negative effect of hospital capacity on jail admissions is not observed, providing no support for the

functional-alternative hypothesis; but the effect of jail capacity on hospital admissions is both positive and statistically significant for all years, providing strong support for the conduit hypothesis. As also hypothesized, it sharply increases for those cities (about one-half of the sample) with public mental hospitals located within the city/county, which facilitates the flow of people between the two systems. Consistent with our hypothesis, this effect is not evident for private mental hospitals.

Now, why does jail capacity so strongly affect hospital admission rates, while hospital capacity does not affect jail admission rates? Part of the answer may lie in the nature of the processes underlying the two hypothesized effects and the relative capacity rates of the two systems. The jail capacity effect on hospital admissions reflects the movement of people from jails to hospitals. Some of this movement occurs via official transfers, "incompetent to stand trial" or "not guilty by reason of insanity"; some of it occurs at a much earlier stage, where charges are reduced or dropped if an arrestee agrees to be hospitalized; and some of it occurs in those cases where being arrested and jailed may be the final straw in a family's decision to have a family member hospitalized, either informally or legally. In this transfer process, the large capacity of the jail system relative to that of the mental hospital system facilitates jail capacity influencing hospital admission rates. For example, the 1987 jail capacity and hospital capacity mean rates are 165 and 52, respectively, per 100,000 population, which means that jails have the capacity to admit many more people, including the mentally ill, than do mental hospitals. If only a small fraction of jail admittees are diagnosed as ill and transferred to local mental hospitals, then variation in jail capacity would substantially influence variation in hospital admissions.

On the other hand, the hypothesized effect of hospital capacity on jail admissions does not assume that people move from the mental health system to the criminal justice system. It suggests a trade-off effect. That is, as the mental health system contracts, the people "served" or controlled by it find their way into the criminal justice system, especially local jails. Thus, it predicts a negative effect of hospital capacity on jail admissions. Although the process seems plausible and has been repeatedly hypothesized, we do not observe it. Again, the ratio of hospital capacity to jail capacity may be the critical consideration. As mental health capacity decreased from the 1950s to 1975 or 1980, more and more marginal deviants may have found their way into local jails, and over this time interval hospital capacity may have influenced temporal variation in jail admissions. However, in more recent years, including the 10 years of our study, hospital capacity has fallen to a level where it may be just too small to substantially affect jail admissions, either temporally or cross-sectionally. By 1990, the public mental hospital capacity had decreased to 40 per

100,000 population, leaving a control system no longer substantial enough to have any impact on jail admissions. That is, while jails today may control significant portions of the mentally ill who were previously controlled within the mental health system, cross-sectional variation in this control may have more to do with cross-sectional variation in the capacity of the jail system than the mental health system.

Our concern then should focus on what drives jail capacity. This leads to the second major finding: the dramatic effect of %black on jail capacity, which is time invariant and increases as segregation increases. Drawing on extant research, we suspect that municipal authorities, who are frequently white, associate dangerous street crime with minorities (see Lizotte and Bordua 1980; Swigert and Farrell 1976; Tittle and Cullen 1988; Liska, Lawrence, and Sanchirico 1982; Skogan 1995). While this association may to some extent reflect contemporary urban realities, it appears to be much stronger than these realities; that is, controlling for area crime rates, %black is still related to whites' fear of crime (Liska et al. 1982; Skogan 1995) and, in this research, to jail capacity. The %black effect strengthens as blacks are spatially concentrated. It may be that when blacks are concentrated, whites have little opportunity to learn that even though the rate of violent street crime is higher for blacks than whites, the vast majority of blacks, as whites, have never committed an act of violent crime.

Given the strong effect of jail capacity on jail and hospital admission rates, %black also shows a substantial and positive indirect effect on both jail and hospital admission rates, which increases as segregation increases. Clearly, the admission rates of both hospitals and jails are indirectly driven by racial composition. Indeed, some researchers (Wagenaar 1989) report that urban mental hospitals are fast becoming jails for the poor and minorities charged or convicted of nonserious offenses.

However, upon controlling for jail capacity, %black also shows a negative direct effect on jail admission rates. Although the direct effect is not that strong, it is consistently negative in all three time periods and statistically significant in two of them. This difference in the direction of the %black indirect and direct effects may reflect a difference in the agents who make the decisions that underlie the above rates. Political authorities make decisions about police and jail capacity. To them, many of whom are white and sensitive to white constituencies, %black may be a symbol of street crime, especially dangerous street crime, that needs to be controlled. Thus, %black influences control capacity. Constrained by capacity and other resources, police make decisions about arrests and jail admissions. Liska and Chamlin (1984) have argued that as the percentage of blacks increases in a community, the ratio of black to white victims increases, and black victims (as minorities) simply have less economic and moral authority to influence the day-to-day allocation of police resources,

leading to lower arrest rates in their research and possibly to lower jail admission rates in our research.¹⁷ This negative effect is also consistent with Black's theory (1976) of the law: law varies directly with rank. He argues that bottom ranks experience the least law, especially in disputes among themselves. Applied to race and police, Black reports that "they [white officers] appear to be relatively unconcerned about how blacks treat each other and less likely to invoke the law when a black complains against a black than when a white complains against a white" (Black 1980, p. 140; see also Warner and Pierce 1993; Klinger 1996).

In conclusion, studies of social control are organized more and more around specific forms of control. In an attempt to address this state of affairs, some research examines the bivariate relationship between forms of control. While this strategy is certainly useful, it is not always theoretically fruitful, because the strength and even the direction of these relationships express multiple causal processes: effects of common processes on both systems and the effects of one system on another. We have addressed this issue, isolating cross-system effects from those of common or correlated causes. We have found considerable evidence for a conduit cross-system effect—the effect of jail capacity on hospital admissions—and considerable evidence that %black strongly affects jail capacity and, through it, both jail and hospital admissions. Unless we are prepared to allow the study of social control to further dissipate into the study of unrelated specific forms of control, we need to shift the focus of study from specific to multiple forms of control, and we must look for commonalities, not in the bivariate relationships between them, but in the social processes and structures that underlie these relationships.

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¹⁷ It is also, however, possible that %black may increase the length of sentence and pretrial commitments, thereby decreasing the capacity available for new admissions and thus the admission rate

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Reducing Uncertainty: A Formal Theory of *Organizations in Action*¹

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This article presents a formal reconstruction of James D. Thompson's classic contribution to organization theory, *Organizations in Action*. The reconstruction explicates the underlying argumentation structure for Thompson's propositions—literally, theorems or problems to be demonstrated. This allows Thompson's propositions to be derived as theorems in a deductive theory. As it turns out, the formal theory is based on general assumptions using only few primitive concepts. In addition, this theory explains why Thompson's propositions do not hold for noncomplex or "atomic" organizations (a restriction on the domain of application). Furthermore, this study reveals that organizations attempt to reduce constraints in their environment—a heretofore unknown implication of the theory.

INTRODUCTION

Thompson's *Organizations in Action* was published more than three decades ago but is still one of the classics of organization theory. The book provides a unifying perspective on open- and closed-systems thinking in organization theory that has been recognized as an important contribution in its own right (Scott 1998). The environment is a key source of uncertainty for an organization, and Thompson argued that much of organizational action can be explained by the need to reduce uncertainty. Consider a specific action such as "buffering" (e.g., building warehouses or storages),

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aiming to seal off the organization's technical or operational core from environmental uncertainty. The main lines of his arguments always are couched in explicitly formulated propositions but also are brought to life by examples such as the typologies of technologies (long-linked, mediating, or intensive), interdependencies (pooled, sequential, or reciprocal), and coordination (by standardization, by plan, or by mutual adjustment). His typologies have inspired much research in organizational design (Galbraith 1977) and contingency theory (Mintzberg 1979). However, despite the impressive number of citations, the book itself is not read much anymore—most students of organization science will only know the book through references.² Because of this, the typologies and examples of Thompson are getting more attention than his core ideas, which are captured in the propositions. The aim of this article is to bring Thompson's main ideas back into focus by presenting a logical formalization of his propositions. That is, we will formalize the propositions of *Organizations in Action* in first-order logic and reconstruct the underlying argumentation for them.

Thompson's *Organizations in Action* is a suitable candidate for a formalization attempt in organization and management theory. First, Thompson's theory is formulated using abstract concepts that transcend individual organizations and particular organization types (Zald 1996). Second, the influential role of *Organizations in Action* ensures that the thinking of quite a number of contemporary scholars is based, at least in part, on ideas presented in this famous book. As a result, the basic assumptions of *Organizations in Action* are likely to turn out to be common assumptions of several organization theories.

In this article, we give an axiomatic reconstruction of the first chapters of *Organizations in Action*. Our main focus is on the second chapter of this book. Arguably, this is the most important chapter of the book since it provides a unifying perspective on rational, closed-systems strategies in an open-systems environment. The article is structured as follows: First, we will introduce the research methodology of logical formalization; then, in the following section, we will apply this method to Thompson's *Organizations in Action*; and finally, we will review the formal theory of *Organizations in Action* and discuss issues related to our work.

RESEARCH METHODOLOGY: LOGICAL FORMALIZATION

Most textbooks on social science research methodology contain chapters on the scientific or logical foundations of social research. Typically, these

² The *Social Sciences Citation Index* lists 145 citations in 1995, 154 in 1996, and 135 in 1997 (Social Sciences Citation Index 1995, 1996, 1997).

chapters are used indirectly to provide the background context for discussing social scientific inquiry (Singleton, Straits, and Straits 1999). Logical formalization is an attempt to address such issues directly, by rationally reconstructing scientific theories and representing them in formal logic. In recent years, there has been a resurgence of interest in logical formalization as a methodology for theory building. The main efforts have been directed at organizational ecology (Hannan and Freeman 1977; Hannan and Freeman 1989), including the theory of organizational inertia (Péli et al. 1994; Kamps and Masuch 1997), life history strategies (Péli and Masuch 1997), niche width (Bruggeman 1997; Péli 1997), and age dependence (Hannan 1998). These case studies have shown the potential of logical formalization for reconstructing theories (Hannan 1997).

Logical formalization uses the classical, axiomatic-deductive notion of a theory (Popper 1959). The premises of a formal theory consist of general statements (universal laws or empirical generalizations, supplemented with definitions) of which the validity is known or assumed. The premises form the axioms or basis of the theory. We distinguish between a "definition," which introduces a name for a meaningful concept that is expressible in terms of other concepts, and an "assumption," which contains a claim that is known or assumed to hold for the theory's (intended) domain of application.³ The theorems of a formal theory are those statements that are logical consequences of the set of premises, that is, statements that are necessarily true whenever the premises are true. The theory consists of all the statements that are logical consequences of the set of premises (including the premises, because they are trivial consequences of themselves). In any exposition of a theory, there are, apart from the premises, only a small number of salient consequences singled out. We use the following labels: a "theorem," which is an interesting consequence of a theory because it is a surprising result or a new testable implication of the theory; a "lemma," which is a minor theorem—an intermediate result that is used to derive further theorems or lemmas but is of some interest in its own right; and a "corollary," which is an immediate consequence of a theorem. Theoretical explanations and predictions correspond to deductions of theorems from the set of premises (Popper 1959).

Formal logic provides a number of strict criteria for theories, such as consistency and soundness of argumentation, which are difficult (if not impossible) to impose on their discursive counterparts.⁴ The *consistency*

³ These statements are also called laws or hypotheses depending on the range of evidence for them. We prefer to use the neutral term, assumptions, here.

⁴ Tests for these criteria usually result in lengthy derivations. Fortunately, there are several computational tools available that can perform these tests automatically. Details on the use of automated theorem provers and other automated reasoning tools

of the formal theory ensures that the theory is free of contradictions. Discursive theories seldom contain conspicuous contradictions, since they are easily obscured by the ambiguity of natural language. When formalizing a theory in logic, these contradictions will surface and can be resolved. The consistency of the theory can be tested by constructing a model (roughly comparable to a case or an example) of the theory. The argumentation for a proposition is *sound* if the proposition is a logical consequence of the premises (i.e., if the proposition is a theorem). If a proposition is not a consequence of the premises, then the argumentation is unsound: the proposition is not a prediction of the theory, and the theory does not explain the proposition's claim. If a proposition is a consequence of the premises, then we know that this prediction of the theory is sound, and premises support a sound argument that explains the proposition. That is, if we consider cases in which the premises hold, then the theorem must also hold (i.e., the theorem is a prediction). Conversely, if we consider cases where the theorem holds, then the premises give an explanation for why the theorem holds. There are also criteria for falsifiability (the existence of a state of affairs that falsifies a theorem or theory) and contingency (theorems that are neither self-contradictory, nor self-contained). In addition, making the underlying argumentation structure of the theory explicit will, in turn, shed light on its explanatory and predictive power, parsimony, and coherence.⁵

THOMPSON'S ORGANIZATIONS IN ACTION

Organizations in Action provides a unifying framework for both the classical, closed-systems theories and the emerging open-systems theories of organizations. As Thompson writes, "A central purpose of this book is to identify a framework which might link at important points several of the now independent approaches to the understanding of complex organizations" (Thompson 1967, p. viii).

This framework has survived the test of time remarkably well, that is,

for formal theory building are discussed in Kamps (1998). In this article, all proofs are checked by the automated theorem prover OTTER (McCune 1994b).

⁵ There are alternative ways to investigate the logical status of theories. Sastry (1997) uses simulation to investigate completeness, consistency, and parsimony of a model based on Tushman and Romanelli (1985). The logical formalization of theories and the examination of their models are complementary approaches. If the theory's propositions are proved as theorems, like we did in this article, then we have shown that the claim holds for *all* models that satisfy the premises. Alternatively, for refuting predictions of a theory, as Sastry (1997) does, it is sufficient to show one particular model in which the premises of the theory hold but the prediction does not. Constructing a single model is also sufficient to prove the consistency of the theory.

TABLE 1
PROPOSITIONS OF THOMPSON'S CHAPTER 2

Label	Content
Proposition 2.1	Under norms of rationality, organizations seek to seal off their core technologies from environmental influences.
Proposition 2.2	Under norms of rationality, organizations seek to buffer environmental influences by surrounding their technical cores with input and output components.
Proposition 2.3	Under norms of rationality, organizations seek to smooth out input and output transactions.
Proposition 2.4	Under norms of rationality, organizations seek to anticipate and adapt to environmental changes which cannot be buffered or leveled.
Proposition 2.5	When buffering, leveling, and forecasting do not protect their technical cores from environmental fluctuations, organizations under norms of rationality resort to rationing.

NOTE —All propositions are from Thompson (1967, pp. 14–24)

the material is part of mainstream organization theory and citations are frequent. In this article, we will mainly focus on the second chapter of *Organizations in Action*. Arguably, the second chapter is the most important one because it provides the crucial link between closed-systems and open-systems strategies. The second chapter starts with one of Thompson's famous typologies, a typology of technologies: *Long-linked technologies* are serial interdependent. Several actions have to be performed in sequence. The prototypical example is an assembly line; *Mediating technologies* are concerned with linking of interdependent customers. Typical examples are commercial banks, insurance firms, and employment agencies; and *Intensive technologies* are crucially dependent upon feedback on their actions. Typical examples are hospitals, construction industry, and military combat teams (Thompson 1967, pp. 15–18).

These three types of technology are increasingly susceptible to environmental influences and are therefore decreasingly faithful approximations of closed-systems strategies. The three variations in technology are introduced merely "to illustrate the propositions we wish to develop" (Thompson 1967, p. 15). These carefully formulated propositions will be the main focus of study in this article, since they capture Thompson's unifying framework. The propositions of the second chapter are listed in table 1.

There has been some debate on the theoretical status of Thompson's propositions. As the editors of a collection of his essays write:

Unlike some sociologists, Thompson cannot be accused of being “wordy” and of using unnecessary jargon. He uses simple language and got to the point quickly. This is especially true for his major work, *Organizations in Action*. Indeed, the reader sometimes feels Thompson gets to the point too quickly; his parsimonious style sometimes leaves the reader behind. Also, although his work contains numerous propositions, all are not derived from a common set of explicit theoretical concepts and assumptions. In this sense, Thompson was not a rigorous deductive theorist. Instead, he introduced concepts and formulated propositions appropriate to the organizational realities he was examining, with little effort to show that different sets of propositions derived logically from still more general propositions, assumptions, and concepts. (Rushing and Zald 1976, pp. ix–x)

However, we observed that Thompson’s informal argumentation for the propositions suggests an underlying explanatory structure. Therefore, we decided to reconstruct this argumentation in formal logic and build a rigorous deductive theory of *Organizations in Action*. In the rest of this section, we will try to derive Thompson’s propositions as theorems in a formal, deductive theory of *Organizations in Action*.

Complex Organizations

Before we can prove the propositions, we will have to introduce a number of general concepts from Thompson’s chapter 1. We will only introduce those concepts from chapter 1 that are necessary for deriving the propositions in further sections. The main ingredient of all organization theories is, obviously, organizations. We use the predicate O for “organizations.” For example, $O(o)$ expresses that o is an organization. We also use the predicate SO for a “suborganization” of an organization. For example, $SO(o_1, o_2)$ expresses that o_2 is a suborganization of (organization) o_1 . A suborganization is a part of the organization (but not all arbitrary parts of an organization are suborganizations).

Organizations in Action is explicitly dealing with “complex organizations.” Thompson states that complex organizations are ubiquitous in modern societies and gives several examples: manufacturing firms, hospitals, schools, armies, and community agencies (Thompson 1967, p. 3). However, Thompson does not give a definition of complex organizations, nor does he dwell upon differences between complex and other, non-complex organizations. We will define complex organizations by some of their characteristics and will discuss in a separate section, below, what noncomplex organizations would look like and why, if they exist, they are exempted from Thompson’s theory.

We will use CO for complex organizations. For example, $CO(o)$ says that o is a complex organization. Thompson gives some structural charac-

teristics of complex organization. He adopts the suggestion of Parsons (1960) that organizations exhibit three distinct levels of responsibility and control: technical, managerial, and institutional. According to Thompson, "every formal organization contains a suborganization whose 'problems' are focused around effective performance of the technical function" (Thompson 1967, p. 10). We introduce the predicate TC for the technical or operational core of an organization. For example, $TC(o, tc)$ says that tc is the technical core of o . We will define complex organizations as exactly those organizations that have a technological suborganization:

DEFINITION 1.—*Complex organizations.*

$$\forall x[CO(x) \leftrightarrow O(x) \wedge \exists y[SO(x, y) \wedge TC(x, y)]].$$

Read: x is a complex organization if and only if x is an organization and there exists a y such that y is a suborganization of x and y is the technical core of x .

Thompson uses the notion of a technical core (the transformational or production process) in a general sense that applies to all three types of technologies: long-linked, mediating, and intensive technology (pp. 15–18). The core technologies of assembly lines are the processing of material and supervision of these operations. In case of mediating technologies like commercial banks, the core activities are the linking of depositors and borrowers. And in case of intensive technologies like hospitals, core activities are the performance of some specific combination of various skills, depending on the patient's state.

Definition 1 still allows complex organizations to have more than one technical core, although Thompson consistently uses the definite article "the" when referring to an organization's core technologies (p. 10). We will explicitly assume that the technical core of an organization can be uniquely determined—complex organizations can only have one technical core.

ASSUMPTION 1.—*The technical core is unique.*

$$\forall x, y, z[TC(x, y) \wedge TC(x, z) \rightarrow y = z].$$

Read: if both y and z are the technical core of x , then y is equal to z .

Assumption 1 ensures that we can talk about *the* technical core of a complex organization. This technical core is the technical suborganization of definition 1, as becomes clear from the following lemma.⁶

⁶ All derived statements are proved using the automated theorem prover OTTER (McCune 1994b). We will give here only the outline of those proofs: the first line of the proofs are by assumption, and the meta-implication symbol, \Rightarrow , indicates steps in the proof. Most steps involve *modus ponens*: if ϕ holds and ϕ implies ψ , then ψ holds as well.

LEMMA 1.—*The technical core of a complex organization is a suborganization.*

$$\forall x, y[\text{CO}(x) \wedge \text{TC}(x, y) \rightarrow \text{SO}(x, y)].$$

Read: if x is a complex organization with core technologies y , then y is a suborganization of x .

Proof.—By definition 1, a complex organization has a technical core that is a suborganization. By assumption 1, an organization can have only one technical core. Therefore, the technical core of a complex organization is a suborganization.

$$\begin{aligned} & \text{CO}(o_1) \wedge \text{TC}(o_1, tc_1) \\ \Rightarrow & (\exists tc_2) \text{SO}(o_1, tc_2) \wedge \text{TC}(o_1, tc_2) \\ \Rightarrow & tc_1 = tc_2 \\ \Rightarrow & \text{SO}(o_1, tc_1). \end{aligned}$$

Q.E.D.

Lemma 1 is a technicality that will be used in some of the other proofs. It allows us to talk about the technical core of a complex organization by ensuring that the technical core is the specific suborganization of definition 1.

The performance of their core technologies is crucial for organizations. According to Thompson (p. 11), "it would therefore be advantageous for an organization subject to criteria of rationality to remove as much uncertainty as possible from its technical core." We call this "rational evaluation" and capture this by a predicate, REVA. For example, $\text{REVA}(o, tc)$ expresses that o evaluates tc in terms of technical rationality. As stated above, we assume that core technologies are rationally evaluated:

ASSUMPTION 2.—*The technical core is rationally evaluated.*

$$\forall x, y[\text{TC}(x, y) \rightarrow \text{REVA}(x, y)].$$

Read: if y is the technical core of x , then x will rationally evaluate y .

Furthermore, if an organization rationally evaluates a particular suborganization, then it will attempt to reduce uncertainty for that suborganization. We introduce a predicate, UC, for uncertainty of a (sub)organization. For example, $\text{UC}(o, u)$ says that o has uncertainty u . We further introduce a predicate, RED, for the reduction. For example, $\text{RED}(o, u, tc)$ expresses that o attempts to reduce u for tc . We can now formulate the assumption that organizations attempt to reduce the uncertainty for suborganizations that are rationally evaluated:

ASSUMPTION 3.—*Organizations attempt to reduce uncertainty for rationally evaluated suborganizations.*

$$\forall x, y, z[\text{SO}(x, y) \wedge \text{REVA}(x, y) \wedge \text{UC}(y, z) \rightarrow \text{RED}(x, z, y)].$$

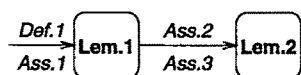


FIG. 1.—Reducing uncertainty (structure of the theory 1)

Read: if y is a suborganization of x , and x rationally evaluates y , and z is the uncertainty of y , then x attempts to reduce uncertainty z for y .

Using assumptions 2 and 3, we can now derive the following lemma for complex organizations:

LEMMA 2.—*Complex organizations attempt to reduce uncertainty for their technical cores.*

$$\forall x, y, z [\text{CO}(x) \wedge \text{TC}(x, y) \wedge \text{UC}(y, z) \rightarrow \text{RED}(x, z, y)].$$

Read: if x is a complex organization with core technologies y , and z is the uncertainty of y , then x attempts to reduce the uncertainty z for y .

Proof.—By lemma 1, the technical core is a suborganization, and by assumption 2, it is rationally evaluated by the organization. Therefore, by assumption 3, the organization attempts to reduce uncertainty for the technical core.

$$\begin{aligned} & \text{CO}(o_1) \wedge \text{TC}(o_1, tc_1) \wedge \text{UC}(tc_1, u_1) \\ \Rightarrow & \quad \text{SO}(o_1, tc_1) \\ \Rightarrow & \quad \text{REVA}(o_1, tc_1) \\ \Rightarrow & \quad \text{RED}(o_1, u_1, tc_1). \end{aligned}$$

Q.E.D.

Lemma 2 will be an important part of the argumentation in the rest of this article, because “coping with uncertainty [appears] as the essence of the administrative process” (p. 159).

In this section, we introduced the setting for Thompson’s proposition by introducing predicates for organizations, O ; suborganizations, SO ; and core technologies, TC . This led to the defined notion of complex organizations, CO (definition 1). We assumed that organizations have only one technological suborganization (assumption 1). We derived that the technical core of a complex organization is a suborganization (lemma 1; see fig. 1). We furthermore introduced predicates for uncertainty, UC ; for rational evaluation, $REVA$; and for reduction, RED . We assumed that the performance of their core technologies is crucial for organizations (assumption 2) and that an organization will attempt to remove as much uncertainty as possible from suborganizations whose performance is rationally evaluated (assumption 3). We derived that complex organizations attempt to reduce uncertainty for their technical cores (lemma 2).

Sealing Off

In this section, we will now try to prove Thompson's first proposition: "organizations seek to seal off their core technologies from environmental influences" (p. 19). Before we can give a formal version of this proposition, we have to introduce some more predicates. According to Thompson, organizations are subject to environmental influences. These environmental influences consist of both environmental fluctuations and constraints. Environmental fluctuations are dynamic, they reflect the change of market conditions (such as seasonal demand). Environmental constraints are static restrictions on the organization (such as new legislation). We introduce two predicates, FL and CS, for fluctuations and constraints, respectively. For example, $FL(tc, f, o)$ says that tc is exposed to a fluctuation f from o , and $CS(tc, c, o)$ expresses that tc is exposed to a constraint c from o . We define environmental influences, ENVI, to be the general term used for both fluctuations and constraints. For example, $ENVI(tc, i, o)$ says that tc is exposed to environmental influence i from o .

DEFINITION 2.—*Environmental influence.*

$$\forall x, y, z[ENVI(x, y, z) \leftrightarrow FL(x, y, z) \vee CS(x, y, z)].$$

Read: x is exposed to an influence y from z if and only if x is exposed to a fluctuation y from z or x is exposed to a constraint y from z .

Thompson argues that "technologies and environments are major sources of uncertainty for organizations" (p. 13). To express that environments cause uncertainty, we use a predicate, C, for causality. For example, $C(i, u)$ expresses that i causes u .⁷ Environmental influences cause uncertainty in the organization:

ASSUMPTION 4.—*Environmental influences cause uncertainty.*

$$\forall x, y, z[ENVI(x, y, z) \leftrightarrow \exists v[UC(x, v) \wedge C(y, v)]].$$

Read: if x is exposed to an environmental influence y from z , then there exists a v such that v is the uncertainty of x , and influence y causes uncertainty v .

Thompson's use of the term "sealing off" (p. 19) corresponds closely to the reduction of uncertainty that is due to an environmental influence. An organization seals a suborganization off from an environmental influence if it attempts to reduce the uncertainty caused by this influence. We define a predicate, SEFF, in precisely this way. For example, $SEFF(o, i, tc)$ expresses that o seals off tc from influence i .

⁷ Causality is a complex notion with intricate properties. In this article, we do not use any formal property of causality, although we would (at least) consider causality to be transitive (if x causes y and y causes, in turn, z , then x causes z : $\forall x, y, z[C(x, y) \wedge C(y, z) \rightarrow C(x, z)]$).

DEFINITION 3.—*Sealing off.*

$$\forall x, y, z[\text{SEFF}(x, y, z) \leftrightarrow \text{SO}(x, z) \wedge \exists v, w[\text{ENVI}(z, y, v) \wedge \text{UC}(z, w) \\ \wedge \text{C}(y, w) \wedge \text{RED}(x, w, z)]]].$$

Read: x seals z off from y if and only if z is a suborganization of x , and there exists v and w such that z is exposed to an influence y from v , and y causes uncertainty w of z , and x attempts to reduce w for z .

We can now prove the following theorem:

THEOREM 1.—*Complex organizations seal off their core technologies from environmental influences.*

$$\forall x, y, z, v[\text{CO}(x) \wedge \text{TC}(x, y) \wedge \text{ENVI}(y, z, v) \rightarrow \text{SEFF}(x, z, y)].$$

Read: if x is a complex organization, and y is the technical core of x , and y is exposed to an influence z from v , then x seals y off from z .

Proof.—By lemma 1, the technical core is a suborganization. By assumption 4, an environmental influence causes uncertainty for the technical core. By lemma 2, complex organizations attempt to reduce this uncertainty for their technical core. Therefore, by definition 3, the organization is sealing its technical suborganization off from the environmental influence.

$$\begin{aligned} & \text{CO}(o_1) \wedge \text{TC}(o_1, tc_1) \wedge \text{ENVI}(tc_1, e_1, o_2) \\ \Rightarrow & \text{SO}(o_1, tc_1) \\ \Rightarrow & (\exists u_1) \text{UC}(tc_1, u_1) \wedge \text{C}(e_1, u_1) \\ \Rightarrow & \text{RED}(o_1, u_1, tc_1) \\ \Rightarrow & \text{SEFF}(o_1, e_1, tc_1). \end{aligned}$$

Q.E.D.

This theorem is a formal version of Thompson's proposition 2.1: "Under norms of rationality, organizations seek to seal off their core technologies from environmental influences" (p. 19). The phrase "Under norms of rationality" is not explicitly mentioned in the antecedent of theorem 1. These "norms of rationality" seem to underlie all propositions and are reflected in the "rational evaluation" of assumptions 2 and 3, which, by lemma 2, play a role in the argument. The phrase "seek to" is captured by the intentional interpretation of the RED predicate: attempt to reduce. Since sealing off is defined in terms of the reduce predicate, it inherits the intentional interpretation.

We introduced predicates for environmental fluctuations, FL, and environmental constraints, CS. This led to the defined notion of environmental influence, ENVI (definition 2). We introduced a predicate for causes, C. We assumed that environmental influences cause uncertainty (assumption

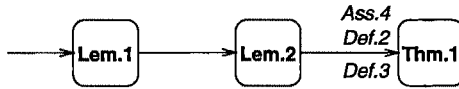


FIG. 2.—Sealing off (structure of the theory 2)

4). We defined a predicate for sealing off, SEFF (definition 3), and learned that complex organizations are sealing off their core technologies from environmental influences (theorem 1; see fig. 2).

Beyond Thompson: Atomic Organizations

Thompson does not discuss why he restricts his theory to complex organizations. In this section, we will step away from Thompson's book and try to answer questions like: What would noncomplex organizations look like? Do they exist in the real world? If so, why are they excluded from Thompson's theory? How do they fare in the real world?

We will simply define noncomplex organizations as organizations that are not complex organizations. We baptize such organizations as "atomic organizations" and define a predicate, ATO, for them: ATO(*o*) says that *o* is an atomic organization.

DEFINITION 4.—*Atomic organization.*

$$\forall x[\text{ATO}(x) \leftrightarrow \text{O}(x) \wedge \neg \text{CO}(x)].$$

Read: *x* is an atomic organization if and only if *x* is an organization and *x* is not a complex organization.

Note that we define ATO in terms of another defined predicate, CO. This is only allowed if it does not lead to circularity. In other words, we must be able to trace back the underlying primitive concepts. This can be done by unfolding the definition of complex organizations (definition 1):

LEMMA 3.—*Atomic organizations are organizations that have no suborganization as technical core.*

$$\forall x[\text{ATO}(x) \leftrightarrow \text{O}(x) \wedge \neg \exists y[\text{SO}(x, y) \wedge \text{TC}(x, y)]].$$

Read: *x* is an atomic organization if and only if *x* is an organization, and there is no *y* such that *y* is a suborganization of *x*, and *y* is the technical core of *x*.

Proof.—By definition 4 atomic organizations are organizations that are not complex organizations. Using definition 1, they are organizations that are either no organizations (which would be a contradiction), or do not have a technical suborganization.

$$\begin{aligned}
 & \text{ATO}(o_1) \\
 \Leftrightarrow & \quad O(o_1) \wedge \neg \text{CO}(o_1) \\
 \Leftrightarrow & \quad O(o_1) \wedge \neg [O(o_1) \wedge \exists y [\text{SO}(o_1, y) \wedge \text{TC}(o_1, y)]] \\
 \Leftrightarrow & \quad O(o_1) \wedge [\neg O(o_1) \vee \neg \exists y [\text{SO}(o_1, y) \wedge \text{TC}(o_1, y)]] \\
 \Leftrightarrow & \quad O(o_1) \wedge \neg \exists y [\text{SO}(o_1, y) \wedge \text{TC}(o_1, y)].
 \end{aligned}$$

Q.E.D.

Lemma 3 gives an abstract characterization of atomic organizations: atomic organizations do not have their core technologies grouped in a special suborganization—there is no clear separation between their core technologies and other activities. We can use this characterization to try to identify organizations that would be atomic in the sense of definition 4. There are some examples of organizations that correspond to this abstract characterization. Examples of atomic organizations are small organizations, especially organizations such as family firms that have only very few employees. These small organizations do not have a clear separation between technological and other activities; for example, they are managed by their owners, who are also involved in the technical operations. Although small-sized organizations have not received much attention in the literature, the majority of organizations has only a very limited number of employees, and small organizations occupy a substantial part of the market (Granovetter 1984). As Granovetter (1984, p. 333) notes, “The study of organizations is often taken to be synonymous with the study of ‘complex organizations’” and “much of what has been done in some otherwise splendid work on the sociology of economic life and complex organizations has proceeded as if the entire waterfront has been covered, when in fact work has concentrated in one important receding pool.” Together with small organizations, another example of atomic organizations is new organizations. New enterprise startups usually do not have a fully crystallized management structure; technical and management activities are all performed by the entrepreneur. New organizations involve new roles, which have to be learned (at the cost of inefficiency) (Stinchcombe 1965). Obviously, new and small organizations show considerable overlap.

Lemma 3, stating that atomic organizations do not have a technological suborganization, still leaves us two possibilities in discussing the technical core of atomic organizations. The first option is to assume that the technical core of atomic organizations does not exist at all.⁸ This option has as a result that all statements that involve the technical core do not apply to

⁸ Formally, $\forall x [\text{ATO}(x) \rightarrow \neg \exists y [\text{TC}(x, y)]]$.

atomic organizations. The second option is to argue that all organizations, including atomic ones, have certain technologies that constitute the core of their activities. Therefore, it makes sense to talk about the core technologies of any organization. If, as in the case of atomic organizations, these technological activities are not grouped together in a suborganization, we treat the entire organization as its own technical core.

Since Thompson does not discuss noncomplex organizations, we can only speculate on which option to choose. We will pursue the second option because it allows us to further investigate characteristics of atomic organizations. We can implement this option by assuming that if the technical core of an organization is not a suborganization, then it is identical with the organization itself:

ASSUMPTION 5.—*If the technical core of an organization is not a suborganization, then we treat the whole organization as its technical core.*

$$\forall x, y[O(x) \wedge TC(x, y) \wedge \neg SO(x, y) \rightarrow x = y].$$

Read: if x is an organization, and y is the technical core of x , and y is not a suborganization of x , then x and y are identical.

Assumption 5 gives us indeed the result of the second option:

LEMMA 4.—*Atomic organizations are identical with their technical core.*

$$\forall x, y[ATO(x) \wedge TC(x, y) \rightarrow x = y].$$

Read: if x is an atomic organization and y is the technical core of x , then x and y are identical.

Proof.—By lemma 3, atomic organizations do not have a technical core that is a suborganization, that is, it is either not a suborganization or not a technical core (which would be a contradiction). Therefore, the technical core of an atomic organization is not a suborganization, and by assumption 5 it follows that the technical core is identical with the atomic organization.

$$\begin{aligned} & ATO(o_1) \wedge TC(o_1, tc_1) \\ \Rightarrow & O(o_1) \wedge \neg \exists y[SO(o_1, y) \wedge TC(o_1, y)] \\ \Rightarrow & \forall y[\neg SO(o_1, y) \vee \neg TC(o_1, y)] \\ \Rightarrow & \neg SO(o_1, tc_1) \vee \neg TC(o_1, tc_1) \\ \Rightarrow & \neg SO(o_1, tc_1) \\ \Rightarrow & o_1 = tc_1. \end{aligned}$$

Q.E.D.

Lemma 4 is the companion of lemma 1, which stated that the technical

core of a complex organization is a suborganization. Lemma 4 reiterates that atomic organizations cannot differentiate between their core technologies and other activities: there is no distinction between levels of technical and managerial responsibilities and control.

Now that we have identified the technical core of atomic organizations, we can try to discuss issues relating to its uncertainty. Unfortunately, assumption 4 puts hardly any restriction on uncertainties caused by the environment. Thompson recognized that an organization may be subject to a number of different uncertainties, for example, technological uncertainty and the uncertainty due to influences (Thompson 1967, p. 1). As a result, we cannot represent the uncertainty as a function of the organization because a single organization may be exposed to different influences that cause different uncertainties for it. However, we would want to require something weaker, namely that a single environmental influence will only cause a single uncertainty:

ASSUMPTION 6.—*A single influence causes a single uncertainty.*

$$\forall x, y, z, v, w[\text{ENVI}(x, y, z) \wedge \text{UC}(x, v) \wedge \text{C}(y, v) \\ \wedge \text{UC}(x, w) \wedge \text{C}(y, w) \rightarrow v = w].$$

Read: if x is exposed to an influence y from z , and y causes uncertainty v of x , and y also causes uncertainty w of x , then uncertainty v is equal to uncertainty w .

This assumption is a restriction on assumption 4, which stated that an influence will cause uncertainty. This restriction is consistent with Thompson's text. Using assumption 6, we can derive a theorem about atomic organizations:

THEOREM 2.—*The technical core of an atomic organization faces the same environmental uncertainty as the organization.*

$$\forall x, y, z, v, w[\text{ATO}(x) \wedge \text{TC}(x, y) \\ \wedge \text{ENVI}(x, v, z) \wedge \text{UC}(x, w) \\ \wedge \text{C}(v, w) \rightarrow \exists u[\text{UC}(y, u) \\ \wedge \text{C}(v, u) \wedge u = w]].$$

Read: if x is an atomic organization with core technologies y , and if x is exposed to an influence v from z , and if v causes uncertainty w of x , then there exists uncertainty u of the technical core y , caused by v , and the uncertainty u is equal to the uncertainty w .

Proof.—By lemma 4, atomic organizations are identical with their technical cores. Therefore, the technical core faces the same environmental influence as the organization. By assumption 4, this causes uncertainty of

the technical core. Then, by assumption 6, the uncertainty of the technical core is the same as the uncertainty of the organization.

$$\begin{aligned}
 & \text{ATO}(o_1) \wedge \text{TC}(o_1, tc_1) \wedge \text{ENVI}(o_1, e_1, o_2) \wedge \text{UC}(o_1, u_1) \wedge C(e_1, u_1) \\
 \Rightarrow & \quad o_1 = tc_1 \\
 \Rightarrow & \quad \text{ENVI}(tc_1, e_1, o_2) \\
 \Rightarrow & \quad (\exists u_2) \text{UC}(tc_1, u_2) \wedge C(e_1, u_2) \\
 \Rightarrow & \quad u_2 = u_1.
 \end{aligned}$$

Q.E.D.

Theorem 1 stated that complex organizations seal off their core technologies from environmental influences. Theorem 2, in contrast, states that atomic organizations and their core technologies face the same uncertainty—noncomplex organizations cannot reduce the uncertainty for their technical core.⁹ Theorem 2 may help explain the massive failure rates of small organizations (U.S. Small Business Administration 1985) and of new enterprises—the liability of newness (Freeman, Carroll, and Hannan 1983; Brüderl, Preisendörfer, and Ziegler 1992; Hannan 1998). In the case of atomic organizations, an environmental influence causes the same uncertainty on the organizational level as it will on the technical core. As a result, atomic organizations cannot reduce uncertainty for their core technologies. Unlike complex organizations, they have no separate managerial level that can mediate between the technical core and the environment. All the environmental influences that atomic organizations face are faced at equal strength by their core technical activities. As argued before (Perrow 1986), small organizations are trivial organizations, but nevertheless they do occur in great numbers.

In this section, we investigated the possibility of noncomplex organizations. We defined a predicate, ATO, for atomic or noncomplex organizations (definition 4). Unfolding the definitions of complex (definition 1) and atomic organizations (definition 4) characterizes noncomplex organizations as those having no technical suborganization (lemma 3; see fig. 3). We assumed that if the technical core of an organization is not a suborganization, then we treat the entire organization as the technical core (assumption 5). We derived that this is the case for noncomplex organizations (lemma 4). We assumed that one environmental influence can only cause one type of uncertainty for an organization (assumption 6) and derived

⁹ Theorem 2 states that the uncertainties on the organizational and technical levels are really identical. It would not be rational of an atomic organization to attempt to reduce these uncertainties within the organization because such an attempt must fail.

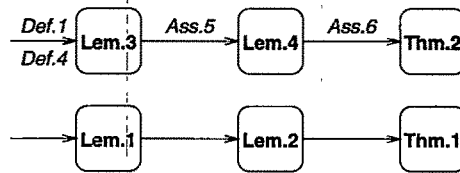


FIG. 3.—Atomic organizations (structure of the theory 3)

that atomic organizations cannot reduce the uncertainty caused by environmental influences (theorem 2). This indicates that Thompson's restriction to complex organizations is not an arbitrary one: his propositions do not hold for noncomplex organizations.

Buffering and Anticipating

After the intermezzo of the previous section, we will continue with our formalization of Thompson's arguments. In this section, we try to prove a formal version of Thompson's second proposition: "Organizations seek to buffer environmental influences" (Thompson 1967, p. 20) and his fourth proposition: "organizations seek to anticipate and adapt to environmental changes" (p. 21).

Theorem 1, above, stated that complex organizations attempt to seal off their core technologies from environmental influences. This suggests that complex organizations have some sort of control over influences directed at their (technical) suborganizations. We introduce a predicate, HC, for "having control." For example, $HC(o, i)$ says that o has control over i . We assume that organizations have some control over environmental influences directed at their suborganizations:

ASSUMPTION 7.—*Organizations have control over environmental influences on their suborganization.*

$$\forall x, y, z, v [O(x) \wedge SO(x, y) \wedge ENVI(y, z, v) \rightarrow HC(x, z)].$$

Read: if x is an organization, and y is a suborganization of x , and y is exposed to an influence z from v , then x has control over z .

The idea is that organizations mediate between the environment and suborganizations like the technical core (Thompson 1967, p. 11). Note an organization does not necessarily have *complete* control over such an environmental influence; it just means that the organization can undertake some actions that will reduce the influence (but which may not eliminate it completely). In the rest of this section, we will discuss some concrete actions for reducing uncertainty.

Next, we assume that if organizations attempt to reduce something (such as uncertainty), and they have some control over one of its causes, then they will also attempt to reduce this cause:

ASSUMPTION 8.—*If an organization attempts to reduce something, and has control over a cause of it, the organization will attempt to reduce the cause.*

$$\forall x, y, z, v[\text{RED}(x, y, z) \wedge C(v, y) \wedge \text{HC}(x, v) \rightarrow \text{RED}(x, v, z)].$$

Read: if x attempts to reduce y for z , and v causes y , and x has control over v , then x attempts to reduce v for z .

This assumption presupposes the organizational rationality that a reduction of the cause will result in a reduction of the effect. As Thompson writes, "Instrumental action is rooted on the one hand in desired outcomes and on the other hand in beliefs about cause/effect relationships" (p. 14). We can now derive the following theorem:

THEOREM 3.—*Complex organizations attempt to reduce environmental influences for their core technologies.*

$$\forall x, y, z, v[\text{CO}(x) \wedge \text{TC}(x, y) \wedge \text{ENVI}(y, z, v) \rightarrow \text{RED}(x, z, y)].$$

Read: if x is a complex organization, and y is the technical core of x , and y is exposed to an influence z from v , then x attempts to reduce z for y .

Proof.—By definition 1, complex organizations are organizations, and by lemma 1 the technical core is a suborganization. By assumption 4, an environmental influence causes uncertainty for the technical core. By lemma 2, complex organizations attempt to reduce uncertainty for their technical core. Thus, by assumption 7, organizations have control over the influences on their suborganizations. Therefore, by assumption 8, organizations attempt to reduce this environmental influence for their technical core.

$$\begin{aligned} & \text{CO}(o_1) \wedge \text{TC}(o_1, tc_1) \wedge \text{ENVI}(tc_1, e_1, o_2) \\ \Rightarrow & \quad \text{O}(o_1) \\ \Rightarrow & \quad \text{SO}(o_1, tc_1) \\ \Rightarrow & \quad (\exists u_1) \text{UC}(tc_1, u_1) \wedge C(e_1, u_1) \\ \Rightarrow & \quad \text{RED}(o_1, u_1, tc_1) \\ \Rightarrow & \quad \text{HC}(o_1, e_1) \\ \Rightarrow & \quad \text{RED}(o_1, e_1, tc_1). \end{aligned}$$

Q.E.D.

The impact of this theorem becomes more clear in its specific predictions for the two types of environmental influences: fluctuations and constraints. The reduction of environmental fluctuations by an organization

is called "buffering" in Thompson: "buffering absorbs environmental fluctuations" (p. 21). We define a predicate, BUF, for buffering. For example, $\text{BUF}(o, f, tc)$ says that o is buffering fluctuation f for tc .

DEFINITION 5.—*Buffering.*

$$\forall x, y, z[\text{BUF}(x, y, z) \leftrightarrow \text{SO}(x, z) \wedge \text{FL}(z, y, x) \wedge \text{RED}(x, y, z)].$$

Read: x buffers y for z if and only if z is a suborganization of x , and z is exposed to a fluctuation y from x , and x attempts to reduce y for z .

Typical examples of buffering are the stockpiling of materials and supplies and the maintaining of warehouse inventories (p. 20). We now have the following corollary:

COROLLARY 1 (of theorem 3).—*Complex organizations buffer environmental fluctuations for their core technologies.*

$$\forall x, y, z[\text{CO}(x) \wedge \text{TC}(x, y) \wedge \text{FL}(y, z, x) \rightarrow \text{BUF}(x, z, y)].$$

Read: if x is a complex organization, and y is the technical core of x , and y is exposed to a fluctuation z from x , then x buffers z for y .

Proof.—Using theorem 3, lemma 1, and definitions 2 and 5:

$$\begin{aligned} & \text{CO}(o_1) \wedge \text{TC}(o_1, tc_1) \wedge \text{FL}(tc_1, e_1, o_1) \\ \Rightarrow & \text{SO}(o_1, tc_1) \\ \Rightarrow & \text{ENVI}(tc_1, e_1, o_1) \\ \Rightarrow & \text{RED}(o_1, e_1, tc_1) \\ \Rightarrow & \text{BUF}(o_1, e_1, tc_1). \end{aligned}$$

Q.E.D.

This corollary is a formal version of Thompson's proposition 2.2: "Under norms of rationality, organizations seek to buffer environmental influences by surrounding their technical cores with input and output components" (p. 20).

The theorem also makes a specific prediction in case of environmental constraints. The reduction of environmental constraints by an organization is called "anticipating and adapting" or "forecasting" in Thompson: "To the extent that environmental fluctuations can be anticipated, however, they can be treated as *constraints* on the technical core" (p. 22; emphasis in original). We define a predicate, ANA, for anticipating and adapting. For example, $\text{ANA}(o, c, tc)$ says that o is anticipating and adapting to constraint c for tc .

DEFINITION 6.—*Anticipating and adapting.*

$$\forall x, y, z[\text{ANA}(x, y, z) \leftrightarrow \text{SO}(x, z) \wedge \text{CS}(z, y, x) \wedge \text{RED}(x, y, z)].$$

Read: x anticipates and adapts to y for z if and only if z is a suborganiza-

tion of x , and z is exposed to a constraint y from x , and x attempts to reduce y for z .

Anticipation and adaptation typically involves the reallocation of resources according to the forecasted market demand or supply constraints. This gives rise to another corollary:

COROLLARY 2 (of theorem 3).—*Complex organizations anticipate and adapt to an environmental constraint for their core technologies.*

$$\forall x, y, z[\text{CO}(x) \wedge \text{TC}(x, y) \wedge \text{CS}(y, z, x) \rightarrow \text{ANA}(x, z, y)].$$

Read: if x is a complex organization, and y is the technical core of x , and y is exposed to a constraint z from x , then x anticipates and adapts to z for y .

Proof.—Using theorem 3, lemma 1, and definitions 2 and 6.

$$\begin{aligned} & \text{CO}(o_1) \wedge \text{TC}(o_1, tc_1) \wedge \text{CS}(tc_1, e_1, o_1) \\ \Rightarrow & \text{SO}(o_1, tc_1) \\ \Rightarrow & \text{ENVI}(tc_1, e_1, o_1) \\ \Rightarrow & \text{RED}(o_1, e_1, tc_1) \\ \Rightarrow & \text{ANA}(o_1, e_1, tc_1). \end{aligned}$$

Q.E.D.

This corollary is a formal version of Thompson's proposition 2.4: "Under norms of rationality, organizations seek to anticipate and adapt to environmental changes which cannot be buffered or leveled" (p. 21).

We have introduced a predicate for "having control," HC, and assumed that organizations have (some) control over environmental influences on their suborganizations (assumption 7). We further assumed that if organizations have control over the cause of something they want to reduce, they will attempt to reduce this cause (assumption 8), and we derived that complex organizations attempt to reduce environmental influences for their technical cores (theorem 3; see fig. 4). We defined buffering as the reduction of fluctuations, BUF (definition 5), defined anticipating and adapting as the reduction of constraints, ANA (definition 6), and derived that complex organizations will buffer environmental fluctuations (corollary 1) and anticipate and adapt to environmental constraints (corollary 2).

Smoothing or Leveling

In this section, we attempt to derive a formal version of Thompson's third proposition: "organizations seek to smooth out input and output transactions" (p. 21). According to Thompson, organizations also attempt to re-

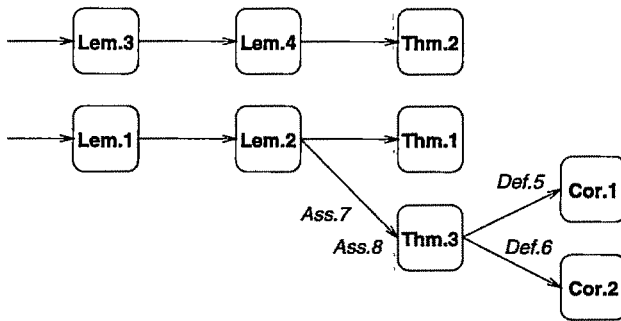


FIG. 4.—Buffering and anticipating (structure of the theory 4)

duce fluctuations in the environment (p. 21). Apparently, organizations have some control over specific elements of their environment. We define a predicate, CEE, for controlled elements in the environment. For example, $CEE(o_1, o_2)$ says that o_2 is an element in the environment of o_1 over which o_1 has some control:¹⁰

DEFINITION 7.—*Controlled environmental element.*

$$\forall x, y [CEE(x, y) \leftrightarrow O(x) \wedge \forall z [ENVI(x, z, y) \rightarrow HC(x, z)]].$$

Read: y is an element in x 's controlled environment if and only if x is an organization, and for all z such that x is exposed to an influence z from y it is the case that x has control over z .

This definition, again, does not imply that organizations have unilateral control over other elements in their environment, the amount of control may be limited. Using this definition, we can now derive:

THEOREM 4.—*Complex organizations attempt to reduce environmental influences in their controlled environment for their technical core.*

$$\forall x, y, z, v, w [CO(x) \wedge TC(x, y) \wedge ENVI(y, z, x) \wedge CEE(x, v) \\ \wedge ENVI(x, w, v) \wedge C(w, z) \rightarrow RED(x, w, y)].$$

Read: if x is a complex organization, and y is the technical core of x , and y is exposed to an influence z from x , and v is in the controlled environment of x , and x is exposed to an influence w from v , and w causes z , then x attempts to reduce w for y .

Proof.—By theorem 3, complex organizations reduce environmental in-

¹⁰ We wish to thank the reviewer who pointed out a deficiency in an earlier version of this definition.

fluences for their technical core. If this environmental influence is caused by another environmental influence on an element of the controlled environment of the organization, then, by definition 7, the organization has some control over the second influence. Therefore, by assumption 8 the organization will attempt to reduce the second influence for the organization.

$$\begin{aligned}
 & \text{CO}(o_1) \wedge \text{TC}(o_1, tc_1) \wedge \text{ENVI}(tc_1, e_1, o_1) \wedge \text{CEE}(o_1, o_2) \\
 & \quad \wedge \text{ENVI}(o_1, e_2, o_2) \wedge C(e_2, e_1) \\
 \Rightarrow & \quad \text{RED}(o_1, e_1, tc_1) \\
 \Rightarrow & \quad \text{HC}(o_1, e_2) \\
 \Rightarrow & \quad \text{RED}(o_1, e_2, tc_1).
 \end{aligned}$$

Q.E.D.

We will investigate the impact of this theorem by considering the specific predictions it makes for environmental fluctuations. Thompson uses the term "smoothing" for the reduction of fluctuations in the environment: "smoothing or leveling involves attempts to reduce fluctuations in the environment" (p. 21). We define a predicate, SM, for smoothing. For example, $\text{SM}(o, f, tc)$ says that o smoothes f for tc :

DEFINITION 8.—*Smoothing.*

$$\forall x, y, z [\text{SM}(x, y, z) \leftrightarrow \text{SO}(x, z) \wedge \exists v [\text{FL}(x, y, v) \wedge \text{RED}(x, y, z)]].$$

Read: x smoothes y for z if and only if z is a suborganization of x , and there exists a v such that x is exposed to a fluctuation y from v , and x attempts to reduce y for z .

A typical example of smoothing is price mechanisms: by charging premiums during peak periods and inducements during slow periods (p. 21). Note the difference between buffering and smoothing: buffering concerns the reduction of fluctuations within an organization, whereas smoothing concerns the reduction of fluctuation in the environment. We now have the following corollary:

COROLLARY 3 (of theorem 4).—*Complex organizations smooth environmental fluctuations in their controlled environment for their technical core.*

$$\begin{aligned}
 & \forall x, y, z, v, w [\text{CO}(x) \wedge \text{TC}(x, y) \wedge \text{FL}(y, z, x) \wedge \text{CEE}(x, v) \\
 & \quad \wedge \text{FL}(x, w, v) \wedge C(w, z) \rightarrow \text{SM}(x, w, y)].
 \end{aligned}$$

Read: if x is a complex organization, and y is the technical core of x , and y is exposed to a fluctuation z from x , and v is in the controlled environment of x , and x is exposed to a fluctuation w from v , and w causes z , then x smoothes w for y .

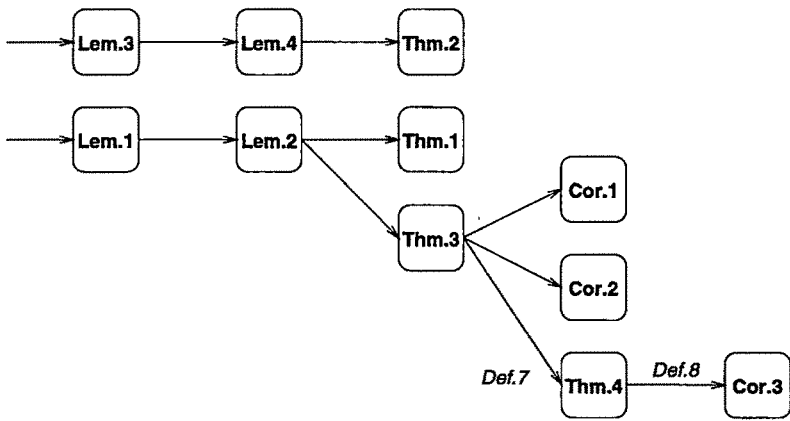


FIG. 5.—Smoothing (structure of the theory 5)

Proof.—Using theorem 4, lemma 1, and definitions 2 and 8.

$$\begin{aligned} &CO(o_1) \wedge TC(o_1, tc_1) \wedge FL(tc_1, e_1, o_1) \wedge CEE(o_1, o_2) \\ &\quad \wedge FL(o_1, e_2, o_2) \wedge C(e_2, e_1) \\ \Rightarrow &ENVI(tc_1, e_1, o_1) \wedge ENVI(o_1, e_2, o_2) \\ \Rightarrow &RED(o_1, e_2, tc_1) \\ \Rightarrow &SO(o_1, tc_1) \\ \Rightarrow &SM(o_1, e_2, tc_1). \end{aligned}$$

Q.E.D.

This corollary is a formal version of proposition 2.3 in Thompson: “Under norms of rationality, organizations seek to smooth out input and output transactions” (p. 21).

In the above section, we have defined predicates for “controlled environmental element,” CEE (definition 7), and for smoothing, SM (definition 8). We derived that complex organizations attempt to reduce environmental influences in their controlled environment (theorem 4; see fig. 5). In particular, we derived that complex organizations attempt to smooth out fluctuations in their controlled environment (corollary 3).

Beyond Thompson: Negotiating

After proving a corollary on the reduction of fluctuations in the environment (corollary 3), we come across an interesting question: What about the reduction of constraints in the environment? Thompson discusses

TABLE 2
REDUCTION OF CONSTRAINTS IN THE ENVIRONMENT?

	In the Organization	In the Environment
Fluctuations	Buffering (Cor. 1)	Smoothing (Cor. 3)
Constraints	Anticipating and adapting (Cor. 2)	...

propositions for both the reduction of fluctuations and the reduction of constraints in the organization but only discusses one proposition for the reduction of fluctuations in the environment (see table 2).

Can complex organizations, analogous to the reduction of fluctuations, also reduce constraints in the environment? The general theorem about the reduction of influences in the environment, theorem 4, suggests that this is the case. Since there is no corresponding proposition in Thompson, we will define a new concept that treats the reduction of constraints in the environment. This term is related to the anticipation and adaptation of definition 6. The difference is that in case of anticipating and adapting the reduction of constraints takes place within the organization, whereas here the reduction of constraints takes place in the environment. For want of a better term, we call the reduction of constraints in the environment "negotiating" and define a predicate, NEG, for it. For example, $NEG(o, c, tc)$ says that o negotiates c for tc :

DEFINITION 9.—*Negotiating.*

$$\forall x, y, z[NEG(x, y, z) \leftrightarrow SO(x, z) \wedge \exists v[CS(x, y, v) \wedge RED(x, y, z)]]$$

Read: x negotiates y for z if and only if z is a suborganization of x , and there exists a v such that x is exposed to a constraint y from v , and x attempts to reduce y for z .

The definition of negotiating does not restrict the domain of the theory, it only defines NEG as shorthand for the reduction of constraints in the environment, allowing us to formulate statements more concisely.

The corollaries about buffering, smoothing, and anticipating and adapting are claims of Thompson (propositions 2.2, 2.3, and 2.4, respectively). A corollary about negotiating is not mentioned in Thompson but would complete the four logical possibilities to reduce fluctuations and constraints within organizations and within the environment (see table 2). Using the same set of assumptions used to derive versions of the other propositions, we can derive the following corollary:

COROLLARY 4 (of theorem 4).—*Complex organizations negotiate environmental constraints in their controlled environment for their technical core.*

$$\forall x, y, z, v, w [\text{CO}(x) \wedge \text{TC}(x, y) \wedge \text{CS}(y, z, x) \wedge \text{CEE}(x, v) \\ \wedge \text{CS}(x, w, v) \wedge C(w, z) \rightarrow \text{NEG}(x, w, y)].$$

Read: if x is a complex organization, and y is the technical core of x , and y is exposed to a constraint z from x , and v is in the controlled environment of x , and x is exposed to a constraint w from v , and w causes z , then x negotiates w for y .

Proof.—Using theorem 4, lemma 1, and definitions 2 and 9.

$$\begin{aligned} & \text{CO}(o_1) \wedge \text{TC}(o_1, tc_1) \wedge \text{CS}(tc_1, e_1, o_1) \wedge \text{CEE}(o_1, o_2) \\ & \quad \wedge \text{CS}(o_1, e_2, o_2) \wedge C(e_2, e_1) \\ \Rightarrow & \quad \text{ENVI}(tc_1, e_1, o_1) \wedge \text{ENVI}(o_1, e_2, o_2) \\ \Rightarrow & \quad \text{RED}(o_1, e_2, tc_1) \\ \Rightarrow & \quad \text{SO}(o_1, tc_1) \\ \Rightarrow & \quad \text{NEG}(o_1, e_2, tc_1). \end{aligned}$$

Q.E.D.

Although there is no corresponding proposition in Thompson (1967), this corollary follows from exactly the same assumptions that we used to derive the other theorems. Negotiation is a hitherto unknown implication of the theory: the theory predicts that organizations negotiate constraints in their environment.

The discovery of a new prediction of the theory gives us a new possibility for the empirical testing of the theory. If the empirical evidence supports the prediction of the theory, our confidence in the theory is strengthened. If, on the other hand, the prediction does not conform to the empirical evidence prediction, we have falsified the theory. That is, we have at least falsified our formal reconstruction of it. We will discuss the new prediction in the light of some recent findings reported in the literature.

Can we be more specific about what “negotiating” in the sense of corollary 4 means? Negotiating is defined as attempts to reduce constraints in the environment. As a result, the reduction of constraints in the environment has an effect for all organizations that are subject to this constraint. All these organizations share the benefits of the reduction and therefore have a collective interest in reducing the constraint in the environment. For the prototypical example of a constraint, new legislation, this would mean that organizations will attempt to reduce the effects of legislation. We actually find support for such a claim in recent empirical findings on legalization (Edelman 1992; Sutton et al. 1994; Sutton and Dobbin 1996). These studies investigate the introduction of equal employment opportunity and affirmative action (EEO/AA) laws. Their main finding is that

organizations can collectively mediate the impact of these laws. "Such laws set in motion a process of definition during which organizations test and collectively construct the form and boundaries of compliance in a way that meets legal demands yet preserves managerial interests" (Edelman 1992, p. 1532). This presents a clear case in which organizations successfully reduce environmental uncertainty by what we termed negotiating. Although negotiating is an implicit consequence of Thompson, it is not an unknown topic in organization theory. Finding this new prediction increases our confidence in Thompson's theory and in our formal interpretation of it.

Note that negotiating is defined as the action of individual organizations. However, consider what happens if more than one organization is facing the same environmental constraint. In that case, corollary 4 predicts that all these organizations will attempt to reduce this constraint in the environment.¹¹ The NEG predicate captures the rationality of individual organizations to engage in a (collective) attempt to reduce a constraint in the environment. Of course, whether such an attempt is successful may very well depend on the proportion of organizations participating in these attempts.

In this section, we defined NEG, a predicate for negotiating (definition 9). Negotiating is not mentioned in Thompson but can be defined in analogy with smoothing (definition 8). A corollary on negotiation (corollary 4; see fig. 6) can be derived from the same set of assumptions used for deriving the other theorems. This concludes our formal theory of *Organizations in Action*. We derived formal versions of four of the five propositions in chapter 2 from general axioms of organization theory.

DISCUSSION AND CONCLUSIONS

We review the formal theory of *Organizations in Action* by the criteria introduced earlier in the second section of the article: consistency, soundness, parsimony, coherence, and explanatory and predictive power.

¹¹ For example, we can derive for the case of two organizations subject to the same constraint:

$$\begin{aligned} \forall x_1, x_2, y_1, y_2, z_1, z_2, y, w [& \text{CO}(x_1) \wedge \text{TC}(x_1, y_1) \wedge \text{CS}(y_1, z_1, x_1) \\ & \wedge \text{CEE}(x_1, v) \wedge \text{CS}(x_1, w, v) \\ & \wedge \text{C}(w, z_1) \wedge \text{CO}(x_2) \wedge \text{TC}(x_2, y_2) \\ & \wedge \text{CS}(y_2, z_2, x_2) \wedge \text{CEE}(x_2, v) \\ & \wedge \text{CS}(x_2, w, v) \wedge \text{C}(w, z_2) \\ & \rightarrow \text{NEG}(x_1, w, y_1) \wedge \text{NEG}(x_2, w, y_2)] \end{aligned}$$

by two applications of corollary 4, and so on.

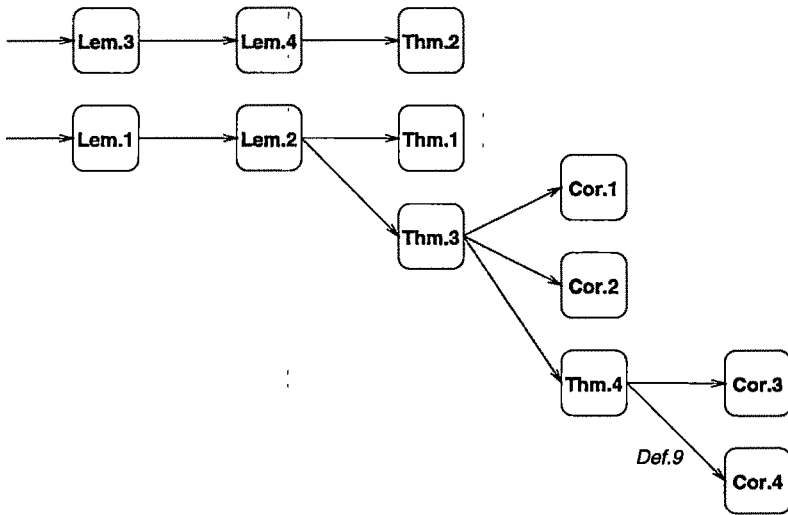


FIG. 6.—Negotiating (structure of the theory 6)

Consistency and soundness.—The main benefit of formal theories is that they provide clarity, both about the theory's propositions and about its argumentation structure. We have disambiguated the theory's natural language formulation by coding it in a formal language. The formal theory contains explicit definitions of complex organizations, sealing off, buffering, anticipating and adapting, and smoothing. Apart from giving a formal and precise formulation of the propositions, we also managed to trace back the argumentation for these propositions by finding reasonable and sufficient underlying assumptions. The formal theory is consistent.¹² All lemma's, theorems, and corollaries in the previous section are sound consequences of the premises. The proofs give sound explanations for the claims and show that the claims are unavoidable consequences of the premises.

Parsimony and coherence.—In the formal theory of *Organizations in Action*, we used the predicates listed in table 3. The theory is parsimonious

¹² A formal theory is consistent if it has a model. There are computer programs currently available that generate models of a formal theory. Formally, such a model is an interpretation function that assigns objects of the domain to the constants, functions, and predicates of the theory. We used MACE (Models and Counter-Examples; McCune 1994a) to generate such a model (and thereby proved the consistency of the theory). Technical details are available upon request from the authors.

TABLE 3
NOTATION USED IN THE FORMAL THEORY

Predicate	Meaning
Primitive:	
$O(o)$	o is an organization
$SO(o, so)$	so is a suborganization of o
$TC(o, tc)$	tc is the technical core of o
$UC(o, u)$	o has uncertainty u
$REVA(o, tc)$	o evaluates tc in terms of technical rationality
$RED(o, i, tc)$	o attempts to reduce i for tc
$FL(tc, f, o)$	tc is exposed to a fluctuation f from o
$CS(tc, c, o)$	tc is exposed to a constraint c from o
$C(i, u)$	i causes u
$HC(o, i)$	o has control over i
Defined:	
$CO(o)$	o is a complex organization
$ENVI(tc, i, o)$	tc is exposed to an influence i from o
$SEFF(o, i, tc)$	o seals off tc from i
$ATO(o)$	o is an atomic organization
$BUF(o, f, tc)$	o buffers f for tc
$ANA(o, c, tc)$	o anticipates and adapts to c for tc
$CEE(o_1, o_2)$	o_2 is in o_1 's controlled environment
$SM(o, f, tc)$	o smoothes f for tc
$NEG(o, c, tc)$	o negotiates c for tc

because it uses only 10 primitive predicates or concepts. In a formal theory, there is a clear distinction between primitive concepts and concepts that are defined in terms of the primitive concepts. A defined concept can be eliminated from the theory by replacing the concept with the expression it stands for. Note that we may only use defined concepts in the definition of other concepts if this does not lead to circularity.¹³ The nine defined predicates do not affect the theory's parsimony because they can be substituted for by their definiens (and the same theorems would still be derivable). The 10 primitive predicates are general notions of organization theory (see table 3).

The assumptions characterize the application domain of the theory: the theory applies to all domains that satisfy the assumptions.¹⁴ Our recon-

¹³ It is impossible to define all the concepts of a theory: if all concepts were defined in terms of the other concepts, then some of the definitions have to be circular.

¹⁴ The definitions are only important for determining the meaning of the defined predicates used in the assumptions (in our case only definition 2, the definition of environmental influences).

struction revealed that Thompson's argumentation is largely based on fairly basic assumptions of organization theory. We used only eight assumptions, and two of them (assumptions 5 and 6) are only used for our discussion of noncomplex organizations. The underlying assumptions are general assumptions about organizations. As a result, the formal theory of *Organizations in Action* is a general theory, and the axiomatic structure of the theory facilitates further extension.

The argumentation structure of the theory was shown in figure 6 in the previous section. The theory consists of two parts: a part about noncomplex organizations (lemmas 3 and 4 and theorem 2) and a part about complex organizations (roughly comparable to Thompson's propositions). Both parts are related by definition 4, which defines the notion of atomic organizations as the complement of complex organizations. There are four possible actions that organizations can perform in order to seal off their technical cores from environmental influences (see table 2, above). Furthermore, we showed that buffering (corollary 1) and anticipating and adapting (corollary 2) are corollaries of a more general theorem about the reduction of environmental influences in the organization (theorem 3). Similarly, smoothing (corollary 3) and negotiating (corollary 4) are corollaries of a more general theorem about the reduction of influences in the environment (theorem 4). As a result, the (formal) theory of *Organizations in Action* is a coherent theory.

Explanatory and predictive power.—When comparing the theorems of our formal theory with the propositions in the original text, there are some notable differences. We did provide formal versions of Thompson's propositions 2.1, 2.2, 2.3, and 2.4. However, we did not give a formal version of proposition 2.5: "When buffering, leveling, and forecasting do not protect their technical cores from environmental fluctuations, organizations under norms of rationality resort to rationing" (Thompson 1967, p. 23). Thompson's propositions 2.1–2.4 concern only environmental uncertainty—they all treat the reduction of environmental influences. Proposition 2.5 about rationing, in contrast, treats the reduction of the *effects* of environmental influences. In chapter 2 of *Organizations in Action*, Thompson tells only *when* organizations resort to rationing: namely, when buffering, leveling (smoothing), and forecasting (anticipating and adapting) do not protect the technical core. However, he does not explain *why* organizations resort to rationing. This explanation requires more detailed knowledge about the technological dependencies and the uncertainty caused by these dependencies. That is, explaining *why* organizations resort to rationing requires material properly contained in the other chapters of *Organizations in Action* (notably chapter 5 on the interdependence of components). Without including the further chapters, we are unable to

explain why organizations resort to rationing, since: "Rationing is an unhappy solution, for its use signifies that the technology is not operating at its maximum" (p. 23).

The formal theory identifies a number of unknown consequences of Thompson's theory. Thompson does not discuss noncomplex or atomic organizations explicitly, although he formulates his propositions for complex organizations. Atomic organizations form an interesting special case because they are more vulnerable to environmental influences. We proved that atomic organizations face the same uncertainty as their technical cores (theorem 2). Consequently, atomic organizations cannot comply with one of the main principles of "organizational rationality" as advocated by Thompson. The explicit treatment of atomic organizations is important because it gives an explanation for the fact that organizations embed their core technologies in managerial activities. These managerial activities negotiate between the technical suborganization and those who use its products. Without them, the organization would be an atomic organization and therefore open and unprotected to any environmental influence. Having a managerial level allows complex organizations to seal off their core technologies from environmental influences (theorem 1 and proposition 2.1 in Thompson [1967]). We also proved that organizations can attempt to reduce environmental uncertainty by reducing constraints in the environment, which we termed negotiating (corollary 4). Negotiating is not mentioned in Thompson but completes the four logical possibilities to reduce fluctuations and constraints within the organization and in the environment. Negotiating is a hitherto unknown implication of Thompson's theory. In other words, the theory predicts that organizations attempt to reduce constraints in their environment. This negotiating is not an unknown topic in organization theory: recent empirical findings on legalization (Edelman 1992; Sutton et al. 1994; Sutton and Dobbin 1996) describe how organizations can (collectively) negotiate the impact of constraints in the environment.

Related and Further Research

We started this article by arguing that, although Thompson's theory remains influential, it seems to have lost much of its cachet. Recent literature focuses on alternative theories for explaining the complex relation between organizations and their environment, such as new institutionalism (Powell and DiMaggio 1991) and organizational ecology (Hannan and Freeman 1989). The feeling that Thompson's theory still has much to offer to contemporary scholars inspired us to conduct the formal analysis reported in this article.

The formal theory of *Organizations in Action* presented in this article is an axiomatic theory in which the underlying assumptions are made explicit.¹⁵ Discussing these underlying assumptions allows us to make comparisons with other theories in organization theory. We used eight assumptions. Assumption 1 postulates the uniqueness of the technical core. This captures some of the background knowledge that had to be made explicit in order to derive the theorems. The assumption is a technicality that makes the notion of technical core more clear. The first major premises, assumptions 2 and 3, capture some of the rationality principles underlying Thompson's theory. Assumption 2 states that the performance of the technical core is rationally evaluated, and assumption 3 states that organizations attempt to reduce uncertainty for rationally evaluated sub-organizations. It is the engine of the theory: all the explanations of the formal theory depend on it. As Thompson (1967, p. 159) states: "Uncertainty appears as the fundamental problem for complex organizations, and coping with uncertainty, as the essence of the administrative process." These two assumptions capture the core of Thompson's argument and seem typical for rational adaptation theories, such as contingency theory (Lawrence and Lorsch 1967) and resource-dependence theory (Pfeffer and Salancik 1978). Of course, institutional and ecological theorists will not readily agree with them. The (new) institutional theory is explicitly based "on the rejection of rational-actor models" (DiMaggio and Powell 1991, p. 8) and organizational ecology is constructed as "an alternative to the dominant adaptation perspective" (Hannan and Freeman 1977, p. 929).

One of the key assumptions of an open-systems perspective on organizations is captured by assumption 4, which states that environmental influences cause uncertainty. Although much has changed since Thompson's

¹⁵ Masuch and Huang (1996) give a different formalization of Thompson in a multi-agent, action logic. The objectives of their formalization are different from ours: our objective was to recover the underlying axioms on which Thompson's argumentation is based, whereas their primary objective is to experiment with a new formal logic that is specially designed for representing actions. They argue, "actions presuppose attitudes and engender change, and both are notoriously hard to express in the extensional context of standard logics, e.g., First Order Logic" (Masuch and Huang 1996, p. 72). This may be the case, in the sense that modeling of actual actions happening in an organizational domain may be overly elaborate or complicated in first order logic. However, modeling a theory *about* actions can very well be expressed in first order logic, as we showed in this article. This corresponds with the findings of Masuch and Huang (1996) since, as it turned out, they did not need to use either the multiagent or the action features of their logic. Due to the different objectives, the formal theory of *Organizations in Action* presented in this article makes the underlying argumentation structure of Thompson explicit, whereas Masuch and Huang use a series of abstract goal definitions (characterizing particular rational agents) to explain the propositions.

book appeared the sixties, this assumption seems as pertinent as ever. And even though one could argue that, on the one hand, environmental influences seem to generate less uncertainty due to current information technology and flexible work practices, one might just as well argue that, on the other hand, this is compensated for by the increased volatility of the environment. The next two assumptions, assumptions 5 and 6, are only used for the discussion of noncomplex or atomic organizations. Assumption 5 postulates that we treat the whole organization as its technical core if it has no suborganizations. This assumption is not based on Thompson's text but expresses a convention that allowed us to discuss noncomplex or atomic organizations. Assumption 6 is a technicality that states that a single influence causes a single uncertainty (a minor restriction on assumption 4). This assumption makes explicit the background knowledge that allows for the discussion of atomic organizations.

The next assumption, assumption 7, states that organizations have (some) control over influences directed at their suborganizations. This is one of the core assumptions of rational adaptation theories but will certainly be challenged by institutional and especially ecological theorists. However, these different views may be less orthogonal than they appear to be at first glance. Assumption 7 by no means implies that organizations can control their environments. First, the amount of control that organizations have may be very limited. Second, even if organizations have control, in principle, over influences, their capability to effectively use this control may depend on their capability to predict such influences. Third, even if an organization can foresee an influence, it may lack the ability to react decisively, due to complex internal decision procedures, rendering their control useless.

Finally, assumption 8 states another rationality principle underlying the theory: if an organization attempts to reduce something, and has some control over the cause of it, the organization will attempt to reduce the cause. As Thompson (1967, p. 14) writes, "Instrumental action is rooted on the one hand in desired outcomes and on the other hand in beliefs about cause/effect relationships." This assumption is an important part of the explanation for theorems 3 and 4 and the corollaries about buffering, smoothing, and anticipating and adapting (as well as negotiating). Assumption 8 is a general principle of rationality that seems generally acceptable. Of course, ecologists will deemphasize the organization's capability to entertain beliefs about cause/effect relationships or, even more so, question the capacity to have control over causes. However, institutionalists and ecologists would agree that in the (according to them, improbable) event that the antecedent is satisfied, the consequent should hold as well.

The formal reconstruction revealed that Thompson's theory can be re-

lated to several alternative theories such as organizational ecology (Hannan and Freeman 1989) and new institutionalism (Powell and DiMaggio 1991). Although organizational ecology was originally presented as an alternative to rational adaptation theory, their position turns out to be more qualified. Organizational ecologists carefully distinguish between the individual intentions of organizations and the organizational outcomes. The part of *Organizations in Action* we analyzed in this article is treating organizational intentions (or desired outcomes in Thompson's parlance). Organizational ecologists do not necessarily reject Thompson's assumptions about the rational intentions of individual organizations but would argue that the relation between these intentions and organizational outcomes is weak—resulting in organizations being relatively inert and unable to change their structure to better match the environment (Hannan and Freeman 1984). Also Thompson does not believe that intentions and organizational outcomes are in perfect relation, for example, when he argues that “the basic threat to organizational success lies in interdependence with an uncooperative environment” (Thompson 1967, p. 160). And ecologists, on the other hand, are “acknowledging that organizational changes of some kinds occur frequently and that organizations sometimes even manage to make radical changes in strategies and structures” (Hannan and Freeman 1984, p. 149). The views of organizational ecologists and Thompson are not in contradiction, as can be evaluated by comparing this article with the formalization of organizational ecology's inertia theory (Péli et al. 1994). Although not in contradiction, there is a noticeable difference in the degree in which organizations are believed to be able to successfully realize their intentions in organizational outcomes. Several authors have made proposals for reconciling adaptation and evolutionary selection perspectives (Tushman and Romanelli 1985; Levinthal 1991; Amburgey, Kelly, and Barnett 1993).

Institutionalists have been more radical in their rejection of the rational adaptation perspective and explicitly focus on “properties of supra-individual units of analysis that cannot be reduced to aggregations or direct consequences of individuals' attributes or motives” (DiMaggio and Powell 1991, p. 8). Rather surprisingly, the formal reconstruction revealed that Thompson's theory can be directly related to new institutionalism. We proved corollary 4, stating that organizations attempt to reduce constraints in the environment, which corresponds to findings reported in institutional theory (Edelman 1992; Sutton et al. 1994; Sutton and Dobbin 1996). The formal theory suggests that adaptation theories and institutional theories are not mutually inconsistent. Moreover, adaptation theories can offer explanations for phenomena that are usually conceived as requiring institutional argumentation. This affirms that

adaptation theories may have been discarded too soon: they can even offer explanations beyond the domains with which they are traditionally associated.

Deriving the corollary on negotiating in the environment strengthens the connection of *Organizations in Action* with related adaptation theories such as the resource-dependence theory. Pfeffer and Salancik (1978, pp. 154–57) analyze interorganizational behaviors from the perspective of uncertainty reduction. The fact that traditional adaptation theories and neoinstitutional theories can offer alternative explanations raises some interesting research questions on the limits of both approaches to organization theory. For example, Sutton and Dobbin (1996, p. 794) observe two types of responses to legal uncertainty, which “sustain the neoinstitutional argument, but offer new support for efficiency and labor-control hypothesis.” There seem to be no *a priori* reasons to reject adaptation-based explanations (nor to reject institutional explanations). Kraatz and Zajac (1996, p. 812) explored the limits of neoinstitutional theory, and their “findings reveal surprisingly little support for neoinstitutional predictions.” This leads them to conclude that “current research on organization-environment relations may underestimate the power of traditional adaptation-based explanations in organizational sociology” (p. 812). Further research is needed to elucidate the relation between (rational) adaptation theories and (neo) institutional theories and to investigate how adaptation theories might be embedded in institutional theory and vice versa. The formalization of one dominant adaptation theory is only a small step toward clarifying the relation between adaptation theories and institutional theories.

One of our future research directions is to extend the material by incorporating further chapters of Thompson. Our main focus has been on the propositions in chapter 2. This chapter provides the crucial link between organizations that strive to use the rationality of closed-systems strategies in an open-systems environment by attempting to seal off their core technology from influences of the environment. Thompson (1967, p. 1) argues that the two basic sources for uncertainty for organizations are *technologies* and *environments*. The part of Thompson we formalized in this article concerns uncertainty that is strictly caused by the environment. We intend to incorporate also those uncertainties that are caused by technologies or by a combination of technologies and environments. This would then allow for explaining why organizations have to resort to unhappy solutions like rationing (the missing proposition 2.5). The axiomatic structure of the theory facilitates such further extension.

Building a deductive theory of *Organizations in Action* explicated the underlying assumptions of the framework that Thompson proposes. Mak-

ing the underlying assumptions explicit implies that they too can become the subject of discussion and criticism. Although we made particular efforts to motivate these assumptions by the text itself, we do not claim that we have always chosen the single best solution. However, by being explicit about our assumptions, any debate about the theory and its assumptions can be unambiguously pointed down. This allows for a constructive mode of theory building in which alternatives can be generated, evaluated on their merits, and well-argued choices can be made.

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Book Reviews

Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed. By James C. Scott. New Haven, Conn.: Yale University Press, 1998. Pp. xiv+445. \$35.00.

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The power and pleasure of this polemical book come from its case studies, 90% of the text. The first describes 18th-century Prussian forestry, imposing regimented reforestation plans, riding roughshod over existing complex ecologies, producing environmental degradation followed by reduced timber yields. The last case study is of "high modernist" agricultural methods imposed during 1945–75, regardless of terrain, ignoring the practical knowledge of local farmers. This led to overreliance on fertilizers, sterile hybrid plant strains, and vulnerable monoculture, ultimately worsening the farmers' lot. The other case studies attack modernist city planners, from Haussmann to Le Corbusier, and scientific socialists, from the Leninist "vanguard party" through Soviet collective farms and the Chinese Great Leap Forward to compulsory village resettlements in Tanzania.

James Scott detects four causes of disaster in these schemes: forcing administrative order on a nature and society that are highly variegated, relying on state coercive power to effect innovation, a civil society too weak to resist, and high modernist Enlightenment ideology seeing scientific rationality as the only source of truth. The main villain is the modern state, falsely believing it exists above society, able to perceive and "make legible" the contours of nature and society laid out beneath it and able to grandly reorganize them with its "standardized formulas."

So the book is part of the global reaction against early and mid-20th-century theories, seeing the state as the bringer of social, economic, and moral improvement to the world. Yet, it is not written from the political Right. Scott's sympathies lie with the poor farmers of the global South, the true purveyors of practical rationality, their experiments strongly disciplined by their need to survive by respecting their small piece of nature. Their practical reason is thus a superior form of knowledge to that of high modernist science.

Scott's scholarship is formidable, his insights many, his rich detail usually stilling criticism. I did groan at poor old Diderot and his 18th-century Parisian friends being blamed yet again for the follies of the 20th century. I groaned again as Jane Jacobs's city of "spontaneous self-diversification" was lauded yet again over Le Corbusier's "city as a machine for living"

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and as Rosa Luxemburg's working-class spontaneity was again lauded over Lenin's vanguard party—both being given a feminist slant here, good gal versus bad guy. Actually, when Lenin thought the workers were supporting him, he reversed his position. Lenin was more opportunist than high modernist. And when modernist city planning included gardens and generous living space, as in the British "Garden Cities" and New Towns movements (denigrated by Scott), it improved the quality of life for large numbers of people—as did welfare states in general. But these are empirical quibbles.

Yet, I also have more theoretical doubts. Scott exaggerates the independence of the state. Like the states he criticizes, his state seems to float above society (as in simpler elite theories of the state). Who actually runs such states so that modernist plans can be implemented? He does mention that the market, "large-scale bureaucratic capitalism," and the profit motive may also be instruments of high modernist folly. But he presents no case studies on their role, and to do so might force revision of his title and his four main arguments. In all the non-Communist case studies, though capitalism looms in the background, it is absent from the foreground. His Third World peasants and laborers seem exploited by state elites but not by landlord classes. He blames not large capitalist farmers but agricultural engineers (i.e., modernist technocrats) for the "fetish of industrial farming" taking hold of American agriculture during the period 1910–30. He discusses urban planning as if it was run by state elites. But sociologists like Castells and Molotch have shown that planning is dominated by "city growth machines" centrally involving developers, corporations, and banks. We learn about legibility tools of the state (like censuses and maps) but not about those of capitalism (like the organization chart or accountancy).

Nor are states as coherent as Scott (or, indeed, class theory of the state) believes. Scott emphasizes the influence of German planning during World War I, especially on Lenin. Yet it coordinated three distinct groups, of which state bureaucrats were less significant than generals and the heads of large capitalist enterprises. Scott names Walter Rathenau as the prototype of the "rational bureaucrat" of this planning system. But Rathenau was a capitalist, not a bureaucrat. His family owned the electrical giant AEG, of which he was chairman. The generals, Ludendorff above all, were indeed technocrats, formally employed by the state and in the military realm "modernists." Yet otherwise they were reactionaries, drawn from the Prussian landowning nobility. Their "modernist" goal (as with many of the Red Army and PLO elites important in communist statist disasters) was less to improve humanity than to kill people more efficiently.

Militarism has been a distinctive form of disastrous modernism yet makes no explicit appearance in this book. Nor does nationalism, which (far more than modernism) has been focused on the state. And Scott's exposés often depend heavily on citations from critical government scien-

tific reports—even opponents of high modernism inhabit states. States have many rooms and many constituencies.

Of course, when disasters are driven not by singular states but by markets or complex congeries of dominant elites or classes exerting diffuse political influence, they are generally less visible. The inefficiency and inequity of the U.S. “corporate market” system of health care, when compared to the statist health care systems of Europe, is not often seen as a “disaster” comparable to Scott’s case studies. Yet, it systematically results in the earlier death of poorer and minority Americans, and we may reasonably attribute it more to class and ethnic exploitation than to American government. Consider the more obvious disaster of the extermination of the native peoples of North America, legitimated by some of the modernist rationality noted by Scott. The declaration that the land was “empty” (*terra nullis*)—and so could be cleared of its peoples—enabled surveyors to lay out the rational grid system that still dominates the entire land surface of the United States. But here genocide was mainly the responsibility of a white settler society rather than a state. Thus, the sources and forms of anything we might wish to call “high modernism,” and its attendant disasters, were more plural than in Scott’s narrative. High modernist disasters have involved states, armed forces, markets, corporations, classes, nationalist movements, and so on, all interacting in complex, confused, and often unanticipated ways.

Perhaps it is not states in general but particular types of states that are implicated in modernist folly. One type is visible in Scott’s most terrible case studies: states in backward countries where elites believed that the future could be planned—since it was visible in more modern countries abroad. State-centered late development projects preceded the Enlightenment proper, as in Peter the Great’s reforms (his rebuilding of St. Petersburg is here cited as part of modernist architecture). Late development inspired the industrializing plans of Marxists in Russia, China, and Tanzania and of nationalists elsewhere. Thus, elites in developing countries like Brazil sought to build Le Corbusier high modernist capital cities. Perhaps neither statism nor modernism per se but subtypes might be the main culprits.

This is a book of powerful case studies and weaker theory. The case studies allow Scott to attribute many appalling disasters to modernism overdosing on statism. But the attribution really requires a better theory of modern states than he possesses.

The Therapeutic State: Justifying Government at Century's End. By James L. Nolan, Jr. New York: New York University Press, 1998. Pp. xiv+395. \$45.00 (cloth); \$18.95 (paper).

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Reading about therapeutic legitimations of the U.S. state, I felt a very strong emotional identification with James Nolan's claim that in the latter part of the 20th century, a "therapeutic ethos" has seeped into the nexus of government and politics. How else could one explain that "a family in Hawaii recovered \$1,000 in compensation for the emotional distress incurred from the negligent death of their dog, Princess" (p. 62)? On the other hand, evaluating *The Therapeutic State* rationally, I found certain methodological, theoretical, and substantive problems in the analysis. In his defense, Nolan can point to a parallel alternation—between the emotional and the utilitarian—in the state itself. My own reaction is anticipated by Nolan's analysis of the discourse about which he writes.

Nolan keys his inquiry to David Beetham's decomposition of Max Weber's concept of legitimate domination into three analytic components—technical *validity*, *justification* of laws via "cultural codes," and popular *consent*. In effect, Nolan narrows the problem of legitimation to the question of justification. He tackles that issue not by asking about some overarching justification, but by exploring the *specific* justifications that state agents and politicians employ. Quibbles to one side, Nolan convinces that there has been a fundamental shift in how government justifies certain activities and, concomitantly, carries them out. The transformative code is the therapeutic ethos—centered in "the victim pathologies of the emotivist self interpreted for us by the priestly practitioners of the therapeutic vocations" (p. 17). The state becomes the therapist of last (or sometimes first) resort. Convicted drug users, for instance, no longer simply receive punishment; they often become the targets of counseling, rehabilitation, and emotional support by people who care. Even courtroom trials become therapeutic encounters. And in prison, inmates are encouraged to voice their feelings about parents who "jacked you around and screwed you up" (p. 122).

The core of the book is a set of case studies exploring justifications of state action in civil law, criminal justice, public education, welfare policy, and the rhetoric of political debates. For each venue, Nolan examines how the therapeutic ethos develops historically and how it articulates with "utilitarianism" and three classically important strands of justification in the United States—civic republicanism, natural-law liberalism, and Protestant Christianity. Thus, for child welfare policy, *The Therapeutic State* contrasts religious rhetorics of the early 20th century (e.g., about the "sacredness" of the home) with contemporary emphases on the child's identity, self-esteem, and quality of interpersonal relations (p. 213).

In his conclusion, Nolan contemplates the admixtures by which a ther-

apeutic ethos and a utilitarian ethos become conjoined and how these admixtures mediate the relationships between a Giddensian "high modern" state and an increasingly postmodern culture. Given countertendencies such as the backlash among religious fundamentalists against therapeutic intrusions, Nolan is no doubt wise to hedge his conclusions about how long therapeutic justifications of the state will endure. Instead, he closes by invoking Michel Foucault and expressing concern about a "post-modern padded cage" (p. 306).

Nolan writes well and within a distinguished tradition of analysts who have sought to discern the basic cultural contours of American life, from Tocqueville to Daniel Bell, Philip Rieff, and Robert Bellah and his colleagues. Yet readers will want to reflect carefully on how far his analysis shows the therapeutic ethos to have "infused the modern American state, thus offering the state an alternative source of legitimation" (p. 21). Nolan rightly poses this as a question, not a claim. Yet *The Therapeutic State* sometimes portrays the therapeutic ethos as an almost free-floating geist, to be discerned in various discourses that invoke it, but having an autonomous *telos* of its own. Though Nolan shows the agency of people who employ therapeutic discourse, he does not much explore the kinds of power gained (and by whom) when it is invoked (for this question, bringing in Foucault more strongly and much earlier would have been useful). Moreover, justification is a slippery concept that can slide away from legitimation: therapeutic discourse *within* government programs is different from the rhetorics of justification for those programs, and these "local" rhetorics of government do not always work as proxies for justificatory legitimation of the nation state as a whole. On this front, the choice of sites in which to examine legitimating discourse loads the analysis in favor of demonstrating the rise of therapeutic discourse. The book gives scant attention to state legitimations in commerce, geopolitics, national security, environmental pollution credits, and corporate antitrust law—arenas that do not seem especially prone to therapeutic justifications. But these considerations should not overshadow the substantial contribution of Nolan's book. *The Therapeutic State* documents an important emergent underpinning of legitimation in emotions talk. Anyone interested in state power today ought to read it.

The Founding Myths of Israel: Nationalism, Socialism, and the Making of the Jewish State. By Zeev Sternhell. Translated by David Maisel. Princeton, N.J.: Princeton University Press, 1998. Pp. xiii+419. \$29.95.

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Sternhell's book presents a passionate (he does not shy from expressing his views or from grading politicians) and sympathetic yet critical history of the ideological and political disputes that have accompanied the cre-

ation, and shaping, of Israel. The analysis focuses on how the newly emerging state blended "socialism" with "nationalism," thus creating an interesting ideological concoction in which nationalism quickly gained ascendancy over everything else. This meant that both socialism and capitalism were manipulated into playing a role in the main drama that was the national revival of the Jewish state. Sternhell reveals that while those I call the "Totemic Fathers" of the state used an external rhetoric of equality and socialism (but not "freedom"), the translation of this rhetoric into reality culminated in at least two contradictory directions.

First, an unequal distribution of resources (and a limited form of capitalism) and a replacement of socialist ideas with nationalistic concepts took place. The name of the book reflects these astute observations: the rhetoric used (the "myth") versus the reality that was as far from the rhetoric as one could possibly imagine (Sternhell even shows how Israel's famous sociologist S. N. Eisenstadt accepted the "myth" [p. 288]). In this respect, Sternhell's book presents a powerful and successful debunking of popular misconceptions about Israel's ideological and political past. Still, one must remember that most of those "Totemic Fathers" were not philosophers or university professors; they were people of action, deeply immersed in the act of creating a new state with new national and personal identities. Nationalism may have been the only route to statehood.

Second, Sternhell provides a penetrating and sobering look at the personal practices of some of the early "Totemic Fathers" (e.g., Ben-Gurion), showing not only that genuine democracy and tolerance never gained priority on their main agenda but that they were personally corrupt and cynical in their abuse of public funds. Chapter 6 is utterly fascinating and amazing. For example: "In the best traditions of nationalist socialism, the lowest sector of society received psychological compensation. Its exalted status was supposed to compensate for difficult living conditions, low income, exploitation . . . and its lack of social mobility" (p. 296). Thus, while preaching equality, purity, and modesty for and above all, emphasizing the supremacy of agricultural work and downplaying the role of good quality education, Ben-Gurion "never saw any inconsistency in the fact that his apartment . . . with its four large, attractive rooms . . . cost him two or three times the monthly salary of an agricultural worker. . . . His children attended the prestigious Gymnasia Herzliya where fees were [very high] . . . and took piano lessons" (p. 295). This, at a time when very few could afford any of the above and when the official ideology preached exactly against the lifestyle Ben-Gurion was practicing.

Sternhell definitely mastered the relevant history. His view is impressively informative, and this meticulous book is packed with information, anecdotes, insights, and quotations, almost to the point of creating an effect of fatigue and exasperation on the part of the reader. Moreover, appreciating this book certainly requires prior (and reasonably good) knowledge of (not to mention interest in) Israel's ideological history.

Sternhell's contextualization of Israel's ideological and political history within major ideologists and movements in Europe integrates its argu-

ments within a larger global view. Along the way, we have to master a decent amount of terms (e.g., nationalist socialism, romantic nationalism, constructive socialism, democratic socialism, experiential socialism, tribal nationalism, integral nationalism, etc.). Eventually, I found this conceptual forest too distracting.

Sociologically speaking, I missed the translation of the political/ideological dimension to other dimensions. Since the book is focused on the ideological and political history of an emerging state, it turned a blind eye to many other important developments that may have been influenced by the disputes in the ideological dimension. For example, the military, police, higher education, crystallizing personal and collective identities outside the ideological/political complexities, authentic music, poetry, literature, ethnicity, sport, popular culture, gender, and the worst internal conflict facing Israel now—that between religious and seculars. Moreover, the translation of the nationalistic ideology to such everyday practices as living accommodations, marriage, divorce, food, transportation, and dress and to institution building (e.g., political, the law, civil service) or to social construction (and substitution) of elites is missing altogether.

While Sternhell uses the term “myth” as a central device, his only reference to theories of myths in the political context is to ancient Sorel. This innocently blithe disregard for the rich (and relevant) scholarly literature about myth (and collective memory) since Sorel’s work is irritating (e.g., Henry Tudor’s *Political Myth* [Macmillan, 1972]).

Knowledgeable students of Israel, interested in its political and ideological history, will find Sternhell’s book both highly useful and indispensable. While reading the book requires time and patience, it is a rewarding experience. At Israel’s fiftieth birthday, ending with a quote from Sternhell’s sobering introduction seems highly appropriate: “Those who wish Israel to be a truly liberal state or Israeli society to be open must recognize the fact that liberalism derives . . . [from separating] . . . religion from politics. A liberal state can be only a secular state, a state in which the concept of citizenship lies at the center of collective existence” (p. xiii).

Governing with the News: The News Media as a Political Institution.
By Timothy E. Cook. Chicago: University of Chicago Press, 1998.
Pp. xi+289. \$48.00 (cloth); \$17.97 (paper).

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Journalists view themselves as members of an autonomous profession that serves the public by reporting information impartially and by providing an independent check on government. In *Governing with the News*, Tim Cook argues that the news media are a political institution that relies on government subsidies for much of the information it reports and is an integral part of the process of governance. For these and other

reasons, Cook maintains that news is biased in ways that often make for bad public policy.

Cook's purpose, however, is not to disparage journalism. It is to establish the political character of the news media as a basis for new and, as he believes, more sensible government policies toward them. In Cook's view, these would-be policies encourage journalists to be more open to alternative sources of news and the media to be more accessible to ordinary citizens.

The first of the book's three main sections traces interactions between press and government from prerevolutionary times to the present. The key argument here is that American journalism has never been as independent of government and governing elites as it would like to believe. In the 20th century, Washington has, as a favor to the big players, consistently used regulation to hold down the level of competition among news providers. Most important, government has developed a "public relations infrastructure" in the form of press offices and media events that, in effect, subsidize journalists' efforts to fill their daily quotas for the production of news.

The second section of the book works its way (somewhat tediously) through the elements of a standard definition of "political institution" to make the point that the news media qualify as one. It also makes the case that the news values of journalists—timeliness, drama, conflict, among others—lead to biased news. "The production values of the news direct [reporters]—and us—toward particular political values and policies: not so much pushing politics either consistently left or right as toward officialdom and toward standards of good stories that do not make for equally good political outcomes" (p. 91).

The third section of the book, "Government by Publicity," shows how each of the major institutions of national government attempts to achieve its goals by using the news media to convey its story to the public. The theoretical point is how deeply journalism is implicated in the normal process of governance. Even the Supreme Court, according to Cook, has a media strategy, since how decisions are communicated "matters for the impact of the court" (p. 160). This section ventures the hypothesis that "media strategies become increasingly useful means for political actors to pursue governance . . . as the disjuncture between the power of those actors and the expectations placed on them grows" (p. 119).

If the news media are a political institution sustained in significant part through government policy, and if media performance is problematic, it is, as Cook argues in his concluding chapter, both legitimate and prudent to consider adopting new policies that promise better performance. One suggestion is that "subsidies to the news media should be continued, but increasingly expanded and targeted toward more economically vulnerable news outlets and news organizations" (p. 187). For example, he argues that the federal government should auction off access to the new digital TV frequencies to the big media players and use the proceeds to subsidize smaller ones, especially ones that promise to empower ordinary citizens.

Because politicians regard news policy as a hot potato, and because they want to keep big media around for their own purposes, I would be surprised if Cook's argument leads policymakers to promote further fragmentation of the mass audience than is already occurring. But I would not be surprised to find scholars of mass communication, many of whom are as critical of big media as anyone, to take up Cook's general argument. "Citizen access to the media" is the tame child of the 1960s slogan of "power to the people," and academics are flocking to it.

I would like to mention one dissent from Cook's argument. Cook is not the first to argue that the news values of journalists lead to bias, and he is one of a vast legion of critics decrying the influence of the profit motive on news. Yet he also wants journalists to be more responsive to what the public wants. I see a contradiction here. What Cook and others pejoratively call journalistic values or production values are, in many cases, barely disguised rationalizations for giving the public what it wants out of the news. Similarly, the pressure to maximize profits is not so different from pressure to make the news interesting to as many people as possible. Cook closes by writing that "it is now time to work toward getting the kind of news, and the kind of politics, that we want and that we deserve." But it seems quite possible that much of what Cook sees as wrong with the news is precisely that the public already is getting what it wants and deserves from the news.

Language Policy and Social Reproduction: Ireland, 1893–1993. By Padraig Ó Riagáin. New York: Oxford University Press, 1997. Pp. xv+297. \$80.00.

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This is an interesting, challenging contribution to studies of language policy and language planning in general, as well as to our understanding of Irish-English bilingualism in Ireland. Ó Riagáin sets out explicitly to challenge some of the prevailing models in studies of bilingualism, specifically those based on the concepts of *domain* and of *network*, both of which currently inform language policy in a number of countries. He argues, in essence, that both suppose a degree of homogeneity of language practices that cannot be empirically supported, and that both suffer from being essentially descriptive, rather than explanatory, concepts. He proposes instead to examine the relationship between Irish-English bilingualism and Irish language policy historically, from a political economic perspective based on Pierre Bourdieu's concepts of *capital* and *market*. This means asking what kinds of political economic conditions give what kinds of value to Irish and English and for whom.

The book focuses mainly on the period 1926–93 (with a briefer account of the period 1893–1926). It is exclusively based on a consideration of

census and survey data. The author first examines census results, which in Ireland, as in many other multilingual countries (notably the United States and Canada), are usually the basis of language policy. By comparing census and survey data, he shows that the picture of the depth and breadth of use of Irish looks quite different depending on what questions you ask. He argues that the census gives a skewed picture, portraying Irish as being too concentrated and too geographically restricted. The reality, he claims, is much more heterogeneous and diffuse, both within the so-called *Gaeltacht* and without.

The rest of the book is devoted to an analysis of the relationship between Irish-English bilingualism, language policy, and socioeconomic change. The author claims that in 1926, the use of Irish was mainly linked to a regionally concentrated small-farm-based economy. By 1993, the use of Irish is spread more evenly across Ireland, but there are fewer monolingual Irish speakers; that is, most Irish speakers also speak English. Also, these bilinguals are mainly found among schoolchildren or families with school-age children. What happened?

The author sees an explanation in a combination of socioeconomic and political changes. On the economic side, it is necessary to take into account the shift from a rural to an urban economy and the growth of an Irish middle class. The political dimension of Irish nationalism is clearly linked to these processes; the state was able to clear the way for a growing urban middle class. Its pro-Irish language policies not only legitimized its actions but created a protected economic space for the middle class. Irish language policies, according to Ó Riagáin, had in this respect a real effect, through the creation of a linguistic market.

For him, 1973, the year the state repealed its language legislation, is a watershed. He argues that the state gave up in the face of the growth of the importance of private sector corporate capitalism and globalization. It is interesting in this regard that Ireland now seems to be the major European locus of international call centers. Yet at the same time, it is necessary to account for the persistence of Irish, albeit in a new mode. The author seems to retain a modernist, nationalist perspective, according major importance to home and community as the only "real" sources of language reproduction. He discounts the power of school and work, the sites where Irish is actually used these days, to accomplish the reproduction of Irish. Of course, the Irish that is reproduced is not the Irish that was once spoken on small farms in western Ireland, but only the modernist nationalist perspective requires that nostalgia as a basis of legitimization.

Ó Riagáin's own data show that what is important now is some form of credentialization of knowledge of Irish, for a certain, mainly middle-class, segment of Irish society. What we need to understand is why this is so and what implications it has for relations of power within Irish society and between Ireland and the rest of the world. In order to accomplish this, it is necessary to move beyond the use of surveys and to engage in more anthropologically informed research based on ethnography and analysis of situated language practices. It is also useful to compare this

case to that of other linguistic minorities; Ó Riagáin's account converges with some recent research on French in Canada, for example, and probably would find echoes in other linguistic minority areas of Europe. The book thus contributes to the development of hypotheses about the fate of linguistic minorities in the modern world; their oft-announced disappearance seems not to be imminent, and yet their transformation poses serious challenges to the nationalist ideologies that, in some sense, spawned them. This is a first glimmer of the rethinking that needs to be done.

Durable Inequality. By Charles Tilly. Berkeley and Los Angeles: University of California Press, 1998. Pp. xi+299. \$29.95.

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In the postwar period, sociologists of the Parsonian persuasion assumed that inequalities of race, gender, and class background were all forms of ascription that would together wither away under the market pressures of capitalism and the rationalizing logic of modernity. The obvious effect of such theorizing, and not an altogether unintended one, was to reduce class analysis to the study of "background effects," thereby subsuming it under a putatively more general theory of ascription. The subsequent rise of neo-Marxian scholarship restored class analysis to a central position; indeed, by treating categorical forms of inequality as mere hindrances to the grand showdown between competing classes, it was effectively assumed that class-based loyalties were in the end fundamental. In recent decades, the discipline has clearly turned full circle, with the class-centered stories of the neo-Marxian past giving way to new multidimensional accounts emphasizing the distinct interests and subcultures formed by the "intersection" of race, class, and gender categories. Against this intellectual backdrop, Charles Tilly has sought to provide a unified framework with which to understand all forms of inequality, thus sharing with Talcott Parsons and other postwar theorists the conviction that ascriptive processes must be studied of a piece but breaking with the complementary view that they are mere residues of our *gemeinschaftlich* past.

The core claim of Tilly is that categorical forms of inequality assist in solving common organizational problems and are therefore durable and entrenched rather than functionless vestiges. There are four related processes at work here:

1. The extraction of surplus value from subordinate workers within a firm is rendered more legitimate and defensible insofar as these workers are drawn from subpopulations that, in the wider social system, are correspondingly subordinate (e.g., females, blacks, immigrants). In effect, such staffing practices press preexisting relational structures with wider institutional backing into service for the organization, thereby avoiding the

potentially high cost of developing new structures and legitimating ideologies from scratch.

2. The same line of argumentation accounts for the well-known tendency of immigrants, women, and other categorically defined groups to dominate particular occupational niches in firms or the labor market more broadly. This form of social closure, which Tilly dubs "opportunity hoarding," emerges because (a) information about job openings travels through networks that are categorically segregated and (b) managers choose to rely on such informal networks because it is cheaper to harness preexisting organization than to devise it afresh.

3. There is good reason to doubt that managers have independently discovered in firm after firm the efficiency of deploying categorical inequality for the purposes of exploitation and recruitment. To the contrary, Tilly argues that such organizational isomorphism proceeds in part from simple emulation, whereby firms end up defaulting to widely disseminated routines and models (e.g., the female sex-typing of secretarial work) rather than experimenting with new ones.

4. The durability of the resulting arrangements can be understood, finally, as arising from the elaboration of social routines and relations (e.g., on-the-job friendships) that most workers, even those who are exploited, come to value and hence strive to maintain.

Although the preceding processes are perhaps revealed most obviously in work organizations, the great contribution of Tilly is to demonstrate that categorical inequality operates under similar principles in all organizational contexts. We are thus treated to masterful interpretations of the South African system of exploitation, the Catholic Emancipation in Great Britain, the development of market niches for Italian emigrants, and the emergence and maintenance of occupational sex segregation in the United States. The resulting book is a tour de force that works equally as a general treatise on the sources of inequality and a substantive study of the more spectacular forms of existing inequality.

It is nonetheless possible to manufacture the requisite criticism. In this regard, stylistic problems loom large, most notably the decision by Tilly to position his work as yet another attack on individualistic approaches to inequality. While the rhetoric of anti-individualism is a tried and true formula, it would have been intellectually more valuable had Tilly sought to defend his approach against other relational approaches that emerged either before or after the heyday of methodological individualism. As hinted above, closure theory may be the most obvious competitor here, emphasizing as it does precisely those Weberian forms of monopoly control that Tilly relabels as opportunity hoarding. The principal distinction, it would seem, is that closure theorists conventionally emphasize the simple facts of exclusive control over property and occupational niches (equating these with exploitation *per se*), whereas Tilly suggests that exploitation is often best realized by coupling such control with categorical forms of inequality. It is unclear, however, why an entirely new language is needed to represent this elaboration, all the more so because it appeared

long ago in simpler form in the neofunctionalist work of Leon Mayhew ("Ascription in Modern Societies," in *The Logic of Social Hierarchies*, edited by Edward O. Laumann, Paul M. Siegel, and Robert W. Hodge [Markham, 1970]). That is, Mayhew argues that the staying power of ascription is attributable to its cheapness, with firms thus saving much in organizational costs by opting for "existent pre-established structure . . . rather than creating a new specialized structure for the same purpose" (p. 313). If Tilly is thus anticipated by Mayhew, he also parts way in assuming that the savings generated by ascription are captured by organizational elites rather than more broadly distributed; and, in this limited sense, *Durable Inequality* may be seen as a case of Mayhew meeting Marxism. The key question is, of course, whether this synthesis is empirically sustainable, since it is at least plausible that subordinate workers will themselves profit from their own subordination. Indeed, if a great many organizations find it advantageous to hire categorically subordinate workers, then the accordingly heavy demand should drive up wages and allow these workers to recapture some of the exploitative transfer.

The foregoing all goes to show that, in characteristic fashion, Tilly has opened up fascinating new lines of inquiry. It is always cause for celebration when Tilly publishes a book, but the present one is especially important because it outlines a general theory of inequality that may well reshape the field. This is classic Tilly, and surely we can ask for nothing more.

Just Institutions Matter: The Moral and Political Logic of the Universal Welfare State. By Bo Rothstein. New York: Cambridge University Press, 1998. Pp. xv+254. \$59.95 (cloth); \$19.95 (paper).

Duane Swank
Marquette University

Bo Rothstein's new book is both a strong defense and an excellent empirical analysis of the universal welfare state. Rothstein's principal aim is to provide a constructive theory of the welfare state where normative theory about what the state *should do* is combined with an appreciation of what the state *can do*. As to normative theory, Rothstein draws heavily on John Rawls, Ronald Dworkin, and others to articulate a "rights-based liberalism" in which a neutral state adheres to the principles of equal concern for all citizens and equal respect for the preferences and choices of rational and autonomous individuals to pursue their "life projects." To realize these values, the state must also provide "basic capabilities" through social protections and services that enable all to fully and equally act on their preferences and participate. Rothstein argues that this "liberal" moral base of the universal welfare state is not only normatively desirable but, contrary to many claims about the universal welfare state,

is also empirically important in the development of (Swedish) social policy.

As to what the state can do, Rothstein offers a highly useful critique and synthesis of the implementation literature, concluding that most difficulties in implementation involve problems of substance or process in the areas of policy design, organization, and legitimacy. Crucial to this analysis is the argument that broadly targeted policies that address static problems (e.g., flat-rate pensions) are much less prone to failure during implementation than interventionist policies that target dynamic problems (social services for specific needy groups); universal programs are simply easier to implement. However, even in the case of dynamic interventionism, successful implementation is possible with careful attention to the structure and process of the organization of public services (i.e., selecting the right organizational form and technique) and to program legitimacy (e.g., incorporating citizen participation and choice). The relative success and legitimacy of Swedish health care and active labor market policies attest to what the state can accomplish.

Rothstein blends normative and empirical concerns in an analysis of the political and moral logic of the (Swedish) universal welfare state. According to Rothstein, support for the welfare state hinges on the "contingent consent" of strategically self-interested and moral citizens. In turn, this consent is dependent on citizens' appraisals of the substantive, procedural, and distributional fairness of the welfare state. The political logic of the universal welfare state weds the self-interest of the poor, working class, and middle class through universal social insurance and services. The electorally crucial middle class receives substantial benefits and insurance against risks in return for significant tax payments. The situation is just the opposite in selective (predominately means-tested) welfare states: the political logic of the residual welfare state is division and distrust and, in turn, a highly vulnerable welfare state. However, for Rothstein, the explanatory power of models grounded in self-interest falls short. Indeed, a substantial part of the book is devoted to highlighting how—contra rational choice and cultural theory—democratic institutions generally, and welfare state structures specifically, cultivate values and norms in society. As to the universal welfare state, the moral basis of equal respect and concern, broadly targeted universal benefits, carefully adapted delivery organizations, and participatory administrative processes achieve relatively high levels of contingent consent. Solidarity, trust, and confidence in state intervention are promoted. In selective welfare states, problems related to substantive justice (e.g., conflicts over defining the "deserving poor"), procedural justice (perceptions of bureaucratic aggrandizement and waste), and a fair distribution of burdens (e.g., constituency fraud) are endemic.

Overall, the book makes the best case for universal social policy I have seen; it is also an insightful analysis of the politics of universal welfare states. However, I believe the book could have been improved on three counts. First, systematic material on levels and trends in income replace-

ment rates, eligibility standards, social service spending, and the like is missing. This omission leads to some errors in generalization. For instance, Rothstein (p. 155) claims that even during Scandinavian bourgeois governments of the last 20 years, one cannot "discern a reduction in welfare expenditures or a change to a more selective social policy." However, to give just one example, in Denmark, the early 1980s bourgeois coalition reduced spending for social services (relative to GDP) and pursued a variety of neoliberal policies (see my "Social Democratic Welfare States in a Global Economy: Scandinavia in Comparative Perspective," in *Globalization, Europeanization, and the End of Scandinavian Social Democracy?* edited by Robert Geyer, Christine Ingrebriksen, and Jonathon Moses [Macmillan, 1998]). While Rothstein is certainly right in emphasizing the resiliency of these welfare states, there have been more neoliberal reforms than the book suggests or documents. Second, it is surprising that there is little about the formation (as opposed to the persistence) of the universal welfare state other than brief reference to the well-known work of Esping-Andersen on the formative role of social democracy. Rothstein could have drawn from recent comparative historical and contemporary quantitative analyses to provide readers with a better understanding of the causal roles of social democracy, trade unions and corporatist institutions, and strategic working- and middle-class alliances. Third, while Rothstein does a good job assessing the welfare state consequences of the rise in demands for individual autonomy, he has little to say about the welfare state pressures of demography, economic crises, or internationalization. Integration of the burgeoning work on these issues would have been useful.

Despite these criticisms, this is an excellent book and can be highly recommended to scholars of social welfare policy, Scandinavia, and comparative politics. It is appropriate for advanced courses in the areas of social theory, public policy, and European and comparative politics.

Bold Relief: Institutional Politics and the Origins of Modern American Social Policy. By Edwin Amenta. Princeton N.J.: Princeton University Press, 1998. pp. xiii+343. \$39.50.

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Many readers will be surprised by Edwin Amenta's title. New Deal social policy would seem to have been neither "bold" nor to have provided much "relief." The subtitle also gives pause. Was it "institutional" or "political" factors that were primarily responsible for the shape of social policy outcomes during the New Deal? But the book delivers compelling replies to each set of questions, accompanied by a generous set of Depression-era photographs by Works Progress Administration artists such as Ben Shahn and Dorothea Lange.

"Bold" relief? In his opening chapter, Amenta nicely reconstructs the logic behind the patchwork of social programs that defined the domestic New Deal. Reformers envisioned a combination of programs in which there would be something for everyone, hence, cumulatively universal. At the heart of this agenda were the job creation programs of the WPA and other New Deal agencies. A variety of other programs were designed to provide income support for the rest of the (nonworking) population. The entire lot were known as "relief" programs, without any intentional separation of "social security" and "welfare" programs. This would come only later, as the New Deal stalled in the 1940s.

The advance of the core New Deal agenda in the 1930s was startling, all the more so given the well-known limitations of social provision in the United States up to that time. Spending levels provide at least one useful comparative measure of welfare effort. Amenta shows that by 1938, amazingly enough, the United States was spending more than any other country in the world on social programs. Reformers hoped that such state-building successes in the 1930s were merely a prelude to their further development in the 1940s. But it was not to be. Why? Here Amenta advances what he characterizes as an "institutional politics" model to explain social policy outcomes in the United States at the national and state levels and (in a brief comparative chapter) in Great Britain. The model has three legs: the constraints and opportunities provided by political institutions and previous policy legacies; the size and strength of "pro-reform" forces in Congress and state legislatures; and pressures for reform from social movements from below and from bureaucratic actors from above.

Amenta's institutional politics model places at its center an analysis of the political power of reform forces (in this case, northern Democrats). Program expansion occurred when the Democratic advantage was enormous (e.g., 1935–38) and rarely at other times. This is a simple point, and it is almost tautological. Yet much of the recent debates over the New Deal and other episodes of state building had centered on controversies about the relative influence of business, social, and labor movements from below and the reform initiatives of professional policy experts from above. The causal importance of party strength is often ignored.

Another contribution of the institutional politics model lies in its determination to make sense of the contexts under which social movements do or do not influence policy outcomes. Here, Amenta's articulation of the interaction between institutions and political struggles is especially helpful. When movements made demands that were consistent with those of previous programmatic initiatives in the context of a large proreform group in Congress, they were able to exert some leverage. In other contexts, their influence was more muted. At best, in hostile climates, even powerful movements typically achieve little more than altering the distribution of spending patterns in those regions where they had strength. This pattern generally appears to hold across the states as well as at the

national level. Amenta's empirical tests of his model are unusual in historical sociology and generally convincing.

I have only a few concerns. Amenta conclusively rejects two sources of New Deal state building that other analysts have emphasized: the Depression and business influence. But it is hard to completely accept either of these claims. The Depression certainly was a *necessary*, if not sufficient, condition for the New Deal to have happened, if for no other reason than its contribution to the election of an overwhelmingly proreform Congress in the 1932–36 period. New evidence of business influence on one crucial program, the old-age insurance (OAI) provisions of the Social Security Act, has been developed in recent scholarship by Steven Sass, G. William Domhoff, Colin Gordon, Peter Swenson, and others. In particular, it is almost impossible to believe that the successful expansion of the OAI program after 1935 could have been achieved in the absence of considerable business support and only modest opposition. Amenta does not challenge any of this newer evidence directly, merely restating (or citing) previously published work. I wish he had explored these issues more systematically here.

The New Deal vortex was complicated, and the prospect of unraveling it has excited many sociological imaginations. Not all issues are (or indeed could be) convincingly treated in a single volume. Still, this elegantly written study delivers more than enough insights to serve one-stop consumers of the political sociology of the New Deal well.

Whose Welfare? AFDC and Elite Politics. By Steven M. Teles. Lawrence: University of Kansas Press, 1996. Pp. x+226. \$29.95.

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This book tells the story of U.S. Aid for Dependent Children (ADC) and Aid for Families with Dependent Children (AFDC) policy from the former's 1939 "normalization" (or sharpened differentiation from old-age and survivors insurance and its widow benefits) through mid-1996. It adds, in my estimation, more to our understanding of AFDC's troubled history than any recent work, and it provides a compelling and comprehensive interpretation of that history.

Teles's core thesis is that politically fueled elite ideological polarization and deadlock brought U.S. poverty-reduction policy to its present nearly ruinous (but not entirely hopeless) state (chap. 1). Elite political dissensus did so despite a popular consensus on individual work requirements and federal work guarantees that provided potential grounds for reform. Teles frames his discussion of elite and mass ideological dissensus in terms of three cultural types (intermingled for two centuries in the United States) that he draws from work by Mary Douglas and collaborators (see

chap. 1). The types are "hierarchists," who are high on "group" (or in-group identification) and "grid" (or interpersonal connectedness and interdependence); "individualists" (low on both group and grid); and "egalitarians" (high group, low grid). This theoretical framing of, in effect, "social conservative," "libertarian" and "left-liberal" ideological groupings is almost as elliptically presented by Teles in *Whose Welfare?* as it is by me (for want of space) here. Indeed, the Douglas framework is rather unsystematically applied to Teles's otherwise superb historical account.

Teles's 1939 "normalization" of AFDC (chap. 2, p. 34) refers foremostly to the 1939 transformation of old age insurance into old-age and survivors insurance (and secondarily to the 1950 congressional prohibition of ADC funds for any recipients of old-age assistance). These legislative turns significantly weakened AFDC (actually still ADC) as a program for *general* "widows relief" and largely sealed AFDC's eventual labeling as a program for the undeserving, and African-American, poor (pp. 34–36). By the 1962 metamorphosis of ADC into AFDC, black Americans counted for nearly one-half of AFDC beneficiaries, and the program's political marginalization was complete.

Teles convincingly documents that popular opinion shifted after the early 1960s from a consensus favoring income maintenance for female household heads to one favoring governmentally assisted work (chap. 3). He argues that development of a popular "jobs policy" consensus was accompanied throughout the 1970s and 1980s by three trends (chap. 4). First, hierarchist/conservative criticism of AFDC for undermining the two-parent family (and the social order more generally) proliferated, gaining popular exposure. Second, individualist/libertarian criticisms of AFDC as a labor market distortion (and federal intrusion) did likewise. Third, egalitarian/left-liberals, increasingly oriented toward litigative struggles for entitlements, championed the principle of income maintenance—and income "rights" (chap. 5). Both AFDC criticisms converged *contra* AFDC, though hierarchist ones often pointed to program reform needs, while libertarian ones tended toward laissez-faire (i.e., "welfare" policy's revocation). Conflicting communal, free-market, and entitlement goals lost a rare chance for legislative reconciliation with the 1969 legislative deadlock and death of Nixon's (negative income tax) Family Assistance Plan by an unlikely legislative alliance of social conservatives and left-liberals (Dixiecrats and the National Work Relief Organization in particular).

Throughout the 1970s, continued elite (and legislative) deadlock drove pro-welfare activist in search of looser benefits, eligibility, and monitoring—goals that never gained popular support and that suffered some Reagan-era legislative reversals (chap. 6). Throughout the Reagan-Bush years, while benefits continued a slow post-Nixon decline, conservative structural programs emerged in the form of federal waivers of AFDC guidelines to states that convincingly proposed experimental revisions, *under constant funding*, of AFDC. According to Teles, extra-legislative waivers swamped federal workfare proposal and legislation (e.g., the

1988 Family Support Act) in importance during the 1980s, much as court battles had swamped legislative action during the 1970s. The waiver "process" created a taste for increased autonomy among state-level policy reformers, right and left (chap. 7). The new elite inclination for state autonomy put Clinton's welfare reform proposals at risk: calls for the end to national welfare could look forward to political cover from the flag of state autonomy. (Indeed, Clinton milked waivers for reform ideas and support, inadvertently strengthening the state-level reform alternative.) In the wake of Clinton's troopergate- and health-care-induced collapses of support (and Clinton's indiscriminately rousing "end welfare as we know it"), Clinton's serious workfare (plus day care) initiative came to be easy prey to a defederalization of U.S. policy. Clinton had helped prepare the state-level understudies for AFDC.

In short, *Whose Welfare?* concludes that poverty reduction policy in the United States fell victim to the ideal (i.e., ideological) interests of the U.S. political elites and to the potent symbolic uses that welfare policy rhetoric had for these elites' electoral contests. Clinton made efforts to mobilize the centrist, work-centered consensus, which had fallen victim to political gaffes and bad timing. However, the opportunity for a coherent federal policy solution to the welfare mess was lost. Teles culturally turned democratic-elitist perspective should be highly suggestive to political sociologists, sociologists of the welfare state, sociologists of culture and public opinion, as well as to political scientists and historians specializing in American public policy. Its timely historical account of AFDC should interest the entire reading public of persons interested in U.S. social policy.

Through My Own Eyes: Single Mothers and the Cultures of Poverty. By Susan D. Holloway, Bruce Fuller, Marylee F. Rambaud, and Costanza Eggers-Pierola. Cambridge, Mass.: Harvard University Press, 1997. Pp. x+245. \$35.00.

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Cultural stereotypes about the pathological "dependency" of poor single mothers permeated the outburst of hand wringing over declining family values that fueled the national backlash against welfare. *Through My Own Eyes* hopes that conveying the perspectives of actual poor mothers to "ensure that mothers and their young children living at the edge of poverty will no longer be faceless strangers" (p. 2) will help to diminish public hostility to welfare recipients. Positioning themselves between what they regard as structural economism on the left and individualistic moralism on the right, the authors seek to integrate cultural and structural analyses of poverty.

A team of psychologists and sociologists collaborated on this qualita-

tive study of the values and meanings of parenting and work among poor mothers living in deprived neighborhoods in Boston. During a three-year period, three authors conducted extensive semistructured interviews with 14 Latina, Anglo, and African-American mothers of young children who had received some AFDC support. The fourth author studied corresponding views held by teachers in neighborhood preschools. Honoring the popular humanist commitment to "give voice" and agency to research subjects, the book portrays these women as self-conscious actors whose culture is neither completely determined by structural constraints nor the product of mindless replication of tradition. Rather, mothers "co-construct" childrearing values and strategies with those educators and social service workers who gain their confidence and treat them respectfully.

The authors creatively supplant that disparaged concept, the culture of poverty, with a pluralistic understanding of "competing cultures of poverty" (p. 5). They find significant variations in childrearing and work attitudes among poor mothers from even similar ethnic origins. Some mothers are sterner disciplinarians, others are more child centered; some have strong attachments to the labor force, others wish they could be home with their children. "There is no single culture of poverty. There are many," (p. 6) the book argues, and no one best way to raise children or to foster upward mobility. To succeed, antipoverty programs should aim to strengthen the "indigenous social foundations of women's lives" (p. 207). The book offers some creative policy recommendations to this end, such as tax incentives to reward valuable forms of intergenerational kin support. Along with cultural diversity, the study does find important common values among the women. For example, the mothers consider it culturally legitimate to resort to welfare for brief periods but share the public's disapproval of long-term "dependency." In fact, the majority found paid employment crucial for self-esteem as well as for income. Likewise, because the mothers view respect and respectability as entwined, they have both moral and instrumental motives for making teaching children respect one of their central childrearing goals.

Although this book makes argument against the formidable opinions of a hostile political climate, one can hope that its sympathetic rendering of low-income mothers will help to chip away at meanspirited images of welfare recipients. Unfortunately, moralists on the right will be able to find sufficient flaws in the study to rationalize dismissing its humanist message. Most serious are flaws in the definition of the study's sample. The book never makes clear whether its subject is poor mothers or poor, *single* mothers, as the subtitle proclaims. In fact, not all of the mothers *were* single; four of the 14 were cohabiting with their children's fathers when the study began. Moreover, the sample is skewed toward the working poor, most of whom "came from, and remained embedded within, strong extended families" (p. 103). Indeed, "these mothers relied extensively on their network of families and friends to augment their own efforts" (p. 103). Such a sample builds in its comparatively positive outcomes, because as researchers like Elaine Bell Kaplan (*Not Our Kind of*

Girl [University of California Press, 1996]) demonstrate, such networks are both especially valuable and much rarer among young black single mothers than was reported earlier.

Many on the left, in contrast, will take exception to the book's unwittingly patronizing stance. At root, this is a work of cultural translation written by and to some of "us" about and for some of "them." Inherent in the effort to "give voice" to the women, paradoxically, is the presumption that it is theirs (ours) to give. At moments, the authors' democratic veneer slips revealing an allegiance to the social engineering prerogatives of the professional middle classes: "If *we* are to improve the odds that these women will rise out of poverty, . . . *we* must devise more effective ways to guide them along *preferred* paths" (p. 204; my emphasis). Thus, although one could use this book in undergraduate sociology courses on social welfare or poverty, it will likely find more receptive audiences among communitarian politicians and in courses offered by social work, public policy, and education schools. A more accurate title might be *Their Words through Our Own Eyes*.

Culture Moves: Ideas, Activism, and Changing Values. By Thomas R. Rochon. Princeton, N.J.: Princeton University Press, 1998. Pp. xix+282. \$39.50.

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Social and political changes are natural to modern life. One only need compare, as Thomas Rochon does, today's society with that of one's youth to realize how much it has changed and how rapidly these changes have come about. In his well-written book on cultural change, Rochon takes on the difficult task of finding some answers to the question of where these tidal waves of change come from. In his view, many social and political transformations are responses to the recurring changes in the cultural values typical of modern society. And so he probes the origins of these cultural changes and the processes by which the altered values are diffused. His study is organized into three parts and contains an attractive mix of theory and empirical findings throughout. The theoretical elements come from a variety of books and papers on culture, cultural change, and social/political movements and from the data of important case studies such as the ERA campaign, the Civil Rights movement, and the Hill-Thomas hearings.

Rochon's basic argument rests on his assessment that political and social transformations come about because people are no longer satisfied with the situation in which they live and that they would like to have that situation changed. This demand for change proceeds from the fact that people have come to view "things in life" differently or, as Rochon puts it, because their cultural values have changed. This change of val-

ues—the root of rapid social and political transformation—does not come about automatically but is the product of a two-stage process of value generation and diffusion. This process is elaborated in the first part of the book, which begins with the concept of “cultural values.” These are seen as temporally and spatially bound ways of thinking about what is right, what is natural, and what works. Changing such values does not mean merely adapting them to altered social conditions but fundamentally transforming the basic categories of thought and perception that make people “think in a different way” (p. 21).

Such changes, however, will only occur if familiar patterns of thought are called into question. At this point, Rochon introduces a very valuable concept: the critical community. This term refers to a (primarily informal) network of critical and relatively independent thinkers who initiate discourses about situations they designate as problematic. In addition to problem identification, they analyze the sources and provide solutions, thus generating new ways of looking at familiar situations. Their ideas may become new cultural values if they are brought to a wider audience and become accepted as valid points of view. The diffusion of new values is the work of social and political movements as they attempt to bring about cultural as well as structural changes. Movements do not only confront established society with the ideas of the critical community, they also reformulate and repackage them in formats that are suited for mobilizing people into action. Movements make their own crucial contribution to change in society by modifying ideas into issues and, what is more, by transforming the particular discourse within the critical community into a public discourse about such issues. Rochon completes his theoretical perspective with an analysis of the different ways in which new cultural values become accepted in society.

In the following parts of the book, Rochon develops in more detail his ideas on cultural change and the agential role of critical communities and movements therein. First, he examines how people are brought to participate in collective actions (i.e., by creating and strengthening group solidarities) and how these actions are made effective by instilling political will and the learning of political skills. Second, he analyzes how structural changes have altered the conditions in which critical communities and movements grow and prosper. Finally, in the closing chapter, Rochon unites these lines of reasoning and considers what his study reveals about the direction in which modern society is heading.

By interweaving theory with empirical findings, Rochon arrives at a firmly grounded theory on one of the most intriguing aspects of modern life. Moreover, the structure of his reasoning and his lucid style make such difficult subjects like culture and change widely accessible and almost simple to comprehend. On the other hand, the attraction of using broad definitions of concepts like values and movements hinders making sharp and precise distinctions. Another less favorable aspect is that structural change depends, by definition, on changes in cultural values. This

would preclude finding that the causal sequence might run the other way around.

Nevertheless, Rochon has written an important book that is a very welcome contribution to the scholarly discourse on movements and culture, particularly because it focuses on the cultural effects of movement activities, a topic that deserves more attention than it has received since the cultural turn of movement studies. This book is a "must" for scholars in the field of cultural studies and social movements and will be very helpful to students of these subjects.

The Art of Moral Protest: Culture, Biography, and Creativity in Social Movements. By James Jasper. Chicago: University of Chicago Press, 1998. Pp. xv+514. \$35.00.

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Once in a great while a book comes along that fundamentally changes the ways we think about a topic. I am confident that James Jasper's deeply theoretical and richly illustrated *The Art of Moral Protest* will have such an impact on social movements scholars. Indeed, its impact could extend well beyond a single substantive area to influence the way sociologists view structure, culture, and agency and the relationships among them. Few writers since C. Wright Mills have so cogently articulated the intersection of social forces and biography.

Perhaps the greatest single contribution Jasper makes is to bring full-fledged human actors back into the spotlight of social movement analysis. These are not the irrational and apprehensive individuals of the crowd theories who mill about mimicking one another or who are occasionally whipped into a collective frenzy by the vicissitudes of rapid social changes. Nor are they the hedonistic, mostly self-interested, profit maximizers of the rationalist and mobilization theorists. And Jasper's movement actors are certainly not relatively helpless pawns of their political and economic environments as the process theorists often imply. Nor are movement actors suffering, as some new social movement theorists suggest, from a postindustrial-induced identity crisis. And finally, they are not simply the dispassionate, strategic manipulators of public discourse and meanings, as often implied by framing theorists. While Jasper acknowledges that under some conditions movement actors may in fact respond in one or more of the foregoing ways, they tend to be much more complicated and multifaceted than classical and contemporary movement theorists depict them.

Movement actors, according to Jasper, are thinking, artfully creating, feeling, moralizing human beings. They are thinking actors who behave strategically and artfully, aware of what they are doing, making plans,

developing projects, and innovating in trying to achieve new goals, all the while learning from their mistakes as well as from the mistakes of their opponents. But they are also feeling beings, whose protests are motivated by anger, fear, dread, suspicion, indignation, outrage, and hope, among other emotions. Far from rendering their actions "irrational," emotions supply much of the motivational impetus for individual and collective action.

And most of all, movement actors are "moral" beings. Their protests are frequently inspired by "moral outrage" sometimes from experiencing a "moral shock" such as news of the catastrophe at Three Mile Island. Protesters' subsequent actions are typically predicated upon moral principles as are their critiques of the conditions they wish to alter and their visions and hopes for a better society. In one of many provocative passages, Jasper asserts that moral protest "provides individuals with a rare chance to probe their moral intuitions and articulate their principles" (p. 367). He observes that contemporary institutions provide few opportunities for exploring, voicing, and pursuing moral visions. Indeed, for Jasper, the "importance of protesters . . . lies more in their moral visions than in their practical accomplishments" (p. 379).

The second significant contribution Jasper makes is to synthesize various concepts associated with cultural/constructionist perspectives, while selectively drawing on constructs from the more established resource mobilization, political process, and new social movements perspectives as well as literature from psychology, philosophy, anthropology, history, political science, and communication studies to creatively fashion a holistic, compelling approach to analyzing protest dynamics. In constructing this synthesis, Jasper critically assesses the major theoretical approaches, beginning with classical theories and ending with various contemporary paradigms. This is not the ritualistic exercise in "theory bashing" consumers of social movement monographs and articles have grown accustomed to reading. Rather, for each theory, Jasper carefully identifies not only the problematic dimensions but also its enduring contributions. A recurrent theme in this critique is the idea that many of the field's core concepts suffer from theoretical and empirical "overextension" (p. 41) due in part to the fact that our "main paradigms are surprisingly metaphorical" (p. 17). Resources, political opportunity structures, collective identities, and framing are all asked to do more work than is warranted. Jasper insightfully specifies each construct's limitations and the contexts in which each would seem to be applicable.

Jasper then identifies four basic, that is analytically autonomous, dimensions of protest: resources, strategies, culture, and biography. After demonstrating the essentiality of these four constructs, he explains why one contender, "structure," is analytically reducible to culture and resources. Once the four dimensions are fused with "artfulness" (agency), they can be analyzed dynamically rather than statically. Most of the remainder of the book is an elaboration of the interrelationships among the basic dimensions, often richly illustrated from case studies of the anti-

nuclear power, animal rights, and environmental movements Jasper and various collaborators have spent the past two decades studying.

The Art of Moral Protest defies adequate description in such limited space. Readers will find something of interest on almost every page of this well-written monograph—cogent observations, theoretical insights, provocative assertions, original research hypotheses, and pearls of wisdom. Serious students of social movements should place it at the top of their reading list. Beyond that, I highly recommend that all scholars interested in the human condition partake in this artful scholarly creation.

Agrarian Reform and Class Consciousness in Nicaragua. By Laura J. Enríquez. Gainesville: University of Florida Press, 1997. Pp. x+206. \$49.95.

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CEIICH-UNAM

Agrarian reform was a most relevant ingredient in the Sandinista strategy for revolutionary change. With regard to the peasantry, it was addressed to the fulfillment of three basic, interrelated objectives: furthering economic development through both productive differentiation and the promotion of cooperative organization; improvement of peasants' well-being by means of access to credit, productive inputs, technical services, and so on; strengthening peasant's political support to Sandinismo. Enriquez discusses the performance of the reform along these three avenues. She focuses on the shifts in the Sandinista regime's approach to the role of peasants in economic development and the impact of these shifts upon peasants attitudes toward both the government and the opposition. Two additional ingredients played a decisive role in the development of the peasantry's political attitudes toward Sandinismo, which Enriquez also deals with in detail: the class origins of specific segments of the peasantry (i.e., poor and landless peasants, minifundista peasants, and others) and the type of productive organization (collective ownership of land as well as of production; individual/family ownership of land together with cooperative management of credit, commercialization, or specific inputs; and so on) promoted by the revolutionary government.

Through two case studies, Enriquez concludes that, despite the agrarian reform's economic success (output growth and differentiation, technical improvements, and so on), its ability to feed political support toward Sandinismo was mostly confined to former poor and former landless peasants, while small producers, enjoying some access to land prior to the reform, tended to be less politically enthusiastic. In turn, the specific types of peasant organization, pushed forward by policy makers, acted in different segments of the peasant class to favor either an increased political involvement in the revolution, a pragmatic acceptance of specific policies while rejecting others, or an increasing shift toward political opposition.

In spite of Enriquez's attempt to link her subject to processes of transition to socialism—an option that she accepts to be quite controversial—her conceptual discussion refers to the relation between economic development and politics and to the role Third World peasantries tend to play with regard to radical socioeconomic and political change. Enriquez contends the hypothesis of an essentialist conservative bias in peasants; her case studies show there is no political essentialism in class identities and that, in order to reach relevant conclusions, differentiations inside the peasantry—in terms of access to land and other resources, as well of the specific histories of particular segments of the peasantry and the institutional environment where these factors operate—have to be acknowledged. The discussion of “plan Masaya” (chaps. 5 and 6) points to the many elements prompting for a strengthened political commitment of peasants to the revolution, while her presentation of the “Los Patios” project (chaps. 3 and 4) stresses the factors conducive to a conservative reorientation of the project's beneficiaries—in spite of economic goals of the agrarian reform being achieved in *both* projects. Enriquez's conclusion points also to the relevance of effective peasant participation in processes of radical socioeconomic and political change in societies where the peasantry makes up a larger segment of the subaltern classes. Yet, the book does not explore the reasons why participatory democracy has confronted so many shortcomings in so many dimensions of the Sandinista agrarian reform. A discussion of the class extraction and ideological commitments of Sandinista policy makers drawing agrarian reform policies could have helped readers to understand the frequent conflicts between public officials and political leaders on the one side and peasants on the other.

While relating Sandinismo to transitions to socialism, neither reduces nor increases the relevance of Enriquez's discussion of the agrarian reform failures and successes, and the inclusion of “class consciousness” in the book's title is misleading. What one sees is a number of peasant groups that share a basic attachment to land as the means to survive and advance in life: some of them became strong supporters of Sandinismo, yet others moved toward political opposition. Furthermore, political cleavages or allegiances *do not* oppose different kinds of peasants. Enriquez finds Sandinistas together with oppositionists in each of the two projects; there are pro-Sandinistas among the small owners involved in Los Patios, as there are in more proletarianized beneficiaries of Plan Masaya, as much as there are also supporters of the opposition in both projects. Since there is no conceptual discussion of class consciousness and its role in the book's subject, the purpose of referring to it in the title is not clear.

Despite these marginal critical comments, the book is worth reading for graduate students with a basic knowledge of Nicaragua's recent history, as well as for readers interested in processes of structural change in peasant societies. It makes an important contribution to both peasant studies and the comparative analysis of revolutions.

Ethics and Activism: The Theory and Practice of Political Morality. By Michael L. Gross. New York: Cambridge University Press, 1997. Pp. xiv+305. \$59.95.

Laura J. Scalia

A man rushes into a burning building to free a child trapped inside. Certainly, if this rescuer's good conscience selflessly guided him, his act deserves to be termed moral. However, what if the child was the man's only son? Alternatively, suppose the saver was a fireman, handsomely paid to put out blazes and rescue trapped inhabitants, or suppose he was an aspiring politician, seizing a photo opportunity to enhance his chances of electoral success. Would the good deed still be deemed moral? Michael L. Gross, in his new book *Ethics and Activism*, asks us not to extend praise only to high-minded, principled acts. His rationale is simple: Most do not risk their lives for the good of abstract mankind; most seek to benefit themselves and their close friends when doing so-called "good deeds." Why not extend moral credit to this majority?

Gross's essential quest is to put a moral soul onto the actions of individuals like the father, fireman, and aspiring politician, who, though mostly concerned with themselves and their immediate relations, nonetheless do great deeds for society. Specifically, he studies to what extent ethical intentions motivate ordinary folk to participate collectively in those everyday causes that invigorate and preserve democracy. To do this, Gross examines two moral conceptions, which he then thoughtfully operationalizes by integrating aspects of social, psychological, and rational choice theory. These interdisciplinary tools are subsequently used to determine which moral construct better explains popular engagement in just causes. Underlying the analysis is a normative aim: "to develop a theory of political morality consistent with empirical evidence" (p. 18). To the author, "Knowing how individuals can act helps us understand how they ought to act" (p. 19). Given his overall methodology and normative aspirations, Gross deserves the ear of empirical theorists, especially those interested in collective action and moral philosophy.

Part 1 discusses two views of morality: the strong, which assumes only principled actions deserve to be termed moral and which Gross seeks to overturn as empirically naive, and the weak, which deems interests and localized concerns viable bases of moral action and which the author sees as realistically reflecting human behavior. Part 2 operationalizes these views, outlining alternative motivational theories. (Herein are scattered helpful figures explicating how different factors are thought to influence political action.) Among the factors described are incentives that rational choice theorists emphasize, including monetary and solidarity incentives as well as normative, nonmaterial stimuli. Also incorporated are influences that cognitive psychologists stress, including a person's way of morally reasoning, from dogmatically choosing as self-interest and authority dictate, to selecting provincially or nationalistically as a good community

member or citizen might, to autonomously relying on universal principles that transcend communal norms. All variables seek to measure whether activists are morally weak or strong, self-seeking or high-minded.

Part three carefully examines specific cases of activism: Dutch and French rescuers of Jews during World War II, Americans in the pro-life and pro-choice movements, and Israeli supporters of retaining or returning occupied lands. Each chapter provides brief but adequate background about the ethical cause and its historical particularities. Each looks at the participants themselves, their cognitive level of moral development, the available social networks, and the obtainable incentives accepted. Although differences exist, in each case, the vast majority of political activists are found to be weak moralists, cognitively parochial or nationalistic, motivated by monetary and solidarity incentives.

These findings certainly illustrate that even individuals engaged in deeply ethical issues mostly fail to satisfy the model of strong political morality. Less obvious is whether that discovery merits rejecting the model. First, the empirical evidence is somewhat biased. The book examines rank and file members, whom elite theorists would consider followers, not society's best guardians. Gross never investigates the motivations of leaders: the initiators of underground rescue, the original mobilizers for and against abortion, or the first organizers for Middle East peace. He analyzes why *ordinary* individuals join and remain attached to *established* organizations and causes, not what drives founders to cultivate ethical movements. If originators were the selfless, principled, autonomous agents of strong moral theory, then perhaps the model is less empirically naive than the author suggests.

Moreover, the normative justification for redefining morality is thin. In his quest to have reality drive theory, the author gives selfish and selfless intentions the same legitimacy, claiming that in politics an action's good consequence is all that matters. But, is outcome really the only significant issue? An accidental killer is not deemed evil, yet the intentional murderer deserves our strongest reprobation. Malicious intentions can make acts immoral; should not good intentions also impact judgment? Though the author might agree, his call to reconceptualize political morality so that only the efficacy of action matters asks us to divorce intentions from actions, something moralists may find troubling. Troubling or not, Gross's analysis forces readers to rethink the issue and provides them with a novel, interdisciplinary framework for conceptualizing the bases of collective action.

The Sociology of Religious Movements. By William Sims Bainbridge. New York: Routledge, 1997. Pp. vi+474. \$74.95 (cloth); \$24.95 (paper).

Rhys H. Williams
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William Sims Bainbridge has produced a textbook, in two senses of the word. First, this is a text on the sociology of religious movements, suitable for graduate seminars and upper-division undergraduate classes. It is less survey coverage of the field than an introduction to religious movements through concepts and examples drawn from Bainbridge's own research and theorizing. Thus, it is perspectival, committed, and engaging.

Second, this is a textbook case of application of theory to data; Bainbridge begins with the "theory of religion," which he has developed in conjunction with Rodney Stark (roughly, a rational choice theory of religious motivation, based on the provision of supernatural "compensators" by religious groups), and applies it to the dynamics of a series of religious movements, such as the family, the holiness movements, and the contemporary new age. As a text (in the first sense), Bainbridge's book will be useful to many scholars. There is a wealth of empirical data, from GSS analysis to ethnographic to historical material. As a textbook application of theory, readers will respond based on their orientation to the grounding assumptions; as the Stark-Bainbridge theory is central to much of the rational choice work currently controversial in the sociology of religion, the response will no doubt be divided.

After an initial chapter that lays out the basics of the guiding theory, the book is divided into three sections, covering the dynamics of "schism" (sects formed from divisions within religious bodies), "innovation" (cults formed as innovative new religions within their cultural contexts), and "transformation" (dealing with and changing the societal environment). Each section has an initial chapter covering a general topic pertinent to the section's theme, followed by three chapters of empirical examples.

The initial orienting chapters in each section are a bit idiosyncratic as Bainbridge only glances over the literature before developing his own ideas about the issue in question. The results are uneven—the examination of church-sect theory works well for the section on schism. However, a lengthy excursion into Watergate (and another on the *Star Wars* trilogy in the conclusion) did not reward the space allotted. As is often true with rational actor models, individual-level analyses continually rise to the fore. For example, the chapter on "cultural diffusion" (orienting the "innovation" section) spends most of its attention on conversion, and the chapter on "morality" (orienting the "transformation" section) discusses the effects of religious beliefs on controlling deviant behavior such as substance abuse, larceny, and suicide.

In the empirical chapters, Bainbridge is interested in religious movements because they represent a special form of religion, rather than being interested in religious movements as a special case of social movements

generally. As a result, he draws few insights from the burgeoning scholarship on social movements and collective action. For example, the chapter on "democratization movements" discusses whether religion promotes or retards democratization, rather than covering the dynamics and history of particular democratization movements (or the genre).

The concluding chapter argues that religion will continue to renew itself into the future through the phenomena of movements. When institutional religion loses touch with the particular needs that initially drew its adherents, it loses its tension with its host culture and undercuts member motivation. Religious movements then arise to provide the missing passion, innovation, and tension with the culture. Thus, religion itself is a "perpetual system" that provides the supernatural compensators humans need to survive. Bainbridge also discusses the factors, both internal and external, that shape the success of religious movements, defined as organizational growth and continued member commitment.

I must note that the final chapter also engages in some tiresome competitive comparisons between the so-called new and old paradigms in the sociology of religion. The new paradigm is represented here by the Stark-Bainbridge theory of religion, while the old paradigm is a caricature of secularization theory, represented by a 1950s-era article by A. F. C. Wallace (I suppose ensuring its status as an *old* paradigm). Then Bainbridge runs through a series of regressions on data on religious membership, church growth, and religious variety. New paradigm-based predictions are pronounced "winners" when they explain more variance than old paradigm assumptions.

The usefulness of this exercise eludes me. I do not understand what is gained for sociology as an intellectual enterprise through this wins-and-losses logic. Further, the credit that might accrue to the Stark-Bainbridge theory is undercut both by the simplistic version of secularization theory used as foil and by the overidentification of the new paradigm with rational choice models. There is something of a chest-thumping tone to the effort. One wonders to what end.

William Bainbridge has much to teach us about the emergence, success, and dynamics of religious movements, and this book is rich reading. Of course, those not persuaded initially by Bainbridge's approach may well remain unconvinced, and that should be a net gain to the field—whether developing or disputing this work, future research is sure to follow.

End of Millennium, vol. 3 of *The Information Age: Economy, Society, and Culture*. By Manuel Castells. Malden, Mass.: Blackwell Publishers, 1998. Pp. xiv+418. \$69.95 (cloth); \$29.95 (paper).

John Boli
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We live in the network society. Relationships of production have shifted in fundamental ways, with flexible production and network-based global structures becoming ever more central. Production is primarily informational: the control, manipulation, and distribution of information as both product and means of organizing other types of products. Power has become primarily a matter of symbolic manipulation; elites are ephemeral and situation specific, while classes decline in significance. In opposition to the Net (the realm of "real virtuality" generated by globally interlinked nodes of informational production) stands the self, that is, the individual defined and self-defined increasingly in terms of primordial identities (gender, race, religion, ethnicity) and engaged in identity movements that have become the central arena of political struggle in this end-of-millennium time.

Such are the claims of Manuel Castells, a polyglot and much travelled, productive scholar who in many ways epitomizes the global information producers conceptualized in his analysis. Castells knows the world well, and his sharp observational sense has been honed through extensive field work in numerous countries. In this book, the third wing of his sprawling, complex edifice that attempts in exceedingly ambitious form to "make sense of our world," he draws out the implications of the analysis in the first two volumes (*The Rise of the Network Society* and *The Power of Identity* [Blackwell, 1996 and 1997]) for major political and economic changes in recent decades.

The book ranges widely, from a detailed dissection of the collapse of the Soviet Union (and, by extension, of the failure of "statism") to analyses of the immiseration of the "socially excluded" (the "Fourth World" that includes pockets of misery in the developed countries), the rise of global criminality, the Asian economic surge led by the "developmental state," and European unification as a defensive response to American and Asian economic domination. All of these topics are handled with great authority and considerable insight.

Despite Castells's prodigious knowledge and effort, I find it difficult to know what to make of this book and of the three-volume series as a whole (readers are advised at least to skim the first two volumes; volume 3 does not stand easily alone). In a blurb on the back cover of the paper edition, Giddens does Castells the disservice of unwisely suggesting comparison with Weber's *Economy and Society*. Against that extraordinary work, *End of Millennium* hardly measures up; Castells offers little of the institutional and historical incisiveness so characteristic of Weber. Castells finds his main inspiration in the work of Daniel Bell and Alain

Touraine on postindustrialism, along with Nicos Poulantzas's neo-Marxist writings. To my mind, the book also recalls Galbraith's *The New Industrial State* (New American Library, 1967), and volume 2 clearly builds on Roszak's *The Making of a Counter Culture* (Doubleday, 1969). The series is, then, a grand-sweep analysis that is decidedly contemporary and clearly intent on coming to grips with emerging and increasingly dominant trends in the social development of the past three decades. For general readers, and even many specialists, it has much to offer in this high-level sense.

Yet, *End of Millennium* and its companions are a good deal less illuminating than I had hoped. Castells warns us early on that he chose to eschew grappling intensely with the theoretical and empirical complexities of the many literatures upon which he draws, and that choice has regrettable consequences. In treating religious fundamentalism, for example, he accepts without question the Martin Marty line that "fundamentalists are always reactive, reactionary," so the insights of alternative interpretations of fundamentalism are ignored and a too-neat-and-tidy view of fundamentalism emerges. The same sort of problem characterizes Castells's treatment of identity movements—they are interpreted as either reactions against the impersonality, vapidness, power, and voracious appetite of the Net or as proactive efforts to construct various forms of idealized community (nonpatriarchal, pristinely natural, ethnically authentic . . .) outside of or as protection against the informational core. That identity movements might be about such boring perennial issues as gaining power and wealth, the imposition of quasi-religious beliefs on others, or rampant individualism raised to an obsessively narcissistic level are interpretations that go begging here.

What bothers me most about *End of Millennium* specifically is that the five substantive chapters generally lose sight of the core ideas developed in volumes 1 and 2. For example, while informationalism is the dominant theme of Castells's analysis, it is prominent in volume 3 only with respect to explaining the collapse of statism, which Castells shows to have been unable to adapt to informational production and application. In the other chapters, informationalism is hardly visible.

Overall, Castells is much less explanatory than descriptive. His descriptive material is often fascinating, but his scholarly contribution would be greater if he had made the effort to explicate the causal chains underlying his expositions and give his readers guideposts to a general theory of informationalism. While he often claims to be developing hypotheses about events, at the end of the day one has great difficulty identifying any clearly testable statements in his analysis.

End of Millennium and its companion volumes are decidedly worth a read, for Castells has an impressive grasp of the contemporary world. As an original contribution to knowledge or a stimulus to further scholarly research, however, they are a good deal less impressive than their scope and ambitiousness. Food for thought, but readers will need to do much of the thinking.

Contested Countryside Cultures: Otherness, Marginalisation and Rural-ity. Edited by Paul Cloke and Jo Little. London: Routledge, 1997. Pp. viii+295. \$85.00 (cloth); \$25.95 (paper).

Michael Mayerfeld Bell
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The countryside is becoming more interesting. Beset for decades by the twin modularizing modernisms of industrial agriculture and positivist rural social research, the landscape of rural life had been determinedly developing into a monotonous monoculture. Those monocultural tendencies are still with us, particularly with regard to agriculture's continued bigger-is-better beggaring of the rural economy and the rural environment. But rural society is taking on a more diversified appearance—at least in terms of its representation by a more diversified rural studies literature more attuned to the margins and the differences and the othernesses contained within the rosy (albeit contradictory) idylls of a mechanized-and-transgenetic countryside or a Babe-and-Farmer-Hogget countryside. As *Contested Countryside Cultures* wonderfully shows, rural social research is becoming less of a monoculture.

This welcome volume by a group of 15 British geographers illustrates the renewed diversity of rural studies in two broad ways. The first I have already alluded to: the field's recognition of the diversity of its topic. *Contested Countryside Cultures* adds to a growing literature on rural difference and its representations and misrepresentations, a critical literature that intervenes in the flow of rural images and inspects the power relations that shape them. Much that rural researchers had taken for granted is now coming into the foreground: the cultural construction of nature, the politics of space, the experience of place, the ideology of rurality. And as well, the diversity of people who live a life they or others deem rural is also coming into the foreground, what Paul Cloke and Jo Little in their introduction to the volume describe as "hidden others" in what is sometimes perceived as a monolithic stronghold of the white, the nationalist, the Christian, the homophobic, and the patriarchal.

In other words, there is now postmodern rural research too. This postmodern turn has also been largely responsible for the second form of diversity now emerging in rural studies, a diversity of method. Here again, *Contested Countryside Cultures* exemplifies the trend. Flipping through its 295 pages, the reader encounters nary an equation or a graph and only a few tables. Rather, the contributors to the volume mainly employ the wide range of ethnographic and textual approaches that postmodernism and cultural studies have ushered back into the social sciences, now welcome in rural studies too.

The contributors to *Contested Countryside Cultures* are all at British institutions, and most of the chapters deal with British case material. A few chapters also consider the United States, but no other countries receive sustained attention. Nevertheless, the themes of all the chapters

have much to inform a North American audience. The book begins by furthering Chris Philo's debate with Jonathan Murdoch and Andy Pratt over what postmodern rural studies should look like, a debate that began in the early 1990s in a series of articles published in the *Journal of Rural Studies*. Philo again advances his view that rural studies needs, without political premeditation, to open itself truly to the possibility of "rural others" and "other rurals," giving here the example of the unusual rural vision of the Shaker movement in the United States. Murdoch and Pratt then repeat their caveat that the study of rural others must also consider "how these 'Others' came to be 'Othered'" (p. 55)—that rural studies needs to attend to power relations and to the possibility of political engagement—and go on to argue that, like power itself, rural identities need to be understood as "fluid" and provisional not eternal or inevitable.

Subsequent chapters present a series of challenges to a stable view of rural life, showing both something of the range of hidden rural others and of the constant contestation of their identities. Annie Hughes and Jo Little introduce us to the identity struggles of British rural women; Gill Valentine revisits the lesbian separatist rural communities of the 1970s and 1980s United States; Julian Agyeman and Rachel Spooner recount the marginalization of blacks and Asians in the British countryside; David Sibley critiques public order legislation that controls the movement of Gypsies, new age travelers, and hunting protestors; Paul Cloke lets us hear the voices of the rural poor of Britain and the United States and discusses their conflicting imageries; Sarah Harper and Clare Fischer remind us about the lives of, respectively, rural elderly and rural craft workers; Owain Jones and David Bell describe the representation of rurality in children's literature and in the American horror film; Keith Halfacree invites us to consider even the counterurbanite commuter as a kind of other.

The book thus demonstrates much of the promise of postmodern research—and some of the pitfalls. There seems to be a tendency here to regard nearly everyone as in some way neglected and misunderstood, as a marginalized other—even counterurbanites—watering down the power perspective that underlies postmodern cultural studies. There also seems to be a potential to not merely give "voice" to others but to create them to begin with and to gain academic standing from their exoticness. In the concluding chapter, Cloke and Little give us a sensitive discussion of this latter problem, what they term "research tourism."

But the promise of restoring to view those commonly overlooked is nonetheless admirably accomplished in this book. Indeed, rural studies in its own way has been something of a "hidden other" within the social sciences. Like the half of the world that still lives in rural areas, including a quarter of the population of the rich countries, rural social research has often been overlooked by an urban-oriented academe. There is some interesting stuff happening in rural studies these days, though, and this book is an excellent illustration of that trend.

Sociology on the Menu: An Invitation to the Study of Food and Society. By Alan Beardsworth and Teresa Keil. New York: Routledge, 1997. Pp. x+277. \$69.95 (cloth); \$19.95 (paper).

Consuming Geographies: We Are Where We Eat. By David Bell and Gill Valentine. New York: Routledge, 1997. Pp. ix+236. \$69.95 (cloth); \$19.95 (paper).

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In explaining why, until very recently, food and eating have received less analytical attention from sociologists than from scholars in neighboring disciplines, Alan Beardsworth and Teresa Keil suggest that, paradoxically, one reason may be that the very centrality of eating to human life has made it relatively invisible to the principal analysts of society. Whether or not this is so, *Sociology on the Menu* has been designed to make visible the complex of activities and relationships surrounding the human food system, from production to consumption, as well as to highlight the contributions made by sociologists to our understanding of its social and cultural dimensions. This is not a modest pair of objectives but objectives that are mostly attained by a book that manages to sustain a certain analytical density while successfully avoiding the arid, encyclopedic quality of most textbook introductions.

Organized into four parts, the book consists of 11 chapters that break down fairly evenly between those areas that would be obligatory in any attempt to reasonably cover such a field (i.e., "The Origins of Human Subsistence," "The Making of the Modern Food System," "Eating Out," and "Changing Conceptions of Diet and Health") and those areas that seem to have been animated by the particular intellectual and political commitments of the authors (a strong emphasis on gender inequality throughout, and specific chapters on "Food Risks, Anxieties and Scares," "Dieting, Fat and Body Image," and "The Vegetarian Option"). Each chapter includes some critical recapitulation of the recent sociological research related to the specific topic, and data from the United Kingdom tends to predominate (while this very well may be the result of more and better research on the relationship between food and society in the United Kingdom, it may also have something to do with the nationality and institutional location of the authors). A chapter devoted to the principal theoretical perspectives offers a useful review and assessment of the contributions made not only by the most well-known theorists (such as Lévi-Strauss, Mary Douglas, Jack Goody, and Norbert Elias) but also by those like Stephen Mennell and Claude Fischler, who may be less widely known outside the field of food and foodways but whose work has been very important in helping us to understand it.

Although the authors assert "change and ambivalence" as the thematic thread linking all of the chapters, this does not really contribute very

much in analytical terms (after all, what domain of social activity is *not* characterized by “change and ambivalence”?) and mostly seems like an attempt to override any perceived imbalance resulting from the dual pressure on the authors to both cover a wide area and to pursue their more focused interests. This is unnecessary, since the very subject matter provides ample thematic coherence, while the authors display a sufficiently firm grip on the principal theoretical perspectives and empirical studies on food and society to give the reader confidence that if some issues have been treated more or less exhaustively than others, nothing really crucial has been left out.

Sociology on the Menu succeeds in its main purpose of introducing the reader to the primary sociological literature and thinking in this area. But the authors have a secondary goal, that of advancing the status of the sociology of food and eating as “an important and distinctive project” (p. 257). By demarcating its intellectual boundaries, they seem to be seeking to establish food and eating as a distinctive subfield. But besides adding a degree of needless severity to the work, what they actually have shown is that many of the key questions tend to be generated by and within a number of existing subfields of sociology (culture, development, gender, class, political, and environmental sociology; themselves rather arbitrary designations). Whether or not their book elevates the status of food and society, it has identified it as a valuable and important area of concern for the discipline.

Less encumbered by these sorts of concerns, David Bell and Gill Valentine have written a book that, while covering some of the same ground as Beardsworth and Keil, shows little respect for either disciplinary boundaries or academic conventions. Beginning with the glossy image on the book’s cover—the distorted head of a fat, loutish-looking man, blissfully licking the whipped cream that is smeared about his mouth and chin—we know that *Consuming Geographies: We Are Where We Eat* has entered us into a world of stylish (some might say, stylized) irreverence. The playfulness extends to the satiric photographs and advertising images smattered throughout the text, as well as to the “favourite recipes” placed at the beginning of every chapter, each contributed by a member of a group of “the most cited” geographers. Despite the appearances, there is definitely a “there” there, as the authors (lecturers in the United Kingdom in cultural studies and geography, respectively) have fashioned their analytical lens from some of the best materials in both fields to examine eating practices as a way of thinking about consumption, identity, and place.

The book is structured according to “spatial scales”: the body, the home, the community, the city, the region, the nation, and the global. Various aspects of food consumption are examined at each of the scales, or levels, as a way of constructing an account of the “circuits of culinary culture as they map across space” (p. 12). So, for example, the chapter on the body examines social and cultural practices related to such issues as body size, dieting, eating disorders, bodily pollutants, the eroticism of

food, and the body as cultural capital. The chapter on the city considers various urban food rituals, including restaurant consumption as entertainment, as performance, the varieties of eating places, fast food and homogenization, and the culture and system of the contemporary supermarket. While the authors' specific analytical points often lean toward the "postmodern," they also frequently summarize systematic research findings when it is relevant to their analyses, and so the book provides one with a reasonably good sense of what sociologists have been learning with respect to contemporary food consumption.

Throughout, the authors engage in a sort of theoretical bricolage, drawing ideas from the sources that seem relevant at the time (Mary Douglas or Pierre Bourdieu here, Michel Foucault or Arjun Appadurai there) and have interspersed, at various points in the text, boxed excerpts, several paragraphs long, from raw interviews (i.e., "'Jackie,' who is 43 and a lone parent. She has two adult daughters and an 11-year-old son" [p. 33]). The extended quotations, many of which have a confessional quality, are implicitly "played off" against the theoretical analyses offered by the authors and the theorists they are working through. Although one can imagine some readers becoming irritated by the authors' meandering style (they often seem to wind their way in no particular direction) the structure of the book serves to contain their movement to some degree and is generally quite effective. Plus, the authors happen to be concerned with issues that most sociologists will find interesting and important. *Consuming Geographies* is provocative, significant, and enjoyable.

Changing Financial Landscapes in India and Indonesia: Sociological Aspects of Monetization and Market Integration. By Heiko Schrader. New York: St. Martin's Press, 1998. Pp. 293. \$59.95.

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DePaul University

In this study, Heiko Schrader attempts to account for the uneven development of financial institutions, formal and informal, in India and Indonesia from the precolonial era to the present. The central thesis is that a world economy requires a more developed network of financial institutions near the center than it does closer to the periphery. Hence, for example, Mughal India, which was more central to the precolonial Asian world economy, had a far more developed set of financial institutions than did precolonial Indonesia. Similarly, colonial India, as a colony of a core power of the capitalist world system, continued to develop an intricate set of financial institutions, while the Dutch East Indies, attached to a far lesser power, developed a far more truncated set of institutions.

Clearly, for Schrader, the key theoretical point of reference is Immanuel Wallerstein, but the design of this book breaks with the monistic view of a single world system. This break is methodological rather than theo-

retical, for there is in this book still a sort of functionalist determination of the last instance: the world system gets the financial institutions that it needs, where and when it needs them. But by conceding a relative autonomy to geographically delimited "subsystems," Schrader is able to proffer a causal rather than a functional account of the emergence of those institutions. The long synthetic essays on India and Indonesia at the heart of the book in which those accounts are elaborated are the book's best feature: lucid, economical, focused, and for the most part convincing. Particularly noteworthy in these sections is an extraordinarily fine-grained appreciation of the interpenetration and meshing of formal and informal financial institutions. By skillfully deploying an array of theoretical categories developed, perhaps at too great length, in the introductory section of the book, Schrader is able to avoid reifying the notions of formal and informal sectors without slipping into mere ad hoc description.

At the end of the book, though, the theoretical tension is not entirely resolved; it is not altogether clear whether Schrader is arguing that India garnered a denser network of financial institutions because its place in the world system required it or whether India garnered a more central place in the world system because of its denser network of financial institutions. Most of the material in the empirical sections of the book points to the latter interpretation, the material in the concluding theoretical section to the former. Nonetheless, despite this unresolved tension, Schrader is largely successful in his avowed aim of showing how "contemporary financial landscapes and regional differences result to a high degree from processes in the past taking place both on the particular national level as well as on a higher, structural level" (pp. 37-38).

There is, however, a less explicit agenda in this work about which I have greater reservations: to demonstrate the centrality of financial institutions to capitalist development on a world scale. The tight focus on relations of exchange in this study often, I fear, slight the importance of the means and relations of production. One finds this uneven emphasis, for example, in the very able summary of the "Great Firm" theory of the Mughal Empire, the notion that the Empire (and its British successors) relied on a network of bankers who underwrote its expansion. One finds no similar discussion of scholars who give primacy to the Mughals' varying success in creating a patrimonial bureaucracy capable of collecting land revenue. Similarly, great emphasis is placed on the ability of the Dutch and British East India companies to tap into preexisting networks of merchant bankers, without considering that in this trade many were called but few were chosen. From the Portuguese on, the official European trading enterprise regularly found itself stymied by its own agents who found trading on their own account to be more profitable than loyally serving their firms. In the end, what distinguished both the British and the Dutch was their transformation from trading companies to quasi states whose revenues were based on agricultural production: land revenue in the British case, plantation agriculture in the Dutch. These colo-

nial enterprises succeeded because they made their money the old-fashioned way—they beat it out of the peasants.

In sum, then, this study is an interesting and unusual use of comparative analysis in a theoretical framework that heretofore has ill supported such work. Anyone who needs a convenient summary of the development of financial institutions over a very long period of time in either India or Indonesia will find this an accessible and for the most part reliable first stop. Those sympathetic to the longstanding critique of Wallerstein's "neo-Smithian Marxism," however, will find little in this study to change their view.

Making Capitalism in China: The Taiwan Connection. By You-tien Hsing. New York: Oxford University Press, 1998. Pp. xiv+250. \$35.00.

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In a time when scholarship on China increasingly relies on large-scale surveys of household income to examine general issues of economic change, it is refreshing to see a rigorous study that is driven by substantive issues rather than available data. Combining ethnographic research with in-depth interviews You-tien Hsing's study of Taiwanese investment in southern China is as deep and systematic as it is informative. The methodological appendix is impressive on its own: for this study, Hsing conducted 221 in-depth interviews with Chinese officials, Taiwanese investors, and Chinese managers and workers; she visited 40 factories in Southern China; and she worked on the assembly line for extended periods in two additional factories. The result is a study that extensively examines an important aspect of economic development in China.

In the opening pages of the book, you get the sense that this is simply going to be another of the studies that views China as a connections-over-all-else world and that business and economic development in China are completely structured around social ties, personal favors, the exchange of gifts, and corruption. However, throughout this nuanced and balanced analysis, Hsing does a wonderful job of weaving a tale of cultural particularism, institutional and historical contingency, and general social analysis. Hsing's central question revolves around how network practices of Taiwanese investors and local officials in Southern China are shaped by the institutional and historical conditions in which these actors are embedded. Hsing's argument essentially is the following: industry in Taiwan is heavily organized around industrial and social networks, especially among medium- and small-scale firms. In the late 1980s, just as local Chinese authorities—especially in southern China—were being given economic autonomy (as well as responsibility for meeting bottom lines) and just as Western investors were pulling out of China (in response to the Tiananmen Square incident), Taiwan was loosening restrictions

on its citizens visiting the mainland. With a common language and knowledge of the nuances of gift exchange in China, medium- and small-scale Taiwanese manufacturers brought their networking strategies to Southern China.

Two issues lie at the heart of the argument. First, a decentralized state that forges national policy at the center but relies on increasingly autonomous local administrations to carry out these rules has fundamental implications for investment practices at local levels. Local officials, who are under economic pressure to make ends meet, want to attract foreign investors to their jurisdictions, and the flexible implementation of the laws and policies that come from Beijing is the central bargaining chip they have to offer potential investors. The key is that investors and officials must keep these projects local and small—and thereby stay off of Beijing's radar screen—so the size of investments is a central part of the story. Second, though Hsing makes connections (called *guanxi* in China), a central part of the story throughout the book, her analysis of the role of connections in Taiwanese investment never loses sight of the extent to which the connections are shaped by institutional contexts. Hsing avoids the trap that many scholars of *guanxi* fall into—that *guanxi* is something particular to Chinese society and that it is rampant throughout China. Hsing repeatedly reminds readers that gift economies and connections are present in many societies and to varying degrees throughout China, and the interesting project for research is to figure out the specific institutional contexts that allow such practices to flourish. Hsing's answer here is that a history of network-based business practices among investors, common culture and language, and autonomy and economic pressure for local officials explain the prevalence of network-based investment practices among medium- and small-scale Taiwanese investors.

The weakness of the study lies in the attention to details: in three of the chapters, Hsing spends much more time giving us background details about different aspects of the structure of Taiwan's fashion shoe industry than she does using her own empirical research and data. While some of the information related in chapters 1, 2, and 5 is necessary, I would have preferred to have the discussion interspersed with direct information and insights about how the managers and investors Hsing interviewed view such issues as industrial networks in Taiwan or local autonomy in China. An additional weakness is a lack of familiarity with certain relevant literatures. While parts of the analysis draw appropriately on extant theories (e.g., the work of Mark Granovetter), Hsing eschews citations to relevant literatures in institutional sociology. For any study so clearly interested in the ways that institutional contexts effect economic action, this is an obvious lacuna, but the omission is especially noticeable in her discussion of "creative imitation," which would clearly tie into Paul DiMaggio and Walter Powell's theory of mimetic isomorphism. This is an unfortunate gap, because Hsing's balanced treatment of networks *and* institutional effects could actually make significant contributions in institutional research. Nevertheless, despite these gaps, this book is an excel-

lent study of investment in China and of patterns of international development more generally.

Portraits of the Japanese Workplace: Labor Movements, Workers, and Managers. By Kumazawa Makoto. Boulder, Colo.: Westview Press, 1996. Pp. xv+267.

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I first encountered Kumazawa Makoto in an article on "labor simplification," written several years before Braverman's de-skilling treatise and drawing on empirical and theoretical studies of Japanese workplaces dating back to the early 1960s ("Rodo tanjunksu no ronri to genjitsu" ["Theory and Reality of the Simplification of Labor"] in *Nihon rodo kyokai zasshi*, June 1970). It was an eye opener, both for what it said about the transformation of Japanese workplaces under rationalization and technological innovation at the time and for the current of scholarship it represented, empirically and theoretically rich but virtually unknown outside Japan. Andrew Gordon may well have had the same experience, and we must be grateful to him and Mikiso Hane for introducing Kumazawa's work in this book.

As Gordon points out in his introduction, Kumazawa's roots are in the school of scholars who took Western or British industrial relations and working class solidarity as a model and from that basis criticized Japanese industrial relations, especially the failure of the labor movement to chart a course toward that model in the postwar period. It is not a simplistic criticism, however, as constant references to "light and dark" in this book suggest. Elsewhere, Kumazawa has suggested that Japanese unions pressed for "citizen's rights" (*kokumin no kenri*) and in many cases gained them, but they failed to protect "villager autonomy" (*sonmin no jichi*) or control over the labor process ("Shokuta shakai no sengoshi" ["Postwar History of the Workplace Community"] in *Sengo rodo kumiai undoshi ron* [On the History of the Postwar Labor Union Movement], edited by S. Shimizu [Tokyo: Nihon Hyoronsha, 1982]). The key questions are whether, in the face of intense employer pressure, they had a realistic alternative and whether the majority of workers who acquiesced in the trade-off of autonomy for rights did so willingly and advisedly.

On these questions, the chapter on the transformation of the shop floor in the steel industry in the late 1950s and 1960s offers important evidence, because these changes set precedents that spread throughout Japanese industry. They also laid the groundwork for the subsequent spread of zero defect and quality control circles, which is discussed in another chapter. Although the questions may not ultimately be resolved, these two chapters are at the heart of Kumazawa's work—rightly so—and are highly informative.

For those who have gained "citizenship" and whose interests do not stray far from those of their employers, the outcome may well have been favorable. For those who have not gained "citizenship," options can be limited and unpalatable, as is shown in the penultimate chapter on working women. For regular workers whose interests diverge from those of their employer and who are unwilling to toe the line, there can be intense hardship, as we see in the final chapter: "Twenty Years of a Bank Worker's Life." Here a Marxist banker called Kawabe faces various forms of discrimination for his views and activities and experiences increasing isolation within the bank. But he himself embodies many contradictions, which is what makes him so real and Kumazawa's analysis so interesting. An enemy of capitalism, he works meticulously in capitalism's citadel and is a family man to boot.

This book obviously calls into question naively rosy pictures of Japanese industrial relations. However, it is also a critique of opposing pictures painted with broad brush strokes, which dismiss Japanese labor unions out of hand and assume that a power imbalance gives employers—or bureaucrats—a free hand to do what they will. The former does not recognize the Kawabes of Japan; the latter dismisses the struggles of the Kawabes (or those who ultimately desert his cause) and sees them simply as workaholic bank employees. Kumazawa seeks to bring complexity, contradiction, and subjectivity into the picture.

The book is clearly a critique of those who emphasize harmony and groupism in Japanese culture as well. Indeed, Kumazawa argues that Japanese workers have been very individualistic and wishes they were less so. Like his groupism of the British working class, however, his atomistic individualism is only partly convincing, not because Japanese workers are all groupist, but because those who have deserted Kawabe work hard for a variety of reasons, and not simply to run up the promotion ladder the company has cleverly lowered for them. And there are other types of individualism in Japan, found for instance among small firm workers and owners, which unfortunately are not explored in this book.

Such weaknesses acknowledged, this book deserves to be read widely because it focuses on vital developments in Japan's industrial relations and fearlessly explores the complexities, contradictions, and meanings of these developments. Atomistic individuals perhaps, the workers in this book are human beings rather than robots, happy or exploited. For this reason alone, the book deserves the attention of scholars of Japan. *Portraits of the Japanese Workplace* is highly recommended for all those who are not satisfied with stereotypes of Japan and Japanese industrial relations and employment.

Work and Pay in the United States and Japan. By Clair Brown, Yoshifumi Nakata, Michael Reich, and Lloyd Ulman. New York: Oxford University Press, 1997. Pp. xi+234.

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While it is common to observe that compared to American companies, Japanese companies have more employee training, job security, employee involvement, and seniority-based pay schemes, explorations of the complex relationships between these practices and the ways in which they influence national economic performance are rare. This is the task Clair Brown, Yoshifumi Nakata, Michael Reich, and Lloyd Ulman set out to accomplish in *Work and Pay in the United States and Japan*. Drawing on data from large American and Japanese companies and on national economic data, they set company practices in the relevant institutional contexts and do an ambitious cross-national comparison.

This is an empirically rich demonstration of the synergy between employment practices, job security, pay structure, and labor-management relations. The authors define two contrasting employment systems, SET (security, employee involvement, and training) and JAM (job classification, adversarial relations, and minimal training systems). SET systems are found in large Japanese firms and in U.S. companies that have "learned from the Japanese" in recent decades. JAM systems are found in more traditional U.S. firms, although in the United States professional and managerial workers may be incorporated into SET human resource systems even when production workers in the same firm are not. Doing a nuanced comparison, they ask how elements of these systems work in various employment environments in each country. Increased seniority in a SET employment system increases voice in the United States but not in Japan. The usual American distinctions between production and managerial/professional workers are not found in Japan, but the SET systems in Japan affect only the "core workers" in large firms, somewhere between 17% and 50% of employed males by their estimates (p. 38).

Security of employment, training programs, and employee participation are all parts of an integrated human resource system wherein workers are more motivated to take extensive training and managers to offer it because both are assured of a long-term relationship. This clearly differentiates Japanese and American employment practices because of shorter average tenure in the United States. While these observations are not new, the authors present extensive empirical detail to ground them in recent corporate and national practice. They discover, for instance, that in spite of the more extensive use of training in Japan, the Japanese spend less on employee training than do U.S. companies (p. 80) and that demonstrating a willingness to be trained aids Japanese workers in achieving promotions and wage increases (p. 91).

The book's strongest contribution is its sober look at specific aspects

of human resource systems in the United States and Japan over the last two decades of domestic and international economic challenges. Avoiding the discourse of collapse and downfall, the authors analyze both efforts to change—most Japanese companies have been trying to increase merit-based components of pay since the early eighties—and outcomes—very few Japanese companies have succeeded in making significant changes (p. 105). They point out ways in which U.S. companies have been influenced by Japan but retain, for example, distinctly more flexible labor markets.

The effort to explain why, in spite of recent economic problems, Japanese productivity has continued to grow and unemployment has remained relatively low is an ambitious one, and it is here that the book falters. Brown et al. conclude that SET systems are partially responsible for some national comparative advantages and highlight practices like the use of overtime for “core” employees as a substitute for new hiring and “just-in-time” learning as a way to target on-the-job training for immediate firm needs. They would, however, be on firmer ground if their arguments incorporated larger institutional and political processes. Their chapter on *Shunto* is a case in point. They offer a fine discussion of how this national wage negotiation institution has been used from the late 1980s to suppress wages rather than equalize and raise them as it had been doing since the 1950s when it was founded (p. 185), but they omit the national political process that resulted in the merger of the national federations *Domei* and *Sohyo* into the new federation *Rengo* in the mid-1980s. The birth of *Rengo* was lamented by labor movement activists as the death of progressive labor politics at the national level. The link between union-sponsored wage negotiation and national economic trends is easier to understand when the virtual elimination of the radical *Sohyo* federation, which founded and set the agenda for *Shunto* in the 1950s and dominated it through the 1960s and 1970s, is linked to the more recent emergence of conservative and conciliatory union bargaining. While many of the elements linking firm employment systems and national economic performance are present in the book, the authors neglect the political processes that link labor activism, corporate strategies, and national and international economic policy and are unable to offer a sharp explanation for national performance. Instead, they conclude that “overall economic performance is not determined by either firm employment systems or national economic institutions” (p. 191) but that there is an important interaction between these factors, a measured and appropriate if somewhat anticlimactic conclusion.

World of Possibilities: Flexibility and Mass Production in Western Industrialization. By Charles F. Sabel and Jonathan Zeitlin. New York: Cambridge University Press. Pp. x+510. \$79.95.

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IRESCO

This book brings together a series of historical essays on varied economic worlds (regions, geographically concentrated industries, and firms). Their products are diverse, as are the periods under study (long or short, shifting from the 18th century to the present) and the disciplines to which their authors belong (though economic history and sociology predominate). We find, for example, the silk industries of Lyon and London in the 19th century, the cutlery industry of Solingen, Germany, since the 19th century, the British engineering industry (1840–1914) or the Italian metalworking industry (1900–1920), the plastics industry in Oyonnax, France, after World War II, and so on. What unites these economic worlds is the fact that the actors, firms, and institutions tried to respond to the changing difficulties and uncertainties of the market by constantly renewing and expanding their product lines and introducing new technologies. These essays are remarkable in themselves for their precision, instructiveness, and innovative methods (such as exploring new sources). According to Charles Sabel and Jonathan Zeitlin (who initiated the international seminar sponsored by the Maison des Sciences de l'Homme in Paris, where this research was elaborated and discussed), each of these worlds offers a ground for thinking and trying out new ways of doing economic history. In so doing, they have largely renewed the approach introduced in their seminal article "Historical Alternatives to Mass Production. Politics, Markets and Technology in Nineteenth-Century Industrialization," published in *Past and Present* in 1985:

Economic history has reached a turning point, if it is to continue providing lessons for contemporary thought. Indeed, today's economies are marked by three factors: the fragility of institutions compelled to adapt to ever-changing contexts, the recombability and interpenetration of a plurality of organizational forms, and the awareness of the actors' role in decision making, innovating, and creating institutions. The traditional methodology of economic history is at fault, as it emphasizes the long-term, historical periodization into major phases of crisis or growth and a structuralist conception of social dynamics. Sabel and Zeitlin intend to renew this methodology by insisting on the relationship between rational economic actors and institutions. Their goal, as I see it, is to place economic history within the framework of an institutionalism revisited by the methodology used in history. In so doing, they have joined, albeit in an original way, similar efforts underway in Europe, especially in France (see Salais and Storper, *Worlds of Production* [Harvard University Press, 1997]), in economic history and sociology. This book is thus a highly im-

portant contribution to the debate on action, coordination, and institutions within social science.

The challengers in this area are, roughly speaking, neo-institutionalism (Coase, Williamson, and so on) on the one hand and evolutionism (Gould, and so on) on the other. Sabel and Zeitlin retain the first group's conception of the rational actor, but they differentiate themselves by making him a *strategizer* rather than a *maximizer*. The research in the book reveals actors "as much concerned with determining, in all senses, the context they are in as they are in pursuing what they take to be their advantage within any context" (p. 5). Action is embedded in strategies, which are possible narratives linking the past, present, and future of the action that are intelligible to the actors and provide a framework for their action. The actors acquire an identity that structures them and makes them aware of their place in a common destiny. Thus, they are capable of designing institutions likely to generate the trust required for collective action within uncertainty. The book offers several convincing illustrations: for example, joint boards of arbitration or conciliation in the case of labor disputes and systems of collective tutelage to maintain the flow of trade in the market, despite the impossibility of drawing up complete contracts. Clearly, we are far from evolutionism. The movement of history is not a selection of able individuals but an adaptation process in which human initiative and reflection are paramount, involving recombinations, re-elaborations, and compromises between various forms of organization.

All this sets out a path for fruitful research. The book is a fine way of opening a discussion within the history of industrialization that will surely flourish. Joining in at the outset, we might ask whether Sabel and Zeitlin have not erred on the side of Schumpeterian optimism. Can we generalize on the basis of the essay results? Should collective failures not be studied more closely? Is history, and more broadly human life, only intelligible as narration? Is rational reflexive action not forced to compromise with material objects and products and with a temporal trajectory inscribed in institutions? Should the concept of possibility not be examined in greater depth, for, while it appears in the title, it remains at the horizon of the book?

Between Equalization and Marginalization: Women Working Part-Time in Europe and the United States of America. Edited by Hans-Peter Blossfeld and Catherine Hakim. New York: Oxford University Press, 1997. Pp. xx+333. \$85.00.

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Part-time work is a cross-national trend of growing significance, yet patterns of part-time work differ across nations and over time. This book, edited by Hans-Peter Blossfeld (University of Bremen) and Catherine Hakim (London School of Economics), is an impressive cross-national study of the long-term development of part-time work in Europe and the United States. Focusing primarily on women's part-time work, the various chapters comprise thorough empirical case studies of country profiles of part-time work conducted by national researchers, using a wide range of cross-sectional and longitudinal data. The book includes studies of part-time work in the following countries: Central and Eastern European countries, written by Sonja Drobnic; Greece by Haris Symeonidou; Italy by Tindara Addabbo; France by Laurence Coutrot, Irène Fournier, Annick Kieffer, and Eva Lelièvre; West Germany by Hans-Peter Blossfeld and Götz Rohwer; the Netherlands by Paul De Graaf and Hedwig Vermeulen; Britain by Brendan J. Burchell, Angela Dale, and Heather Joshi; Denmark by Søren Lenth-Sørensen and Götz Rohwer; Sweden by Marianne Sundström; and the United States by Sonja Drobnic and Immo Wittig.

The country-specific studies document a postwar growth in female labor force participation and the development of full-time and part-time work. Several authors also include statistics on men's part-time work, which in many countries is nonnegligible and increasing. The distinction between part-time work, flexible work, and reduced work hours is also discussed. The impact of education and family-stage for married women's labor supply is emphasized. Several authors also discuss the impact of political and institutional frameworks, such as the availability of child care arrangements and rules of taxation. Since most studies rely on official statistics and individual-level surveys, such as the Labour Force Surveys, changes in the employers' behavior cannot be studied directly. Indirectly, however, some aspects of demand-side mechanisms are addressed, such as the thesis of the "reserve army" and various theses pertaining to the restructuring of the labor market in terms of postindustrial development, sexual segregation, and public sector growth.

The research design of the book is a challenging one. The aim is to investigate the rise in women's part-time employment in modern societies in a cross-national perspective, combined with an awareness of time-related variations in industrial structure and economic development. There is always a danger that a project like this results in a number of very interesting and well-performed national studies (as this book also does),

without the synthesis of results that a comparative perspective requires. In the last chapter, however, Blossfeld integrates the common features as well as differences between the countries studied by returning to the six clusters of societies (or welfare state regimes) that was advocated in the first chapter of the book (this typology is partly following Gøsta Esping-Andersen, *The Three Worlds of Welfare Capitalism* [Polity Press, 1990]): The Scandinavian welfare states; the liberal welfare states, such as Britain and the USA; the conservative welfare states, such as West Germany and the Netherlands; France (with the most sophisticated provisions of child care within Europe); the South European countries; and finally the former socialist countries of Central and Eastern Europe. These clusters of countries have different "packages of family, employment and welfare policies" (p. 12) that are expected to influence women's employment in the postwar period. The results show that the integration of married women into the labor market has occurred between the mid-1950s and late-1970s in Northern Europe and the United States. Married women's part-time work is a typically Northern European phenomenon. Within the former socialist countries, part-time work hardly existed, and presently, part-time work is not specifically related to women. The countries in Southern Europe hardly supply part-time jobs, and the editors argue that this can—at least partly—be explained by the later economic development of these countries.

The title of the book reflects two perspectives on women's labor market participation. These perspectives are briefly elaborated by the editors in the introduction of the book. The first perspective argues that women's labor force participation increases their independence and may be a vehicle for greater equality between men and women; whereas, the other perspective argue that most women—in particular married women—work part-time, and part-time work is "secondary" or marginalized work. The editors of this book argue that the first perspective is too optimistic, since it overstates the "liberating" effects of women's employment; whereas the second is too pessimistic, since it exaggerates the negative aspects of part-time work. Blossfeld and Hakim, therefore, are in favor of a third perspective, which emphasizes that married women's work must be understood within the context of their families and the sexual division of labor in the family. Part-time workers are usually secondary earners within their families, thus the high level of job satisfaction that often is documented among part-time workers may not be a paradox after all: Low-paid and noncareer jobs "can not only be tolerated but even enthusiastically appreciated by dependent wives and other secondary earners," (preface) I would want to add that these women would also—given the choice—most certainly prefer part-time work with better terms.

In order to avoid the "time bind" of present-day societies (see, Arlie R. Hochschild, *Time Bind* [Metropolitan Books, 1997]), part-time work may be seen as an indicator of a new and alternative work orientation, since it allows women to structure their time schedules differently. Yet the "male

model" of full-time continuous employment is not challenged, since part-time workers are dependent on a "main" breadwinner.

Despite the national differences in the overall level of women's part-time work, these findings also suggest that there are cross-national similarities and continuities over time in the structuring of gender relations; married women still carry out the major part of child care and domestic work, yet their labor market profiles differ greatly.

A comparative design can illuminate the importance of political and ideological country-specific contexts, and the longitudinal perspective reveals different country-specific trajectories. This book is an important contribution to our understanding of part-time work and women's employment across countries and over time. The book contains useful statistics and analyses of part-time work in European countries and the United States, and I can recommend it to anyone interested in labor markets, family research, and comparative welfare state research.

Essential Outsiders: Chinese and Jews in the Modern Transformation of Southeast Asia and Central Europe. Edited by Daniel Chirof and Anthony Reid. Seattle: University of Washington Press, 1998. Pp. vii+335. \$25.00 (paper).

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The economic success and political vulnerability of the overseas Chinese have frequently been compared to that of Jews in Europe and elsewhere. Both groups have been subsumed under rubrics such as "middleman minorities" or "ethnic entrepreneurs." There have been articles that compare a specific Jewish community in one country (Poland or Romania) with a Chinese community in another land (the Philippines or Indonesia), as well as various works that have compared these groups among others. This is the first volume specifically devoted to a comparison of Chinese in one region and Jews in another area. The phrase, "essential outsiders," speaks to the central issue of how the Chinese in Southeast Asia and the Jews in Europe played central roles in the lives of these lands, while remaining "strangers." They were "strangers" in Georg Simmel's sense of being inside the society but not of it.

The volume as a whole conveys a particular viewpoint regarding both groups as well as issues in the sociology of ethnic relations. The two introductory essays by the editors, Daniel Chirof, a Europeanist, and Anthony Reid, an Asia specialist, are the only ones that directly compare the two groups. In parts two and three, there are essays on both Chinese and Jews, while part four is devoted exclusively to essays on Chinese business in contemporary Southeast Asia. Although one essay, Victor Karady's article on Hungarian Jewry, brings the story of one Jewish community

into the 1990s, the other articles on Jews, by Hillel Kieval and Steven Beller, deal with the late 19th and early 20th centuries. For the editors, the issues relating to anti-Semitism and European Jewry are passé. The dilemmas relating to people who are identified as Chinese are very much part of the present scene in Southeast Asia, having prospered during the recent boom. The articles by Kasian Tejapira, Gary Hamilton, and Tony Waters; Linda Y. C. Lim and L. A. Peter Gosling; and Edgar Wickberg portray communities that have become more, rather than less, assertive about their Chinese identity, unlike Chinese in the region in the 1960s. The assertiveness of the Chinese may have some relevance for the United States, considering the role attributed to Chinese immigrants to the United States as well as wealthy businessmen of Chinese ancestry, such as the Riadys in Southeast Asia in the campaign fund-raising scandal during 1996.

The essays by Chirot and Reid, as well as the other contributors, show familiarity with what has been called "entrepreneurial" or "middleman minority" theories. Yet the editors, in particular, dismiss these theories. Reid writes that the nomenclature used by the North American "middleman minority" theorists is too broad, and their lumping together of "status-gap minorities" in developing nations with immigrant small businesses in the United States is not useful (p. 36). Some of the authors do address these theories head on. Kieval, for instance, uses his essay to refute the economic explanations implicit in most middleman minority theories in accounting for anti-Semitism. His analysis of propaganda used in 19th-century central Europe to support accusations that Jews murdered Christian children for ritual purposes was not grounded in either an economic or a medieval theological discourse but in a modern criminological language. Takashi Shiraishi also places the rise of anti-Sinicism in Indonesia into a larger context. He shows that the Chinese role in the Indonesian colonial economy was not static and that Dutch colonial officials, as well as Indonesian Muslim activists, viewed it differently in various periods. The stress on historical change is a major theme in this book. These two articles go along with the editors' emphasis on a nationalistic framework that excludes Jews and Chinese from the national community as an explanation for the status of these groups. They claim that the "blood" and "civic" varieties of nationalism explain the different fates of Jews and Chinese during various periods and in different countries.

Whether one comes to this subject matter from the field of "middleman minorities" or from comparative nationalism and ethnic relations, the essays here do provide one with useful comparisons, contrasts, and generalizations. For instance, Steven Beller, on the Jews of Vienna, contrasts "anti-Semitic" Vienna with other Hapsburg cities and populations, such as the Germans of Prague and Hungarian Budapest, where non-Jews were friendlier to Jews than in Vienna. Takashi Shiraishi shows similar changes over time in relations between Chinese and Muslims in Java.

The present climate in Southeast Asia, in which anti-Jewish sentiment is promulgated by Muslims makes Chinese in the area loathe to identify

themselves with Jews, as the editors point out. Yet, not only have the Chinese been called "the Jews of Southeast Asia" by outsiders, they have also at times seen themselves in this way (W.P. Zenner, "'We are the Jews of . . .': The Symbolic Encounter of Diaspora Chinese and Jews," in *Points East*, vol 8:3:1 pp. 3-4, 16-18).

There are also some roads not taken in this book. The parallels between the wealthy Chinese in Indonesia and Thailand, who have been clients of powerful military elites, and the court Jews of 18th-century central Europe are close, despite the preindustrial nature of the latter economies. This would be a fruitful field for future research; yet there are important differences. The global economy plays a much more important role than the emerging "world-system" of the early modern period. In addition, the connections between China, Taiwan, Singapore, and the minority Chinese are many stranded. In fact, the opening of mainland China to global capitalism has had a major impact on the Chinese in Southeast Asia. Many of the Chinese in this region have now been able to demonstrate their roles as brokers between China and their present homelands, a relationship far different from what it was in the past.

While the authors of the articles in this book write during the boom, several suggest that in the event of a sharp economic downturn, relations between the Chinese and their neighbors may turn in a hostile direction. This has not occurred in Thailand, but Indonesia in early 1998 has seen a number of anti-Chinese riots. As the title of this volume makes clear, they remain "outsiders." In general, I would recommend this volume highly to both those interested in Southeast Asia and Europe and those who wish to learn more about the interaction of economics and nationalism.

Opposite Poles: Immigrants and Ethnics in Polish Chicago, 1976-1990. By Mary Patrice Erdmans. University Park: Pennsylvania State University Press, 1998. Pp. xi+267. \$50.00 (cloth); \$19.95 (paper).

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Opposite Poles by Mary Erdmans is an engaging account of the struggles of three distinct groups in Chicago's Polonia to work together to ensure an independent, democratic Poland. While Erdmans focuses on new immigrants (Polish immigrants since the 1960s) and ethnics (descendants of earlier immigrants), she also analyzes the role of World War II émigrés who acted as mediators between the two groups. Like the new immigrants, the émigrés had been born in Poland, but they had lived in the United States for decades and thus also shared many characteristics with ethnics. Although much of the current literature on ethnics and immigrant groups assumes uniformity among people of the same nationality, Erdmans discovers that there are extensive differences among groups. In

short, identity and solidarity are influenced by borders created by migrational and generational differences. She situates her study in the pivotal years of 1976–90 when Poland was making headlines around the world as Poles tried to free themselves from the grip of Soviet rule. Data was collected through participant observation, interviews, archival resources, and surveys.

Erdmans suggests that in some circumstances the various groups were able to work together toward their common goals. Different resources gave them something to exchange, while having distinct networks minimized competition. For example, new immigrants had ties to the opposition in Poland whereas Polish Americans had connections to American institutions and officials. They also supplied legitimacy to each other; Polish-American organizations gave legitimacy to the newer ones because of their established history, while political refugees lent legitimacy to Polonia. New immigrants were able to channel aid to local groups and individuals in the Polish underground through their contacts, while Polish Americans were limited to giving assistance to legitimate institutions in Poland.

The groups experienced conflict, however, when their strategies and goals diverged. Although all the groups traced their ancestry to Poland, their political strategies and ethnic identities are affected by their political identities—Polish or American. For this reason, new immigrants, with strong ties to Poland, advocated more radical measures for helping solidarity activists than did Polish-American ethnics. Frustration also arose over who the legitimate leaders of Polonia should be. New immigrants believed that their knowledge about the current situation made them better leaders, while the established Polish Americans believed that they were the superior leaders due to their American connections. The differences became glaringly apparent during the Polish partially free elections of 1989. In this instance, national loyalty took precedence over ethnic identity. It did not matter if one felt culturally attached to Poland, because it was a political decision whether or not to vote.

One of the most fascinating accounts in her book is the policing of ethnic boundaries within Polonia. The ethnics and immigrants both believed that they were the “authentic” Poles in the United States. The new immigrants focused their identity around current events while Polish Americans had a historical orientation. As a result, new immigrants accused the Polish Americans of being more interested in doing the polka than in the struggles of the solidarity workers in Poland. Polish Americans, however, thought the new immigrants should be concerned also about U.S. issues, such as defamation and cultural maintenance. They simply had different understandings of what it was to be Polish and centered their identities around disparate cultural symbols. The question raised was, who are the “true” Poles? Those who had lived under Communist rule and thus were tainted by it or those who had fled Poland before Soviet occupation?

It would be interesting to see similar studies done on Polish-American

communities outside of the Chicago area. This study is unique in that there is such a large concentration of members from all three groups in Chicago. Did the same identity and national issues come about in smaller communities? It is likely that in areas where people of Polish ancestry (whether born in the United States or Poland) are small in number and dispersed in the suburbs that there would be a stronger inclination to embrace the new immigrants, despite the differences. In fact, in communities without a significant émigré population, new immigrants might be considered the "real" ethnics, not those who had been in the United States for several generations.

This book should be of great interest to American ethnic and Polish historians as well as to scholars of immigration, ethnicity, and social movements. Erdmans clearly summarizes the historical circumstances in both Poland and the United States, which lead to the cooperation and conflict between the ethnics, émigrés, and immigrants in Polonia. In addition, she situates her discussion in the literature on ethnic identity, assimilation, and pluralism. Finally, she synthesizes the relevant literature on social movements to help explain the different resources and strategies available to the Polonia organizations to mobilize for a democratic Poland.

Competing Visions of Islam in the United States: A Study of Los Angeles. By Kambiz GhaneaBassiri. Westport, Conn.: Greenwood Press, 1997. Pp. xiv+202. \$59.95.

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The study of Islam in the United States is an increasingly viable field of inquiry open to investigation by scholars in every discipline in both the social sciences and the humanities. While this text has critical deficiencies as an "in-depth sociological analysis," it is of definite value for the author's discussion of important, hidden issues in Muslim communities. It is the analysis of these issues—such as gender, ethnocentricity, new interpretations of scripture—that make this text important. The central argument of this text is that in the absence of a central Islamic authority (i.e., no government or religious oversight) in the United States Muslims have become ethnic enclaves with freely competing interpretations of Islam and its practice.

GhaneaBassiri, an American Muslim of Iranian descent, situates his study as a survey from a 13-page questionnaire given to a variety of Muslims living in Los Angeles and its suburbs. Unfortunately, the survey instrument is far too ambitious for the population, is not reproduced in the text, and was not analyzed. He uses 83 multiple choice and 14 short answer questions for a respondent population that is significantly under 18 years old (36%). Because of the preponderant age of the respondents one

questions the relevancy of the questions to them as well as their ability to answer them. Even though some of the questions are used (approximately 45) throughout the text to enhance discussion, we are unable to surmise who answered the questions or what values have been assigned to the responses used. I surmise that what we actually can benefit from are GhaneaBassiri's discussion, based upon his extensive experiences of the issues, concerns, and problems in a diverse Muslim community.

There are several, at least seven, studies of Muslims living in Los Angeles that attempt to describe the population. This text does not add to that general information but does for example, take readers inside some Iranian organizational meetings. From this we are able to take a rare look at Shi'is operating in social groupings rather than individually as is typically the case in most studies. Here a good discussion of Shi'ism would have been helpful in giving readers background for understanding issues around the Iranian Revolution, an event that is the major dividing line in the community.

What emerges as significant for research on Islam in America is how "Muslims understand Islam in the United States" (p. 11). Here GhaneaBassiri explains the dimensions of Muslim outreach to each other and to non-Muslims (*da'wah*) insightfully. He explores in some detail the changes in Islamic understandings that are occurring in the United States, such as changes in the role of the imam and the diversity of thought in the absence of authority. Examination of gender issues leads to discussions of women leading prayers in the mosque and divergence of concerns about women's dress, dating, and even diet.

GhaneaBassiri asserts that in the United States Islamic laws have been deemphasized as American Muslims are quite pleased with the absence of authority that dictates everything in the Muslim world. He found that Muslims have the same problems as the majority society when it comes to raising children. The various freedoms in this society and the violence cause problems across the board. It is these freedoms, however, that permit women to assume more meaningful roles Islamically, such as their attendance and participation in mosque activities. This does not mean that the longstanding Muslim assertion of equality between men and women is being actualized. Even in America "women almost never share in the official leadership of mosques." The American Muslim community like its world counterpart has not provided an atmosphere where there is an equality of responsibility. Immigrant women, though they have the freedom to go, still do not attend the mosque. Women who do go find the separation or rather the type of separation between men and women for the congregational prayer, "negative" or "isolating."

Even though Muslims of differing ethnicities and ideologies interact more in the United States than anywhere else, there is only tolerance, not cooperation, not a real brotherhood. No leader has emerged to lead the whole community. GhaneaBassiri expresses the sentiments of many Muslims when he points to "Muslim leaders" who do not actually have followers. He asserts that it is the lack of leadership that prevents Muslims

from exerting any political or measurable social influence. Almost every aspect of living Islam has undergone some change. Mosques are built and maintained by individuals in the United States rather than the government as in the Muslim world. While this can be good it definitely changes the understanding of place of prostration, since individual owners can then decide who can or who cannot come in.

This text could have given more information on the interactions of Muslims in the general Los Angeles community but, as previously stated, finds its forte in discussions of concerns and issues. GhaneaBassiri does a good job of integrating other research on issues into his narrative thereby adding to its credibility. This is a welcome addition to texts on Islam in America.

From Black to Biracial: Transforming Racial Identity among Americans.
By Kathleen Odell Korgen. Westport, Conn.: Praeger, 1998. Pp. ix+143.
\$55.00.

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In *From Black to Biracial*, Kathleen Odell Korgen proposes that there has been a transformation in the racial order of the United States. She argues that the civil rights era changed societal recognition of and individual identification among those persons who have an African-American and a white parent. Although historically the "one-drop rule" assigned all persons with known African heritage to black racial identity, it is no longer strictly applied. Today these individuals can identify and are socially recognized as biracial.

Korgen's analysis is based on 64 interviews with persons who have a black and a white parent: eight primary interviews and 22 secondary interviews (from Lise Funderburg's *Black, White, Other* [William Morrow, 1994]) with persons who were born before 1965 and 32 primary interviews with Boston area college students who were born after 1965. (The methodology is outlined in an appendix.) The first chapter gives a well-written and informative history of miscegenation and black/white racial identity in the United States from 1619 to the present. The next chapter uses three case studies of biracial adults, born before, during, and after the civil rights movement, to examine how social context influences racial self-identification. Here Korgen establishes the foundation for her proposition that those born before the civil rights movement adhere to the one-drop rule and identify racially as black, while those born during and after the movement acknowledge or adopt biracial identity. Chapter 3 looks at the influence of appearance, the civil rights era, culture, and neighborhood and family on black versus biracial self-identification. The evidence here points to the importance of social class and neighborhood composition for racial self-identification. Korgen argues that racial identi-

fication is problematic for those who have a mixed racial heritage. However, the concept of race is shifting, although the boundary between black and white still exists (p. 55). Even though the younger cohort identifies as biracial, none of the respondents identify as white. In chapter 4, Korgen argues that in college the younger group of biracial Americans face a tremendous amount of social pressure, from both blacks and whites, to racially identify as black. Dating provides the litmus test for which racial group a biracial student has chosen. Failing the racial litmus test can have serious consequences: "Some are literally driven from the communities in which they find themselves tested" (p. 66). In the following chapter, Korgen applies the concept of marginality to the experiences of her respondents. Even though the younger cohort is willing to label themselves biracial, they, like their older counterparts, express some difficulty around being able to "fit in." On the other hand, many of the respondents perceive themselves as having a more objective view of race than those who are not biracial. Chapter 6 examines symbolic interactionist and postmodernist theories of identity formation. Korgen argues that these theories provide useful insights into racial identity transformation, however, they should be expanded to include "chosen" identities (p. 95). She says globalization and economic shifts require that people have more fluid identities. "As identities shift, so do persons' demands on society. This in turn prompts a further alteration in social structure" (p. 95). In the final chapter, Korgen addresses the policy implications of her findings. She notes that biracial people face the same discrimination and racially based injustices that African Americans face. Additionally, like other Americans, her respondents have varying opinions about affirmative action, but they are overwhelmingly supportive of biracial Americans benefiting from it. Also, this chapter examines adding a multiracial category to the United States census in 2000. Here the concerns are with whether biracial/multiracial people will succeed in having their interest in this recognized and what form the question would take (i.e., allowing people to check as many boxes as apply or a single box labeled multiracial). According to Korgen, the multiracial future of the United States "is at hand" (p. 118).

For those who are interested in the racial categories that will appear on the census in 2000, Korgen's research is very useful. She clearly demonstrates that her younger respondents see themselves as having more than one racial identity, even though they are very aware that American society continues to apply the one-drop rule in identifying them racially (see, e.g., chap. 4 and p. 114). Thus, Korgen's analysis is not fully convincing in terms of her argument that the racial identity of biracial Americans is no longer socially constructed as black. She never provides an analysis of a transformation in the racial structure of American society to support her proposition about societal change. The only applicable data she presents are from a survey of 204 New England area college students, where 74% of them agreed that people with an African-American and a white parent should be able to identify as biracial (p. 42).

Furthermore, Korgen's analysis is weakened by her focus on identity theories without incorporating race and ethnicity theories. While it is reasonable to propose that people, to some degree, can choose their identities; within the subfield of race and ethnic relations, one factor that distinguishes race from ethnicity in the United States is the optional (or choice) character of ethnicity and the identifiability that is associated with racial labeling. Additionally, it is the ability to identify those who are black or nonwhite that makes them easy targets for discrimination. Although Korgen finally acknowledges that biracial persons, and, what is more important, her respondents experience the same discrimination as African Americans, she never analyzes if these experiences influence whether her respondents identify as black or biracial.

While I agree with Korgen that today there is more willingness to acknowledge the conflicts around black/white racial identity, biracialism does not resolve the racial dilemmas in American society. If as Korgen suggests, "Our multiracial future is at hand" (p. 118), maybe it should begin by acknowledging the multiracial identity of African Americans that she outlines in her first chapter. Wider awareness of this history may have the potential to close the racial divide and transform the racial structure of the United States.

After Pomp and Circumstance: High School Reunion as an Autobiographical Occasion. By Vered Vinitzky-Seroussi. Chicago: University of Chicago Press, 1998. Pp. x+203. \$39.00 (cloth); \$14.00 (paper).

Shaunna L. Scott
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Perhaps it was fortuitous for Vinitzky-Seroussi that her analysis of high school reunions arrived in my mailbox near the 20th anniversary of my own high school commencement. My general enthusiasm for this topic and my favorable assessment of the author's description and analysis may result from my reunion "state of mind." The time has come for me to reminisce and account for myself, after all. So, what could be more enjoyable and enlightening than reading about others in a similar situation? However, the fact that this book offers an accessible and competent analysis of an underresearched social phenomena should make it appealing to most sociological reading audiences, regardless of when they graduated high school or what their experiences of high school and high school reunions may have been.

Vinitzky-Seroussi examines high school reunions as "autobiographical occasions" (p. 3), a term coined by Robert Zussman (*Contemporary Sociology* [1996] 25:143) to refer to social settings in which actors share their life stories. This author takes seriously Zussman's complaint that autobiographical scholarship concentrates too much on the narrative while paying scant attention to the social context in which the narrative un-

folds. She sets about the task of providing an explicitly *sociological* account of identity construction and life story narration that occurs at high school reunions. For the most part, the book succeeds in this task quite well. In fact, the primary strength and appeal of the book is its absolute insistence upon the social nature and embeddedness of human beings, even in their most "personal" and "individual" endeavors of self-scrutiny, reflection, reminiscence, and interaction at the microlevel.

This analysis is based upon participant observation at five class reunions held at three suburban East Coast high schools (one from a middle-class neighborhood and two from lower-middle-class areas) as well as interviews with 94 reunion participants, 200 questionnaires from reunion attendees who were not interviewed, and questionnaires from 115 individuals who declined to attend their high school reunions. It covers such topics as the construction of personal and situated identities, the creation of communities and collectives, how pasts and presents can be connected and evaluated, and how collective and personal memories are forged. The author takes a symbolic interactionist approach to these subjects, with particular reliance on Erving Goffman's dramaturgical metaphor and Charles Cooley's concept of the "looking glass self." In general, the book is well researched; it makes a clear argument and employs appropriate methodological tools and theoretical perspectives in accomplishing its intellectual goals. Readers on such general sociological topics as collective memory, community, social control, identity, autobiographical occasions, and the study of life story and narrative should find this book worthy of their attention.

This work is also a prime candidate for use in undergraduate courses where it is important to offer a "good read" on a "sexy" topic that is, above all, clearly focused upon the *social* dimension of human life and thought. Readers of *AJS* who are considering this as an undergraduate text should be forewarned, however, that the author occasionally employs concepts without adequately defining them—alienation, for example (p. 34). Similarly, in a discussion of spouse's reunion experiences, she writes, "spouses bracket the event" (p. 64). Without adequate explication, undergraduates could be left confused by this statement. In the same vein, the author cites Howard Chudacoff (*How Old Are You?* [Princeton University Press, 1989]) in order to make the claim that age-based reference groups have assumed great importance in contemporary American society. Yet, she never adequately describes the empirical basis upon which she and Chudacoff have based this conclusion. Finally, chapter 3, which makes an excellent argument concerning the subtle processes of social control operating in even the most benign and voluntary social settings, could use some editing with attention to organizational concerns.

More troubling, however, is the appearance that the author may be overgeneralizing from this study of (predominantly) white, East Coast adults to "American culture," more generally. In an attempt to counter exaggerated claims concerning the death of the subject, on one hand, as

well as criticisms of American culture as individualist, shallow and overly concerned with appearances, on the other, Vinitzky-Seroussi occasionally oversteps the empirical limits of her study. Statements, such as "Americans are loath to confront tension between outer appearance and inner beliefs" (p. 15) and "contemporary Americans find it difficult to completely separate them [situated and personal identity] and have trouble living with the tension between what is publicly held and what is internally felt" (p. 162), push the boundaries of what can be reasonably concluded on the basis of this data. Though more limited in scope than its author sometimes admits, this book is nevertheless interesting and significant.

Procreative Man. By William Marsiglio. New York: New York University Press, 1998. Pp. xi+276. \$55.00 (cloth); \$18.95 (paper).

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Just think how far we have come: Someone can write a book called *Procreative Man*, and that is exactly what it is about—men, fellas, guys, not women. Just think how far we have to go: Someone can write a book called *Procreative Man*, and the text runs to less than 200 pages—there are only 30 pages of references, not all of which are even about men.

I am grateful to William Marsiglio for having done this book: it is a very useful piece of work. The bibliography alone, wonderfully interdisciplinary, including some classics but brought right up to date, makes the book indispensable. Want to know what is known about men and birth control, men and childbirth, men and abortion? This is the place to begin one's research.

The problem is that we are indeed only at a beginning. Look up "cesarean section" in the index, as I did, to see what is known about men's experiences with this surgical procedure that accounts for close to one-fourth of the births in the United States, and you find it mentioned once. Look up lactation, breastfeeding, nursing—not listed. Am I focusing too much on men as other here? So look up impotence: two references, both in passing.

When it comes to reporting the data, the book is—necessarily—filled with phrases like "while no reliable data exists" and "perhaps," "maybe," and "one study suggests." This is not Marsiglio's problem; this is his contribution. He highlights for us that we have not really bothered to ask these questions. We do not know a lot about how fathers whose children are born by cesarean section experience that event, how it does or does not affect their relations with their partners or the children themselves. We do not know much about how men experience living with what the breastfeeding literature likes to call "the nursing couple." And until Via-

gra burst on the scene, we did not even know how many men are troubled by their (relative) impotence, and we still do not know what if anything that has to do with their experience of themselves as fathers.

So while I would like to see a 500-page, densely packed encyclopedia of procreative man, I am grateful for what I have in this book. The best statement of what this book is about, its accomplishments and limitations, is offered by the author in the preface. He assures us that while the title is simple, he does not mean to imply or portray a singular figure, a universal "procreative man." He remains sensitive to the varieties of experience men have. Obviously, "procreation" means different things to men older and younger, married, single and gay, fathers of newborns, grandfathers, nonfathers, and to men of different cultural backgrounds. But where is the data supposed to come from for Marsiglio to draw this out for us? "The paucity of data on some of the topics I cover limits my ability to explore the procreative experiences for different categories of men, many of whom are affected by different types of masculinities. My principle contribution, then, is to develop new ways of thinking about men's diverse procreative experiences and to generate future research avenues" (p. x).

I think he is much better at the latter than the former. I saw dozens of different dissertations rise up before my eyes as I read this book. I did not, however, feel I had come to some new way of thinking or understanding men's diverse procreative experiences.

The first chapter does lay out a theoretical scheme, guided, Marsiglio informs us, by symbolic interactionism, the scripting perspective, and identity theory, each briefly explained. His conceptual model is organized around "two loosely defined social psychological concepts or themes," which he calls "procreative consciousness" and "procreative responsibility" (p. 5). The next chapter provides a brief history from the discovery of biological paternity through the HIV epidemic, all in under 20 pages.

The third chapter, "Gender, Sex and Reproduction," I found most productive of theoretical thought: the answers may not be available, but the questions are very rich indeed. Chapters 4 and 5, on birth control and on abortion, are the most solidly data based. Chapter 6 on "Pathways to Paternity and Social Fatherhood" talks about pregnancy, new reproductive technologies, adoption, and stepfatherhood. I want to know more about how men feel about nurturance, about their relationships with children, and how that does or does not relate to ideas about "paternity" in the bio-legal definitions. Here is where gender politics between the author and this reviewer come to the fore. As an adoptive mother and as a sociologist who focuses on the significance of human relationships, I felt distressed by the conclusions of the section on adoption and stepfathers: "While anecdotal evidence reveals that children are sometimes quite close with adoptive fathers and stepfathers, this does not negate the possibility that children may at times emphasize the presence or absence of a biological connection. Indeed, some adopted children devote a considerable amount of time and energy to locating their birth parents" (p. 143). We

do not need to relegate the significance of family ties in adoption and stepparenting to "anecdotal evidence." We have better than that. And when we do look at the adoption search literature, what is immediately apparent is that the search has been overwhelmingly for birth *mothers* and relatively rarely for genetic fathers. And what does any of that tell us about procreative *man*?

By the last chapter, Marsiglio and I had parted company. "The future of Procreative Man" includes a call for a Pregnancy Resolution/Child Support (PRCS) contract (p. 76), "ideally signed prior to partner's having sex" (p. 175), which would "delineate the negotiated rights and responsibilities of the parties involved" (p. 75). As a man who began this book discussing his own entry into fatherhood with an unplanned pregnancy when he was 18, Marsiglio seems to have wandered rather far from the real world.

In spite of my concerns, I return to where I started: This book is a good indication of how far we have come and how far we have to go in understanding procreative man.

Post-Soviet Women: From the Baltic to Central Asia. Edited by Mary Buckley. New York: Cambridge University Press, 1997. Pp. xvii+316. \$59.95 (cloth); \$22.95 (paper).

Bolshevik Women. By Barbara Evans Clements. New York: Cambridge University Press, 1997. Pp. xiv+338. \$64.95 (cloth); \$24.95 (paper).

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The plight of the majority of the population in the former Soviet Union has received little attention. Women, the focus of so much Soviet propaganda, live mostly on the margins of the "new" society rising from the ashes of the Soviet Union. Mary Buckley is prominent among a group of British scholars who have consistently produced significant studies of Soviet and post-Soviet women. The collection of essays in *Post-Soviet Women* is Buckley's latest contribution.

The author provides a useful introduction, framing the discussion in terms of continuity and change, diversity and distance from current western feminist debates. Her book is divided into two parts: part 1 focuses on Russia, its economy, society, and polity; part 2 discusses women in the newly independent states formed from the former Soviet republics. A number of the essays challenge common assumptions; others reinforce them, but the overall tone is gloomy. Sarah Ashwin and Elaine Bowers argue that women's unemployment has been overstated, that women remain the backbone of many industries, and are more reluctant than men to leave their traditional work for the private sector. In contrast to western press accounts, for most Russian women, the allure of housewifery

is not appealing; they praise the work collective and prefer to stay in the paid labor force. But in so doing, they face increasing discrimination and shifting stereotypes, all aimed at keeping women at the bottom of the heap.

Women in the countryside fare no better. They remain a critical part of the rural workforce, especially now that more men are moving into the private sector. As Sue Bridger shows, increased alcohol abuse and malingering by their men make the women even more reliant on subsistence farming and barter to get by.

Rebecca Kay discusses images of the ideal Russian woman, showing the ways in which young women have absorbed western images of "femininity." Lynne Attwood addresses the issue of violence against women and the ways in which women are blamed for this increasing phenomenon. While post-Soviet changes have allowed more open discussion of this issue and have allowed the establishment of women's crisis centers and shelters, the problem persists, and societal attitudes are getting worse. Hilary Pilkington makes visible the plight of Russian women and children forced by rising nationalism to migrate from the newly independent states.

The situation of women in the other areas of the former Soviet Union is the subject of essays that take up about one-third of the book. Nijole White is comparatively positive about the prospects of women in Latvia and Lithuania, citing the greater support for women's organizations and the openness to western models of feminism. Solomea Pavlychko is pessimistic, outlining the problems in Ukraine of blending nationalism and feminism, and citing the "strengthening of discrimination" and women's conservatism. Nora Dudwick is similarly pessimistic about the situation of women in Armenia, and Shirin Akiner outlines the problems of central Asian Women, the "surrogate proletariat" emancipated by the Soviets and now caught in the midst of resurgent Islam and economic uncertainty.

So what is a woman to do? Even when women do attempt political activism, the results are mixed at best. The blending of women's activism, nationalism, and pacifism is described in Tamara Dragadze's account of the Georgian women's peace train seeking and failing to stop the conflict between Georgians and Abkhazians in Abkhazia. Kathryn Pinnick explores the history of the Committee of Soldiers' Mothers, founded in 1989 by mothers who had lost their sons in the Afghan war. Mary Buckley traces the checkered history of the Women of Russia party, the old Soviet Women's Committee transformed into the first all-female group in the Russian parliament, or indeed in any parliament, from its high point in the 1993 elections to its subsequent slide. Only Olga Lipovskaia is positive, recounting the growth in women's activism despite the hostile political and popular climate.

Russia's future may be less in politics than in profit making. Here again, women are marginalized, mostly at the bottom, but adapting to changing conditions. They are invisible in accounts of the new entrepre-

neurs but quite visible as street vendors, selling goods to survive. Despite lack of access to capital, some women have even created successful, generally small-scale, businesses. In a more hopeful vein, Marta Bruno shows how women, outside the sphere of big money, have created a "counterculture of entrepreneurship" based on networking and mutual support and thus are adapting to the vicissitudes of the market economy in their own way.

How is it possible that a "post-Soviet patriarchal renaissance" is taking place in a country that considered itself the cradle of women's emancipation? Barbara Clements analyzes the antecedents of early gender debates and their results in *Bolshevik Women*, her study of the Bolshovichki, the women who joined the Bolshevik party before 1921, and shows how the changes in their careers and lives mirrored the resurgence of patriarchal attitudes and values from the earliest days of the socialist state.

Most studies of women in the Soviet period have connected the reassertion of more traditional values with Stalin's rule. For Clements, the change occurred as early as 1917, when the Bolsheviks seized power. She argues that the Bolshevik Revolution, despite its sweeping enactment of laws liberating women, actually marked revolutionary women's nadir of power, that "women rose highest in the Bolshevik organization when it was at its most egalitarian, that is, before and during 1917" (p. 12).

After the revolution, in Clements's view, the Bolsheviks reverted to "tyrannical political habits deeply embedded in Russian culture" and to the kind of gender discrimination that marked all European political parties. Her argument runs counter to the prevailing scholarship, which links Bolshevik authoritarianism directly to Lenin's philosophy and personality and any pre-Revolutionary "egalitarianism" to Lenin's inability to control the party from afar.

Clements based her study on a database with records of 545 Bolshovichki, 318 pre-1917 members (about 13% of approximately 2,500), and 227 of those who joined during the civil war years (about 7.5% of the total of about 30,000), which she then compared to a database of male Bolsheviks, culled from the massive 28,000 entry *Soviet Data Bank*. She then added information from the biographies of seven leading Bolshevik women, including Inessa Armand and Elena Stasova, both of upper-class origins, and Alexandra Artiukhina and Klavdiia Nikolaeva from the working class. She bolstered her work with 41 tables, six graphs, and 31 illustrations, adding a further dimension to her portraits of the Bolshovichki.

Clements applies current social movement theory to her study, especially stressing notions of collective identity in shaping the views and actions of these women. She makes much of the concept of *tverdest*, or hardness, as an important part of the collective identity of the Bolshovichki. The term has layers of gendered meaning, but for the revolutionaries, it meant someone who was "coldly rational and unsentimental," someone with a tight control on their emotions, in other words, a woman who had mastered the ideal qualities of a man.

The author provides a useful perspective on the comparative progress of female and male Bolsheviks both before and after the Revolution. It should come as no surprise that female Bolsheviks fared less well than their male counterparts, were passed over for prominent positions (Armand), or unceremoniously ousted when they did get them (Stasova). Nevertheless, Clements provides significant details to flesh out the story of Bolshevik sexism. The Bolshevikki were subject to sexual innuendo in the fierce intraparty power struggles before and after the Revolution, and responded by withdrawal, self-exile, or in at least one case, committing suicide. In the early years, troublesome female activists were generally demoted or exiled. Losers in most power struggles, the female activists' gender sometimes aided them in avoiding the worst of the purges. Reversing the traditional patriarchal notion that marriage protected a woman, single women such as Stasova, nicknamed "Comrade Absolute" and Kollontai, were considered least dangerous and generally spared.

Younger activists, without the baggage of life in the underground, found opportunity in a system whose propaganda promoted equality; nowhere else in the world were there so many female professionals, artists, scientists, judges, as in the USSR by 1930. As Stalin gained power, many of the Bolshevikki showed themselves lacking in the requisite amoral toughness, but some survived, and one, Rosaliia Zemliachka, was rewarded for her enthusiastic participation in the purges with appointment to the Council of People's Commissars, becoming the sole woman to break through this Stalinist glass ceiling.

Clements is particularly good when she fleshes out her portraits, in good feminist fashion integrating information about the personal and political lives of her subjects. There is more about the better known Bolshevikki, such as Inessa Armand, Alexandra Kollontai, and Elena Stasova, but there is also information and reinterpretation of the importance of less well-known women, such as Alexandra Artiukhina, a worker and the last head of *Zhenotdel*, usually portrayed as a party hack.

This is the first time that so much information has been gathered in one place about both prominent and rank and file Bolshevik women. Clements is to be commended for her diligent detective work and dedication to her task. In showing how the Bolsheviks co-opted the ideology of women's emancipation for their own ends while preserving basic aspects of patriarchal power, she helps explain the conditions that have led to the dramatic loss of opportunity and economic independence analyzed so vividly in Mary Buckley's collection.

Reconstructing a Women's Prison: The Holloway Redevelopment Project 1968–88. By Paul Rock. Oxford: Clarendon Press, 1996. Pp. xii+360. \$69.95.

Andreas Glaeser
University of Chicago

Narrating the history of the reconstruction of Holloway, England's most notorious women's prison, Paul Rock lays bare the ironies of policy, planning, goodwill, and anxiety in a sophisticated, multilayered analysis of the vicissitudes of an attempted prison reform. Rock begins his fascinating tale in the mid-1960s when Holloway, an imposing Victorian structure built to intimidate, discipline, and guard its inmates, started to be perceived as inhumane because it seemed to constitute an environment hostile to the therapeutic treatment of its inmates. Thus, the replacement of the old structure with a pathbreaking modern design was conceived, literally inverting the old panopticon by substituting a park as meeting ground for the central inspection bridge. Intertwined with the reconstruction was a plan for reform. The centrality of security was supposed to be replaced by an emphasis on treatment by building the new Holloway on the model of a hospital and thus changing the fundamental character of the relationship between personnel and inmates from surveillance to treatment. However, soon after its initiation, the reconstruction project ran into difficulties when unforeseen environmental changes began to challenge some of the basic assumptions underpinning the reconstruction project. With the advent of prominent women terrorists and with the overall number of female prisoners steeply on the rise, the proposition that women were not really criminal in the same way as men had to be reconsidered. Moreover, the first oil-price crisis triggered comprehensive cost-cutting measures putting a definite end to liberal public spending. Both of these changes in the external environment led to significant alterations in the plans for the new building, in staffing levels, and in perceived staff responsibilities. By the mid-1970s, contrary to the reformers' original intentions, security measures were moving to the foreground again.

The result of the thwarted reform was, according to Rock, a vicious cycle of violence between wardens and inmates. Anxieties about an uncontrollable situation provoked prison personnel to insist on locking prisoners up into their cells for longer periods of time. Inmates, in turn, reacted to these restrictions by increasingly violent, seemingly crazy behavior, giving wardens yet more cause to keep them in their cells. Rock's tale culminates in the analysis of a strike staged by the wardens' union in 1988 to enforce even tighter security measures in an already very strictly controlled environment. Instead of caving in to the demands of the union, Holloway's governor chose to fight for more liberal prison regime. Locking out the striking security officers, the governor decided to run Holloway with a minimum of staff, relying on the maximum cooperation of the inmates. Allowed to take greater responsibility for their

own lives, the inmates in fact did cooperate, leading to what Rock describes in vivid colors as the most humane episode in Holloway's pained history. The governor succeeded in the end, but his success came at a price. Alarmed by the anger the strike had caused in the warden's union, the home office relieved the governor of his duties, thus rendering the final success of a more lenient, more humane penal regime at Holloway uncertain. What remains according to Rock is the hope that the strike was a rite of passage permanently transforming the consciousness of all involved.

Rock's well-written book thematizes human agency from two different, equally fascinating angles. First, he explores the structural and cultural conditions under which bureaucrats enjoy the freedom to enact their own agenda. Narrating his story from the point of view of Holloway's governors, while considering a truly impressive variety of influencing factors (ranging from the impact of spatial environments and the rhythm of career trajectories, over personal biographies and networks, to group conflict, local politics, changes in public ideology, and international affairs), Rock is able to draw an admirably nuanced picture of the decision making of the reform project in progress. He also strikes a convincing balance between the agency of individual and institutional actors on the one hand and structural changes on the other by integrating systemic changes both as political constraints and as cultural representations into the life worlds of creative actors. In this sense, Rock's book provides an excellent quasi-ethnographic perspective to the literature on bureaucratic decision making and the state.

Second, Rock looks at the consequences of structural constraints and cultural presuppositions of the prison personnel on the types of action taken by Holloway's inmates. Rock reads the increasing rates of seemingly crazy, violent, and self-destructive behavior of inmates as a direct consequence of curtailing their freedom. As a flipside to this view, he interprets the cooperation of the prison inmates during the strike of 1988 as the humanizing effect of trust and respect granted to them by the emergency staff keeping Holloway operational during the strike. Given the importance of this interpretation for Rock's rhetoric of reform, it is very unfortunate, however, that Rock has very little interview material with inmates to substantiate this part of his analysis. Thus, it remains quite unclear, for example, how inmates interpreted their own actions before, during, and after the strike.

Given the two very different angles on agency developed by Rock, it would have been helpful if he had moved beyond a juxtaposition of his material on inmates and officials to systematic comparison and theoretical reflection. Without theoretical reflection, the tenor of Rock's work remains paradoxical, for he suggests more autonomy for the inmates as effective means of prison reform. A rigorous theoretical exploration of this paradox might have also enabled him to respond more effectively to Foucault's biting observation that the very idea of reform has always been a functional part of the prison system without ever yielding the pro-

claimed result of improving the reintegration of ex-prisoners into mainstream society.

Being There: Culture and Formation in Two Theological Schools. By Jackson W. Carroll, Barbara G. Wheeler, Daniel O. Aleshire, and Penny Long Marler. New York: Oxford University Press, 1997. Pp. xi+299. \$35.00.

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This book is an ethnographic study of two postbaccalaureate institutions that prepare students for various types of ministry in protestant churches and organizations. Mainline Seminary is the pseudonym for a seminary affiliated with a liberal protestant denomination. Evangelical Seminary denotes a multidenominational seminary loosely aligned with conservative, evangelical churches and organizations. The authors' goals are to map the culture of each seminary and to show how this culture affects the formation of students' character, vocation, and resources for answering their call to ministry.

The authors find that each school has a distinct, core normative message that "functions as the pivot of the institution's culture, anchoring the culture and orienting the educational agenda" (p. 205). Evangelical Seminary's cultural anchor is that the world needs to embrace a sober, religious discipline that allows people to follow God's orderly plan for human redemption that is inerrantly inscribed in the Bible. Mainline Seminary's central message is that the world needs to be cleansed of all prejudice and oppression to help usher in God's reign of justice.

Despite the substantive difference in these cultural messages, the authors identify striking formal similarities in how the school cultures are organized and how they shape and are shaped by students. Each culture provides a "tool kit" of strategies for ministry in uncertain times (Ann Swidler, "Culture in Action: Symbols and Strategies" [*American Sociological Review* 51 (1986): 273-86]). Each culture has a normative core and permissible variations that include purer, more radical and watered-down versions of the core message. Both seminaries allow students leeway in contesting the core cultural message and in ultimately negotiating a qualified acceptance of some version of that message. The processes of critique, negotiation, and altered reproduction occur in small student groups and in the classroom. The cultures of both seminaries are thus constructed and changed over time by the interaction of students, faculty, and staff. Each school culture is also affected by the seminary's selective adaptation to and isomorphism with broader organizational fields from which it derives resources and legitimacy. And in contrast to other scholars' accounts of culture wars, these authors find that each school emphasizes the reform of the religious institutions with which they are closely

affiliated rather than castigates those at the opposite end of the ideological spectrum.

By the end of the book, the authors convincingly make the theoretical argument I have just summarized. However, readers may find the road to this argument long and frustrating. Although the authors say they followed Barney Glaser and Anselm Strauss's grounded theory method (*The Discovery of Grounded Theory* [Aldine Publishing, 1967]), the book's organization does not mirror a grounded theory approach of reciprocal attention to data, literature, theory formation, back to data, and so on. Rather, the book first presents nine chapters of pure description before offering any explicit theorizing. The reader must absorb a dizzying array of detail about the two schools and meet a huge cast of characters before any framework is given for the data's relevance or interpretation.

Without context or framework, some of these data are baffling. While my education at a liberal divinity school helped me follow the description of Mainline Seminary, I lacked the cultural capital to understand much of life at Evangelical Seminary. For example, I could only very superficially follow what is presented as a significant argument between students and faculty over process theology (pp. 64–68), and I remain bewildered by the debate between dispensationalism and the mainstream Evangelical Seminary position. The descriptive chapters lack an authorial voice to guide readers through the maze of characters, ideas, and debates. Yet perseverance in reading the first nine chapters is rewarded by the clear, thoughtful interpretation in chapters 10–13. The book ultimately presents a compelling analysis of each school's culture and its relationship to student formation.

This book will interest sociologists of culture, religion, organizations, and education. Several findings are relevant to higher education in general. Most students in both schools are profoundly effected by their interaction with faculty and with other students. But students who do not spend substantial amounts of time at the seminary miss out on this process of vocational formation and seminary culture reproduction and change. One must "be there" temporally and geographically to shape and be shaped by the school culture. The authors caution that many trends in higher education, from part-time and commuter students to video- and internet-based distance learning, will undermine the process of personal and institutional formation and reformation.

Is Science Multicultural? Postcolonialisms, Feminisms, and Epistemologies. By Sandra Harding. Bloomington: Indiana University Press, 1998. Pp. xi+242. \$35.00 (cloth); \$14.95 (paper).

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Sandra Harding is no stranger to challenging the boundaries of the philosophy and social studies of science and knowledge. As a feminist philosopher of science, Harding has made issues of diversity and difference a central aspect of her program of "strong objectivity." This program has always had two basic components. The first is an analysis of modern science as historically gendered and raced in ways that privilege a predominantly white, patriarchal, and eurocentric standpoint. The second is the development of an epistemological framework that allows for the recognition of other nonwhite, nonpatriarchal, and non-Western standpoints without retreating from a conception of objectivity or falling prey to a form of naive pluralism or relativism.

In effect, Harding's latest book, *Is Science Multicultural?* is a logical extension of her previous work in feminist epistemology with a few refinements. Harding's previous work might be understood as an attempt to mediate between the concerns of constructivist sociology and history of science on the one hand and more politicized feminist science studies on the other. This new book adds a third voice: that of postcolonial science and technology studies. The mere attempt to bring together these three often disparate trajectories of research on science marks this book as both unique and important to the development of a broader understanding of knowledge-making and science. *Is Science Multicultural?* initiates a conversation that should have taken place 10 years ago amongst science studies scholars; if there is a major drawback, it is that the conversation barely gets started before the book is over.

Harding's project is perhaps too grand to suit most social scientists, but her intention is to lay out the possibilities for a multicultural epistemology that might transform scientific practice, international science policies, and their effects. Modern science, for Harding, is not culturally transcendent but is bound to specific cultural histories and their relations. Harding's targets, therefore, are the dominant eurocentric and androcentric science and technology policies that are in part legitimated by a "positivist" and "internalist" epistemology, which has its origins in the development and expansion of European patriarchal culture. To counter these conceptions of science, Harding advocates adopting a postcolonial standpoint and looking at Western science from the outside. As most readers will be unfamiliar with the genre of postcolonial science studies, Harding's advocacy is valuable for this reason alone.

The first half of the book considers three kinds of postcolonial perspectives: studies of the relationship between European colonial expansion

and the emergence of modern science, studies of contemporary non-European (and American) cultures' scientific practices, and development studies that question the universal applicability of Western paradigms of scientific progress. Harding provides brief accounts of a range of historical and ethnographic studies written from a postcolonial perspective, which demonstrate the cross-cultural contingency of both early-modern and modern scientific practices. While the main issues are clearly laid out, Harding's discussions of these texts are all too brief and are seldom critical.

In the second half of the book, Harding proceeds to articulate the consequences of adding postcolonial voices to the development of feminist standpoint theory and her brand of strong objectivity. Particularly useful here is Harding's discussion of the possibilities for antiessentialist and nonrelativist "borderlands epistemologies." Although important in their own right, Harding's overt epistemological concerns cut short the conversation on the implications of postcolonial perspectives for science studies and vice versa. A more interesting and important task, perhaps, is articulating the ways that knowledge is made and deployed across cultural contexts. Postcolonial studies are important for understanding the relationship between non-Western epistemic practice and the globally dominant ways of knowledge-making we call science. While Harding takes an important epistemological and political stand in referring to all systematic attempts to produce knowledge about the world as sciences, this does little to help us understand how actors must regularly negotiate the boundaries of what counts as legitimate knowledge within and across cultures.

A final disappointment is that, despite Harding's advocacy of postcolonial scholarship and its epistemological implications, her text contains almost no discussion of extant work in postcolonial critical theory. Harding's arguments would benefit from the reflection on the problematic meanings of postcoloniality, the process of decolonialization, and neocolonialism by authors such as Franz Fanon, Edward Said, Gayatri Spivak, and members of the Subaltern Studies Group. To a certain degree, the same criticism applies to Harding's use of the term "science." It is often unclear when Harding wants to refer to science as a Western cultural institution, a set of local practices, a collection of beliefs about the world, or a particular epistemological framework that grounds those beliefs. All of these meanings are fine, but the lack of specificity in her discussion makes finding the points of convergence and divergence between postcolonial, feminist, and constructivist science studies a more difficult task than it needs to be.

The Myth of Continents: A Critique of Metageography. By Martin W. Lewis and Karen E. Wigen. Berkeley and Los Angeles: University of California Press, 1997. Pp. xv+344. \$55.00 (cloth); \$19.95 (paper).

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Metageography, the set of spatial structures we use to organize and comprehend the world, would seem to be a topic of immense interest at this juncture in history. It is surprising, though, that systematic inquiry has not kept pace with developments that threaten to demolish longstanding understandings of global geography. Martin Lewis and Karen Wigen see this oversight as one of the most serious flaws in contemporary geography and attempt to rectify it by identifying the dominant metageographical frameworks currently in use, the spatial distortions to which these give rise, and the ideological and political consequences that flow from reliance on an overly simplistic metageography. Their critique provides the basis for the building blocks needed to construct a more sophisticated global geography.

With the collapse of the Soviet Union, our view of a world composed of relatively stable units has been revealed for the fiction that it is. Boundaries that once seemed natural and immutable are now contested and ephemeral. Deeply rooted ethnic and religious conflicts have erupted in regions that were once largely ignored or treated as if they formed homogeneous cultural units. Despite these and other changes, we continue to describe the world with such static metageographic categories as continents, East/West, First/Second/Third World, and Europe/Asia.

According to Lewis and Wigen, the bulk of our misunderstanding can be traced to an uncritical acceptance of "a series of convenient but stultifying geographical myths, based on unwarranted simplifications of global spatial patterns" (p. xiii). These include the myth of continents, the myth of East and West, the myth of the nation state, and the myth of geographic concordance. The myth of continents and the myth of East and West are singled out for detailed treatment, while the myth of the nation state and the myth of geographical concordance occupy supporting roles, reinforcing the prejudices and stereotypes that grow from the central myths.

The myth of continents is by far the most basic and damaging metageographic category. Continents, as generally understood, are large, continuous expanses of land separated by bodies of water. The most obvious problem with this scheme is that the required size and degree of physical separation have never been defined. Over time, different criteria have been used to draw continental lines in different places, depending on the interests of who was doing the drawing. Thus, Europe, which is most accurately viewed as part of single continent that includes Asia, is nevertheless elevated to continental status. In doing so, it assumes a level of cultural and political importance that is not warranted by size alone. This

is not simply a matter of convenience or the result of a simple oversight. Instead, it has been a key element in the effort to establish and maintain a cultural dichotomy between Europe and Asia, "a dichotomy that was essential to modern Europe's identity as a civilization" (p. 36).

Like the continental scheme we have inherited, the spatial division between East and West is largely arbitrary. And it too has been subject to shifts over time. During World War II, for instance, some historians classified Germany as an eastern country in order to distance Nazism from "true" European civilization. With the onset of the cold war, the boundaries shifted again; West Germany became part of the West, while everything behind the iron curtain was East. In this incarnation, the West was synonymous with capitalist democracy and the East with communism. Today, the West has come to mean the developed world, without reference to the actual location of particular countries. With this redefinition, Japan becomes a western nation while the countries of Latin America simply become part of the Third World.

The spurious geographical division between East and West is compounded by unexamined assumptions about the cultural differences between these regions. While Lewis and Wigen catalog a host of attributes that supposedly distinguish Western and Eastern Culture, they argue that rationality has long been the most important. In this view, Western progress was made possible by a commitment to a "peculiarly Greek spirit of rationality" (p. 83). The implication here is that this force was missing in the East. However, if one defines rationality in broad terms, the West can hardly be said to have a lock on reason. At times, in fact, China and parts of the Islamic world have in many ways been more committed to rational inquiry than Europe.

The misunderstandings that grow out of the myth of continents and the East-West myth are exacerbated by a persistent environmental determinism that continues to assert a causal relationship between the physical environment and cultural and social traits. This is seen most clearly in the notion that Europe's developmental trajectory was largely dependent on a temperate climate: "Europe's physiographic and climatic diversity are now sometimes viewed merely as having prevented the consolidation of large empires and allowed scope for the development of a market-driven economy" (p. 44).

While it is obvious that our uncritical use of metageographic categories serves to highlight the achievements of European civilization, it also has a more pernicious consequence. As Lewis and Wigen put it, "Our flawed metageography has become a vehicle for displacing the sins of Western Civilization onto an intrusive non-European Other in our midst" (p. 68). By redrawing the physical and cultural lines between East and West when the need arises, the blame for all manner of atrocities can be shifted and historical responsibility denied.

This last charge is open to serious debate. For while some scholars may indeed have used stereotypes of the East to shift blame for Western sins, Lewis and Wigen ignore the work of Horkheimer, Adorno, and other

members of the Frankfurt School who trace some of the most despicable acts of the 20th century to contradictions embedded within the Enlightenment. Despite this omission, Lewis and Wigen's broader argument is a sound one. Overly simplistic metageographic categories are a barrier to understanding an increasingly complex world, and our reliance on them *does* lead to generalizations with often damaging political consequences.

Replacing the existing framework, however, creates new problems. For all the deficiencies inherent in the systems currently in use, they do provide a shared language for talking about the world. The move to "an open-ended melange of overlapping and incommensurable distributional patterns" (p. 13) will upset common understandings and threaten our ability to communicate effectively. Hence, some form of taxonomy is essential. The world region framework is the scheme that comes closest to meeting these criteria. Lewis and Wigen propose that we take this as a starting point for developing a more subtle map of the world. In addition to the 10 regions that are typically delineated, they suggest that we add three new ones: African-America, Melanesia, and Central Asia. Their world map is also explicitly rooted in historical processes, ignores political and ecological boundaries in favor of meaningful cultural areas, and conceptualizes regions both in terms of internal characteristics and their relations with one another. This rendition provides a more nuanced view of sociospatial relations, and while useful to scholars and policymakers, it does little to correct popular stereotypes and prejudices that are inseparable from the myths of metageography. Dislodging these will take more than a new set of lines on the map.

The Struggle over the Soul of Economics: Institutional and Neoclassical Economists in America between the Wars. By Yuval P. Yonay. Princeton, N.J.: Princeton University Press, 1998. Pp. xiii+290.

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This luridly titled book is a fine example of the sociology of science applied to the history of economics, in particular, the interwar period in American economics that consisted in large part of a struggle for hegemony between institutionalists of the Veblen-Commons-Mitchell variety and orthodox neoclassical economists. It utilizes the actor-network approach (ANA) associated with the Paris School of Bruno Latour and Michel Callon that views science as a social network of individual scientists who negotiate with one another in an endless struggle for intellectual dominance, the outcome of which is never decisively influenced by either internal or external forces but rather by an unpredictable combination of both cognitive and sociological elements. The appeal to the names of Latour and Callon, not to mention Barry Barnes, Harry Collins, Karin Knorr-Cetina, and Trevor Pinch, suggests that we are going to be shown

how economists in such interwar economic "laboratories" as the National Bureau of Economic Research and the University of Wisconsin Department of Economics actually conducted their research. But this is not Professor Yonay's tack: instead, he confines himself to the written word, a limitation that he frankly acknowledges. Nevertheless, the thesis that he establishes by an examination of the primary literature is that institutional economics was in ascendancy throughout the 1920s and maintained its grip on the economics profession even in the 1930s and 1940s, only to be vanquished by neoclassical economics in the late 1940s. Moreover, this brand of "neoclassical economics" (a label that was itself invented by Veblen as early as 1903) was nothing like what we now call neoclassical economics. Somewhere around 1950, economics went through a veritable revolution of "mathematization," with names like John Hicks, Paul Samuelson, Kenneth Arrow, and Gerard Debreu becoming the idols of a new generation of postwar economists. From that moment on, in the customary manner in which history is always being rewritten by the winners, the story of interwar American economics was reinterpreted as one of an inevitable victory of orthodox "neoclassical economics" and the equally inevitable demise of institutionalism. In consequence, it is now difficult to recognize the writings of some of the leading mainstream interwar economists such as Frank Knight and Jacob Viner as orthodox at all: they seem at times to be highly critical of assumptions regarded nowadays as absolutely sacrosanct.

This may not be as novel an interpretation as the author claims (e.g., see my own "Disturbing Currents in Modern Economics," *Challenge* [May-June 1998]) but nevertheless this is the first detailed demonstration of the argument and hence may convince even the most skeptical of textbook writers in the history of economics. As a bonus, we are given an attractive account of the new school of ANA as a species of the genus of "constructivism" in the philosophy of science, of which other closely related examples are the Bloor-Barnes "strong program" in the sociology of science and the McCloskey-Klamer program in the "rhetoric of economics" (chaps. 1, 10). Until the very last pages of the book, ANA is sold to the reader as the only successful explanation of the evolution of interwar American economics, superseding Thomas Kuhn's appeals to paradigms or Imre Lakatos's notion of scientific research programs. But in the final pages (pp. 218-22), it is suddenly conceded that the methodology of any social science such as economics also has a normative objective, namely, to appraise developments in the subject with a view to improving the quality of practice and even to criticize the drift of current developments. Professor Yonay argues that construction is not indifferent to normative issues and does not necessarily condone whatever is standard fare in an area of scientific inquiry. I was not entirely persuaded by this all too brief defense. It does seem to me that ANA, like all varieties of constructivism, is bound to conclude, not only that anything goes but that everything goes. As for causally explaining the past, ANA likewise suffers from excessive generality. What could possibly contradict an ANA expla-

nation of a historical episode when in fact every conceivable element in scientific disputes is recognized and is indeed recognized as having equal value? An ANA explanation of a past event strikes me as very much like playing tennis with the net down.

Nevertheless, I can think of no historian of economics who would not learn a great deal from this book; needless to say, it is vain to imagine any workaday economist reading this book since they (a) never read *books*, and (b) never read books on *intellectual history*. Is this an ANA argument?

La découverte du social: Naissance de la sociologie en France (1870–1914). By Laurent Mucchielli. Paris: Editions la Découverte, 1998. Pp. 572.

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Reference books usually have the limited yet practical function of providing information on a particular topic. Laurent Mucchielli's work is more than a simple reference book; it deserves to be read in its entirety. Defending an interesting and important general perspective, Mucchielli proposes to situate the institutional genesis of French sociology in its social, cultural, intellectual, and political context. Using a sociology of knowledge approach to identify the multitude of influences at the origin of the main French sociological theories that emerged between 1870 and 1914, Mucchielli shows that "the discovery of the social" was the result of a major collective effort.

In the first part of the book, Mucchielli argues that at the end of the 19th century the previously dominant biological and racial models were replaced with an understanding of sociology as an objective science. He analyzes in minute detail the works of sociologists of that period, their intellectual networks, and their main theoretical concepts. Mucchielli goes on to demonstrate how Émile Durkheim and his collaborators represent a new "sociological paradigm," in the Kuhnian sense of the term. The theoretical projects of Durkheim's key opponents are examined, including Gabriel Tarde's theory of imitation and René Worms's organicism. The strength of Mucchielli's work lies in the second part. He shows how the Durkheimian school, which gravitated around the journal *L'Année Sociologique*, tried to impose its project on the other social and human sciences of that period (i.e., criminology, psychology, biology, geography, history, linguistics, ethnology, political economy), while at the same time being largely inspired by them. The contribution of Durkheim's major collaborators (Bouglé, Halbwachs, Hubert, Mauss, Meillet, Richard, and Simiand) in the inauguration of a fertile dialogue with these competing sciences is brilliantly analyzed.

This book is a very valuable work for many reasons. First, the subject

itself is highly original from the perspective of the history of ideas: Mucchielli focuses on the birth of French sociological thought in this decisive period of its history when the fundamental questions, which are still with us, were first addressed (i.e., the links between theory and empirical research). Some authors analyzed by Mucchielli, against whom the Durkheimian paradigm raised its voice, had never previously been seriously taken into account, despite the fact that they played an important role in the emergence of sociology in France. Second, the interest of the interpretative framework of the book must be underlined. This framework, which rests on an impressive erudition, is conceived in such a way that the main thread of Mucchielli's argument is never broken between the general project and the particular constructions, that is, between the analysis of central theoretical concepts and the effort to reconstruct an intellectual horizon on the one hand and a series of portraits of authors on the other. Beyond individual sociological contributions, it is the ideas themselves, their confrontation with each other, and their broader intellectual significance that are at the center of Mucchielli's investigation. Third, while Durkheim's work has been abundantly discussed, the contributions of his disciples have been less well studied, with the exception of his nephew Marcel Mauss, who has been the object of a few scholarly books in recent years (Marcel Fournier, *Marcel Mauss* [Fayard, 1994]; *Marcel Mauss, écrits politiques* [Fayard, 1997]). Mucchielli's most remarkable contribution is his presentation of a generous sample of works from main contributors to *L'Année Sociologique*. Some of the Durkheimians, Célestin Bouglé and François Simiand in particular, have played a central role in the elaboration and diffusion of the Durkheimian paradigm, but their names often remain unknown outside French borders. Mucchielli reminds us what Philippe Besnard's works have already shown ("La formation de l'équipe de *L'Année sociologique*," *Revue française de sociologie* [1979] 20:7–31; *The Sociological Domain* [Cambridge University Press, 1983]): the emergence of the Durkheimian paradigm is the result of a fruitful teamwork of young scholars from different disciplines. Finally, Mucchielli's bibliography is impressive, containing an important compilation of both primary and secondary sources.

There is no doubt that this well-written reference book will be of interest not only to both undergraduate and graduate students but also to professional sociologists and other social scientists who want to learn more about the birth of French sociology. Its merit resides in the fact that it accurately presents an intellectual landscape about which very little is known in North America and, to some extent, even in France. In sum, Laurent Mucchielli succeeds in meeting the challenge he had set for himself: his book will be useful to many, and one can already predict that it will become an authoritative source of knowledge on a crucial period in the history of sociology.

Culture and Power: The Sociology of Pierre Bourdieu. By David Swartz. Chicago: University of Chicago Press, 1998. Pp. viii+333. \$57.00 (cloth); \$15.95 (paper).

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Of all contemporary European sociological theorists, only Pierre Bourdieu has yet to find his definitive American interpreter. Partly because of the fragmented nature of our disciplinary field, Bourdieu's complex corpus has been appropriated largely in bits by specialists who are unaware of or unconcerned about the integral whole. So in a manner reminiscent of the Indian folk tale of the blind men and the elephant, there are many American Bourdieus: an anthropologist of Algeria, a sociologist of education, an analyst of art and culture, a researcher of stratification. Bourdieu has contributed to this fragmentary appropriation by adamantly refusing to "theorize," that is, present his general concepts and ideas in a form abstracted from his empirical research.

This faulty American reception of Bourdieu's sociology will hopefully change with the publication of David Swartz's new book, which is the first successful and accessible overview of Bourdieu's entire corpus. *Culture and Power* puts all the parts of the elephant back together, revealing not merely the power and breadth of the animal but the warts and wrinkles as well. Swartz is in the right position to accomplish this sympathetic but probing treatment. As a student at the Sorbonne in the 1970s, he attended Bourdieu's seminars but did not become a disciple. Familiar with the Parisian intellectual field, yet viewing it from the remove of the American university, Swartz achieves a felicitous balance between insider and outsider. The result is a book unsurpassed in the breadth and depth of its comprehension of this major sociological theorist. But readers looking for a mere introduction to Bourdieu's work had best look elsewhere, for this is a highly sophisticated work that presents a wealth of details, complexities, and nuances.

Before diving into his exposition of Bourdieu's basic concepts, Swartz gives us an enlightening chapter on his career and position in France's intellectual field. Bourdieu's relentless criticism of the role of culture in general and education in particular in reproducing social inequalities is explained by his own origins as a petit-bourgeois outsider who, despite his remarkable upward mobility, has always felt marginalized by the upper-class culture of France's *grandes écoles*. Bourdieu's concepts and research are insightfully interpreted as strategic moves against competitors in this complex and contentious Parisian intellectual field.

Swartz then moves on to explicate the general concepts that inform all of Bourdieu's research. His utmost objective is to extend the notion of self-interested action usually associated with economics to cultural practice. For Bourdieu, all social action is motivated by the pursuit of profits

and power, but the seeming disinterestedness of cultural pursuits allows them to "launder" and legitimate the structured inequalities of other areas. He argues that in modern societies cultural practices have become the most important method of reproducing inequalities, as the upper classes invest money and time in education and art, which become the basis for social selection. Bourdieu's theory departs, however, from rational choice and conflict models of action in holding that the pursuit of gain is not conscious but is governed by a set of preconscious dispositions—a "habitus"—inculcated by early socialization in the class structure. The effects of habitus on action are, however, always mediated by the particular "field of struggle," one of Bourdieu's more recent and less understood concepts, which Swartz does a good job of clarifying.

Swartz then devotes several chapters to detailing how Bourdieu mobilizes these concepts in empirical studies of social class, education and art, and intellectuals. Researchers in these fields will find a wealth of intriguing material here, for the author is in thorough command of the French literature by and on Bourdieu. Unfortunately, however, he only rarely engages the American literature in these areas, which often reveals the limited generalizability of Bourdieu's French findings.

The last several chapters explore Bourdieu's conceptions of science and reflexive sociology. His insight that science too is a self-interested practice in a field of competition leaves Bourdieu struggling to maintain faith in the objectivity and emancipatory potential of science. But ever the defender of an autonomous and scientific sociology, he argues that sociologists can partially transcend the limits of their own interests by reflexively analyzing their own scientific field. As Swartz insightfully notes, however, Bourdieu's professional optimism about the objectivity and progressive political potential of sociology contradicts his pessimistic theory that all culture, however autonomous, is inevitably interested and reproduces inequalities.

Swartz's book is a welcome and indispensable contribution to understanding Bourdieu's sociology, but it is not without flaws. In his attempt to be comprehensive, the author's expositions often end up exhaustive and repetitive. There are also too many footnotes, some of which are interesting textual supplements, while more are merely annoying interruptions. Finally, I was surprised to find in a book published by the venerable University of Chicago Press a generous sprinkling of typographical errors in the text and numerous errors and omissions in the references. Readers deserve better.

Simmel et la modernité. By Lilyane Deroche-Gurcel. Paris: Presses Universitaires de France, 1997. Pp. x+336.

Suzanne Vromen
Bard College

This significant addition to our knowledge of Simmel is remarkable by the breadth of its scholarship and the depth of its interpretations. In tracing the many ways in which Simmel is an exponent of modernity, the author argues for the overall coherence of Simmel's thought and shows the close relationship between his sociological, philosophical, and aesthetic writings both in concepts and essential reasoning. Simmel's aesthetic contributions have received relatively little attention. *Simmel et la modernité* remedies this situation.

As sociology's search for global and definitive explanations has waned and holistic systems have been replaced by critiques, the author argues, Simmel's popularity has grown. By founding social life on interaction, by stressing process and the flux of becoming (in contrast to being), and by overcoming the limits of disciplinary boundaries, Simmel shapes a unitary and modern vision of culture and society.

The author contends that Simmel is not a positivist, because he rejects the belief in an immanent, unique truth defined by its contents, nor is he a postmodern relativist, because he refuses to assume that truth is an illusion and that all opinions are equally valid. His theory of knowledge is founded on a nonskeptical relativism. In this epistemological rupture, truth is not content but a relation, a reciprocity of action. Relativity is the essence of truth, thus an isolated or absolute truth does not exist, only a relational one. For example, a line considered in isolation is neither short nor long. Truth and reality have to be separated. It is precisely on this point that Simmel criticizes naturalism, synonymous for him with realism. Art and the social sciences have the ability to create a world instead of merely copying one. Just as truth is not the duplication of reality, the art work does not copy the real.

Reciprocal action explains the dynamics of social life, but it is also central to the critique of modernity embedded in his sociology of art. Simmel criticizes two unsatisfactory artistic conceptions of modernity. The first pursues only purely formal intentions; form clearly predominates over content. The other, expressionism for example, gives priority to the effusive subjectivity of the artist and claims authenticity by sacrificing form. For Simmel, both conceptions lead into an impasse, for both miss the expression of totality. The real modern artist seeks the totality of life in which both beauty and ugliness, perfection and imperfection are included. Pure technical virtuosity cannot achieve excellence. On the contrary, Simmel praises art for life, thus affirming the interdependence of form and content.

In the works of Rodin and Rembrandt, Simmel finds the most accomplished expressions of modernity, for, different as they are, they both rep-

resent the unity of soul and body, the totality imperative for Simmel. In Rodin's sculpture, constant movement yields a new way in which surfaces meet and confront each other. Rembrandt is modern in expressing the immediate individuality of his subjects through form and color without concern for classical beauty. Opposition and contradiction together with individuality and particularity characterize modern art, and Rembrandt is for Simmel the first painter to have realized this. Academic painting only establishes types. But Rembrandt frees himself from the general semiotic code that imprisons the academic painter and expresses individuality as a pictorial problem, not as a psychological or metaphysical or anecdotal one. A quote from Simmel's book on Rembrandt cited by the author sums it up well: "this knowledge of life that speaks through creations and not concepts" (p. 84; my translation).

The author contends that Simmel refutes the pessimism that modernity inspires in his contemporaries. It is ambivalence that marks modernity, therefore, a possible disenchantment is never fatal. If the blasé displays melancholy, this attitude reveals itself paradoxically to be a technique of adaptation.

To highlight Simmel's modernity, the author contrasts him with Durkheim. Durkheim is roughly characterized as an interventionist sociologist, deterministic, with a voluntaristic reformatory thrust, a moralizer emphasizing permanence and regulation. In contrast, Simmel stresses agency and the creative power of the individual, with the blasé attitude an effective adaptive behavior and aiming at lucid understanding but no societal prescription.

A short review cannot do justice to all the facets of this work nor to the incisive connections that the author makes with major literary figures and art critics. Various secondary sources are used, yet two prominent Simmel scholars, namely Donald Levine and Birgitta Nedelmann, are overlooked. The author also neglects to discuss Simmel's thoughts on the place of women in the modern world, a subject embedded in his theoretical scheme and one that he treats seriously in contrast to his contemporary peers. Further, is it really necessary to juxtapose Durkheim and Simmel, or to put it differently, does praising Simmel demand damning Durkheim? Finally, I would have liked to learn more about the reception of Simmel's aesthetic works in his time.

The book is unfortunately written in a turgid and redundant style. It will, however, be cherished by Simmel scholars for its insights and broad range, and it deserves to be rapidly and coherently translated. Simmel should be satisfied; justice has been done to his thought.

Re-forming the Body: Religion, Community, and Modernity. By Philip A. Mellor and Chris Shilling. Thousand Oaks, Calif.: Sage, 1997. Pp. 234. \$75.00 (cloth); \$26.95 (paper).

Ann W. Ramsey
University of Texas, Austin

Mellor and Schilling contribute to a growing sociological literature on "embodiment" as the matrix for both collective experience and the sense of self. Their nonteleological model uses ideal-types ("the medieval body," "the Protestant modern body," and "the baroque modern body") to examine the persistence of the sacred in Western development.

The argument proceeds on three fronts. First, the authors aim to broaden historical perspective in current debates about the postmodern condition by showing how the senses and the human body have been constantly restructured over time. They focus on the character of these changes rather than the complexities of defining the agents of these changes. Second, they argue that debate over the future of Western civilization must consider the resilience of the human body and its resistance to "cognitive control." They warn that this resistance may be liberating or dark and violent. Third, by defining their work as an expansion upon Emile Durkheim's interest in the relationships between forms of embodiment, forms of sociality, and forms of knowing, the authors join the ongoing debate about the legacy of Durkheim himself.

They draw inspiration from the analysis of the "sacred 'fires' of effervescent sociality," in Durkheim's *Elementary Forms of Religious Life*. Lloyd E. Sandeland's work on embodiment ("The Body Social," in *The Mark of the Social*, edited by John D. Greenwood [Rowman & Littlefield, 1997]) critiques the primacy of cognition in Durkheim's approach to social life. Mellor and Schilling, however, pursue a different project of cultural criticism and historical analysis emphasizing Durkheim's commitments to *homo duplex*. The authors stress "the enduring significance of sacred forms of sociality" (p. 201). Their critique of the human costs of the "rationalist Enlightenment project" (p. 22) finally depicts mind/body dualism as an inadequate ontology of the human body.

The argument begins in affinity with Max Weber and presents the disciplined and cognitively controlled early modern Protestant body as the touchstone of Western modernity. The authors have clearly, however, been paying attention to dramatic shifts in historians' understanding of the Reformation era and the religious culture of medieval Catholicism. The present generation of cultural and religious historians have shifted their attention from doctrinal analysis in order to explore peoples' experience of the sacred and the changing attitudes toward the human body that shaped religious values, authority relations, the sense of the self, and the fundamental ways in which one knows the world.

The authors are right to warn the reader not to look here for "empiricist history" (p. 32). As a historian, I missed the lack of direct analysis of the

historical material of the past. All their historical material on the Middle Ages and Reformation era is drawn from monographic and even textbook interpretations (for the latter chiefly Euan Cameron, *The European Reformation* [Oxford University Press, 1991]). But, the authors have chosen well: Peter Brown, Caroline Walker Bynum, John Bossy, and Robert Scribner, among others, have led a veritable revolution in historians' understanding of the place of the body in the changing religious culture of Christianity.

One may criticize Mellor and Schillings's generalizations about the "superstitious" character of premodern religious belief or their lack of social differentiation in treating medieval epistemologies of the sacred. More important, however, this book enriches the conceptual arsenal for interdisciplinary analysis of political, social, and cultural change. Their work particularly stimulates more nuanced thinking about the cultural and political legacy of the Reformation era. By drawing attention to fundamental differences in the way Catholic and Protestant reform movements of the 16th and 17th centuries reshaped the senses and bodily experience, the authors in effect emphasize an often overlooked diversity in the Western cultural inheritance. This forces rethinking of oversimplified schemas of historical development that tended to focus on "Protestant" paths to modernity. Their methodology of ideal types, if read as incitement to further research, manages both to clarify tensions surrounding cultural and social integration in the late 20th century while underscoring the real historical complexity of modern bodies.

This is most evident in their treatment of the importance of the Catholic Reformation and its culmination in the culture of the baroque, which opposed the asceticism of Calvinism with complex strands of sensuality and a more physically grounded asceticism. The interpretation of the "baroque modern body" continues this theme. In tracing connections between baroque sensuality and contemporary pursuits of the "hard" athletic body, the authors draw upon an important, relatively new historiography on late-medieval and early modern Catholic religious experience and its connections to the postmodern temperament. Here one misses acknowledgment of the work of Michel de Certeau (*The Mystic Fable* [University of Chicago Press, 1992]) or Edith Wyschogrod's *Saints and Postmodernism* (University of Chicago Press, 1990). But, there is much more to be grateful for than can be mentioned here, especially in the political vein in critique of the "contractarian" mentality and in the conceptual distinctions stressed in the notes. The book and its bibliography will be a welcome overview in courses on the body and the sensoria. Readers in all disciplines who are interested in pursuing the important insight that the construction of reality is an embodied process will find this provocative reading.

Social Mechanisms: An Analytical Approach to Social Theory. Edited by Peter Hedström and Richard Swedberg. New York: Cambridge University Press, 1998. Pp. x+340. \$59.95 (cloth); \$22.95 (paper).

James Johnson
University of Rochester

The contributors to this volume are dissatisfied with the current state of social and political research. They claim that much of what now passes for empirical social science and much of what now passes for social theory is deficient because those who engage in either enterprise—one symptom of deficiency is that hardly anyone engages in both—neglect the task of identifying the causal mechanisms that explain phenomena in the social world. Many practicing social scientists and social theorists will be tempted to dismiss such criticism as a distraction from their ongoing research. They should resist the temptation. They should resist, most obviously, because the contributors to the volume form a distinguished group of sociologists, economists, and political scientists. I am not invoking authority in a superficial way here. Instead, I simply suggest that we ought to listen to colleagues whose own research—some quite recently, others over the course of several decades—sets standards that are very difficult to match. In the end, this initial presumption is born out. The message that the contributors convey is, taken as a whole, very persuasive.

Although they sometimes are repetitive and do not always agree on all details, the contributors to this volume would agree that social mechanisms enable social scientists to identify causal agency. In this sense, a mechanism m is a component of some more encompassing theory T , where m typically operates at a level below T and makes T more credible in the sense that m renders the explanations that T generates more fine-grained. So, for example, when we elaborate a theory to account for some social practice or change, we may invoke a mechanism (such as risk aversion, dissonance reduction, or utility maximization) that, while it operates at the individual level, can, once we properly specify the relevant aggregation processes, help us explain the particular features of that practice or change. The contributors all elaborate on this general idea.

The largest set of papers develops, in a more or less abstract manner, conceptions of social mechanism and explores what such conceptions entail. This group includes, besides an insightful introduction by the editors, essays by Raymond Boudon, Tyler Cowen, Jon Elster, Diego Gambetta, Gudmund Hernes, and Thomas Schelling. These papers explore a variety of theoretical issues. They explain why the search for mechanisms does not presuppose that we can identify general laws and why, consequently, while they are crucial to explanation, most mechanisms afford unreliable bases for prediction. They impress upon us the difficulties that the search for mechanisms involves because, for instance, mechanisms typically are not directly observable and because they often interact in complex, dy-

namic ways. They also stress that the search for mechanisms pushes social research toward a sort of modest abstraction that relies on the formulation of models to represent crucial features of the social or political phenomena we wish to explain. And they stress that the search for mechanisms directs theoretical attention to the forms of interdependent human agency that sustains most aggregate social practices and institutions. As this too schematic account suggests, the argument of the book is in certain respects old-fashioned. The authors generally hope to resuscitate a mode of social research exemplified by a lineage whose most exemplary figures are Alexis de Tocqueville, Max Weber, Robert Merton, and James Coleman.

The importance of this enterprise becomes clear when we turn to a second set, or rather, pair of papers. These papers canvass existing work in sociology and persuasively detail the baleful consequences that emerge when we fail to identify the mechanisms that animate our inquiries. Aage Sørensen chastises "empirical" social scientists for mistaking statistical for theoretical significance. He argues that social research often is naively and unduly preoccupied with statistical methods that, he rightly points out, have "no social theory whatsoever." As a result, social research focuses almost entirely on describing effects (accounting for the "variation" of some factor or other) and so neglects to specify in a theoretically credible way the causal processes that generate and so *explain* those effects. Axel van den Berg, by contrast, works from the opposite direction. He probes the sort of grand theory to which most contemporary social theorists aspire. He focuses on the writings Jürgen Habermas, Pierre Bourdieu, Jeffrey Alexander, and Anthony Giddens, searching, largely in vain, for any explicit, systematic discussion of mechanisms that might allow social theorists to account for observed features of the social or political world. In combination, then, these two papers make clear how inattention to mechanisms has not only impoverished both empirical social research and social theory but made efforts to reduce the distance between the two enterprises especially difficult.

A final group of papers illustrates the power and generality of specific mechanisms. This group includes papers by Peter Hedström on rational imitation, by Timur Kuran on varieties of dissonance reduction, and by Arthur Stinchcombe on monopolistic competition. Each author induces us to see how particular causal mechanisms operate in distinctive, if somewhat different, ways across a range of empirical settings. These more constructive analyses nicely complement the theoretical and critical offerings described above. Taken together, the contributors advance a sophisticated and penetrating agenda for social research. Readers surely will disagree with various parts of the argument that this volume advances. I am confident, however, that research in social science and social theory can only be improved by confronting the challenge that the contributors lay down.

Rational Choice Theory and Large Scale Data Analysis. Edited by Hans-Peter Blossfeld and Gerald Prein. Boulder, Colo.: Westview Press, 1998. Pp. xiv+322. \$60.00.

Peter Abell
London School of Economics

Why are sociologists moved to contribute to a book linking rational choice theory (RCT) and large-scale data analysis? Economists, for instance, would not do so; they would take the connection for granted and view much of the content of the book under review as self-evident. It is for them standard practice to test theoretical models, derived from rational choice precepts, against large data sets. Things are different in sociology; first, RCT has only a tenuous hold on theory, and, second, most research that draws on large data sets is notable for an absence of systematic theory (chap. 2). Why is this so? Take the second difference first. As a number of the contributing authors note, social theory is a failed intellectual tradition and has neither had a significant impact upon empirical research nor achieved any depth of its own. Thus, the barely concealed agenda behind the present volume is not merely one of locating a symbiosis between RCT and large-scale statistical analysis but of promoting RCT as *the* theoretical framework where others have failed. The canvas is correspondingly large.

While these objectives are, in my view, perfectly laudable, I am afraid some of the essays are not always quite up to the mark. Many show signs of having been hastily written, and the English language editing of others is sometimes far from perfect. Uninitiated and skeptical readers will often find it difficult to follow the argument and even, in places, to understand what is going on. This is a shame because, with a little more care, the book, I believe, could have proven a landmark. As it is, it will necessarily have to fight an uphill battle.

Based upon chapters—some by well-established rational choice theorists and some by statistical modelers—and linked commentaries, the contributions are varied, running from the slightly technical (Stanley Lieberman) to the more philosophical (Hartmut Esser and Undo Kelle and Christian Lüdermann). There are also a number of chapters based upon empirical research. The editors provide a most lucid introduction, and Michael Hechter concludes with observations that future research in sociology is likely to be a team effort. Furthermore, since rational choice is the best general theory we have, until something better turns up, we best stick with it.

Despite the book's title, one of the most arresting sections is given to a skirmish between Stanley Lieberman, promoting a well-argued skepticism about small-*N* studies and advocacy of probabilistic causality, and Charles Ragin, advancing his own version of the Boolean analysis of small-*N* case study research. Although one can find no intellectual closure

here, Lieberman's insistence that we should draw a sharp distinction between the causal factors that shape a distribution and those that allocate individuals within the selfsame distribution is surely central to the RCT treatment of large data sets. Nonetheless, the distinction is rarely made by empirical researchers. Rational choice theorists, however, usually work with the grain of this distinction in requiring (at least as an opening gambit) that preferences and opportunities are independently determined. What I found missing here though, and throughout the essays, was any treatment of the strategic interaction of individuals in determining opportunities. Clearly, if individuals are strategically linked, then the standard assumption underlying most large-scale data set research, whereby the units of analysis (individuals) are drawn independently, must be called into question. Surely, both endogenous and exogenous autocorrelation must in practice be rife. I find little recognition of this in the research literature or in the essays in this volume.

Part two of the book covers the central issue, linking RCT and quantitative sociology. Predictably enjoyable essays by John Goldthorpe and Siegwert Lindenberg both find a compelling complementary between the two traditions, though in different ways. Goldthorpe, as one might expect, implores us to start with well-established empirical generalizations and then to search for RCT explanations for them. (Peter Hedström and Richard Swedberg agree—calling the explanations mechanisms.) Lindenberg, however, finds the connection less seamless; he wants to protect us from simple “economic imperialism,” feeling that the unadulterated combination of statistical analysis and RCT will not work without a richer model of the individual (and her constraints) than RCT usually provides. He is not, however, happy with the standard procedure of multiplying the arguments in any postulated utility functions. If we are to do this, then “feeder theories” are required telling us how, why, and when. These will inevitably transcend the confines of any straight-laced RCT. Lindenberg, thus, raises an issue taken up in different ways by many of the authors (notably Hans-Peter Blossfield and Harmut Esser), namely as to the role of both generalization and historical specifics in sociological explanation. Blossfield and Prein, in separate essays, begin to formulate the problems of using longitudinal models in this respect, both of which begin to push empirical models toward a more adequate approach to causality than we usually find in cross-sectional studies.

A number of chapters deal with “bridge assumptions,” that is, with the links between macro and micro (both ways), and there are three chapters devoted to empirical research: Anthony Heath on voting, Wout Ultee on the cohesion of Dutch society, and Karl Dieter-Opp on political mobilization. I would recommend that the skeptical reader start with these, then move on to Goldthorpe's and Lindenberg's essays before embarking on the rest.

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